Einstein Studies
16

Alexander S. Blum
Roberto Lalli
Jürgen Renn
Editors

The Renaissance of General Relativity in Context

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Volume 16

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Preface

This volume is the result of a project of Department I of the Max Planck Institute for the History of Science which started in 2014 in preparation of the centenary of Einstein's theory of general relativity. Within this context, an international working group was established with the goal to discuss research on general relativity after World War II and deepen our knowledge of the process dubbed the "renaissance of general relativity." These scholarly activities resulted in a number of meetings, papers, conferences, special issues of international journals, and books. A major event was the conference "A Century of General Relativity" co-organized by the editors in cooperation with the Albert Einstein Institute in Potsdam. The conference, held from November 30 to December 5, 2015, at the Harnack Haus in Berlin, was divided into two parts: one part dedicated to physics, and one dedicated to the history of general relativity, with 1 day in between that brought both fields together. The editors organized the historical part of the conference titled "The 'Renaissance' of General Relativity in History: Assessing Einstein's Legacy in Post-World War II Physics," during which many of the authors of this volume presented their preliminary research results. On that occasion, we benefited enormously from discussions with the many physicists who participated in the historical sessions. Another important meeting was the conference "Space-Time Theories: Historical and Philosophical Contexts," held at the Van Leer Jerusalem Institute from January 5 (Alexander Blum's birthday) to January 8 (Roberto Lalli's birthday), 2015, with much occasion for celebration. We contributed to the organization of the conference and presented our first studies on the topic, benefitting from comments of the participants, which included philosophers, physicists, and historians. Meetings on these topics were also organized as sessions of the History of Science Society meeting (2015) and of the European Society for the History of Science conference (2016), as well as various smaller workshops involving the working group members. A further meeting was held at Caltech in 2016, the 6th Biennial Bacon Conference on "General Relativity at One Hundred," the results of which are presented in a volume published with Princeton University Press in 2020.

While we and other members of the working group have already published parts of our findings, we consider this book to be the conclusion of our intellectual path toward understanding the “renaissance of general relativity.” We hope that all those who have been on this path see how much their contributions have influenced and shaped our work. Our heartfelt gratitude goes to all of them. We are especially indebted to the participants of the above-mentioned conferences and meetings for the insights they provided. It would be impossible to mention all the people whom we have interacted with along the way. We will give here only a partial list of the many people who have been involved in the project. We are deeply grateful to the members of the working group who are not authors in this volume: Markus Aspelmeyer, Jean Eisenstaedt, Domenico Giulini, Hubert Goenner, Dennis Lehmkuhl, Christoph Lehner, Jim Ritter, David Rowe, Tilman Sauer, Matthias Schemmel, Robert Schrader†, Kurt Sundermeyer, and Jeroen van Dongen. Their involvement, insights, and research projects were fundamental to the success of the initiative. We are also extremely grateful to many scholars and collaborators who attended the conferences as speakers and provided a number of important insights in our projects: Abhay Ashtekar, Yemima Ben-Menahem, Alessandra Buonanno, Yvonne Choquet-Bruhat, Jed Z. Buchwald, Leo Corry, Thibaut Damour, Reinhard Genzel, Joshua Goldberg, David Gross, Hanoch Gutfreund, Michel Janssen, David Kaiser, Daniel Kennefick, Diana Kormos-Buchwald, Helge Kragh, Gabriel Motzkin, Hermann Nicolai, Cormac O’ Raifeartaigh, Carlo Rovelli, Remo Ruffini, Thomas A. Ryckman, Jim Peebles, Roger Penrose, Robert Schulmann, Bernard Schutz, Chris Smeenk, Lee Smolin, John Stachel, Kip S. Thorne, Rai Weiss, Aaron Wright, and Christian Wüthrich. We want finally to express our warmest thanks to Lindy Divarci and Sylvia Szenti for their fundamental help in the editorial and publication process.

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Contents

The Renaissance of General Relativity in Context: A Historiographical Review

Alexander S. Blum, Roberto Lalli, and Jürgen Renn

This volume aims to provide a multifaceted and nuanced understanding of the process dubbed as the "renaissance of general relativity" (Will 1986, 3–18) by elaborating on a historiographical framework developed by the editors (Blum et al. 2015). Our papers on the subject (Blum et al. 2015, 2016, 2017) and the occasion of the 100th centenary of Einstein's formulation of general relativity sparked an intense and complex historical debate on the causes, origins, and manifestations of this process, identified as the renaissance of general relativity by physicist Clifford Will, who defined it as follows (Will 1989, 7):

> Despite its enormous influence on scientific thought in its early years, general relativity had become a sterile, formalistic subject by the late 1950s, cut off from the mainstream of physics. Yet, by 1970, it had become one of the most active and exciting branches of physics.

This view, exposed by a working physicist who had entered the field as Kip Thorne's PhD student at the climax of the renaissance phase, is complementary to the view that research on general relativity underwent a period of stagnation between the mid-1920s and mid-1950s, as discussed in detail by historian of physics and pioneer of historical studies on relativity, Jean Eisenstaedt (1986, 1987). Strikingly enough, both Will and Eisenstaedt began to publish their views in the same year: 1986. Clearly, by the second half of the 1980s, physicists and physics historians alike were under the strong impression that research on general relativity had undergone a major shift with regard to the situation in the early 1950s—a shift that could be understood as the return of Einstein's gravitation theory to the mainstream of physics.

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From a historical perspective, this perceived shift poses major questions that have many implications on how we understand the process of scientific change when it relates to major intellectual or conceptual developments. Was the perceived view of a renaissance a faithful representation of a deep historical transformation? Or was it rather a simple consequence of the radical transformation of the social landscape of physics following World War II? Which historical forces played a role in sparking the renaissance process? Was it a purely social phenomenon? Or did it entail modifications—conceptual or otherwise—in the theory itself and in its relation to other branches of physics? If one accepts that there was a major transformation, what were the main causes of this phenomenon? And how did it unfold? Finally, what does a nuanced understanding of the renaissance phenomenon imply for our historical conceptualization of radical scientific changes in physical theories akin to Kuhnian paradigmatic shifts?

In addressing these questions, science historians and physicists have reached quite different and at times conflicting conclusions, which we will briefly sketch and categorize in the following section. Most of these analyses are based on novel empirical research and, like all the chapters in this volume, they have greatly increased our understanding of the renaissance process. Our own approach is to provide a synthetic view that, while taking into consideration most scholars' findings and perspectives, aims to make evident the structural changes inherent in the renaissance process by emphasizing the *connections* between the changing social dimensions and the intellectual transformations we identify as having occurred during the renaissance phase, once this phase has been properly defined.

The decision to use the label "renaissance" in this volume and in some of our previous papers could be, and indeed has been, criticized by some. We admit that the name is strongly value-laden, as is all historical periodization, starting from the problematic categories of the Middle Ages and the artistic, cultural, and political Renaissance from the fourteenth to the seventeenth centuries. In fact, we did not start our analysis from the uncritical assumption that there was a renaissance. Nor do we believe that the word renaissance is necessarily the best metaphor for identifying this phase of post–World War II research on general relativity. However, our approach has never been to question the "word" itself, which we see as an accepted and widespread identification of a historical period in the development of general relativity research that is used by both working physicists and science historians alike (Will 1989; Ellis et al. 1993; Kragh 1999; Kennefick 2007). Rather, we critically focus on the renaissance as a historical period in relativity research that requires better definition, and in doing so we have tried to identify major changes (real or perceived), as well as their most relevant causes. In using the word renaissance, however, we are not automatically subscribing to the triumphalist tone sometimes related to this or similar terms, such as the "golden age" of black hole research in the period 1963–1974 (Thorne 1994).

1 Views of the Renaissance

Those historians who strongly object to the use of such terms as "renaissance" or "golden age" usually consider the changes—if indeed there were any—as simply being related to social or, more precisely, quantitative transformations in the physics landscape following the changing role of physics in national welfare and security after World War II, or even after the Sputnik Shock (Goenner 2017a). It has been pointed out that in specific nations and in specific research programs, there was much more continuity than the term renaissance can imply. The term is perceived as being implicitly dismissive of pre-renaissance contributions, or even apologetic toward those research programs (often related to specific research groups or "schools") that became prominent during the renaissance phase (Goenner 2017b; see also Salisbury in Chap. 7).

The changing social conditions of the Cold War are usually credited as one of the major forces behind the renaissance, also by those who perceive general relativity as having undergone a major shift sometime between the 1950s and 1960s. Historical actors, especially those based in the United States, stressed the role of military patronage and financial help in supporting research on general relativity when it was still a marginal field (Goldberg 1992)—a view we would like to call the "Sugar Daddy" scenario. Recently, science historians have provided much more nuanced perspectives on this point. It has been shown, again especially for the US environment, what the changing Cold War situation meant for the development of a field like general relativity. David Kaiser and Dean Rickles uncovered the role of private patronage from eccentric American businessmen who had a passion for gravitational theories or, more precisely, nurtured the hope that research in this field would result in the development of anti-gravitational technology (Kaiser and Rickles 2018; see also Rickles in Chap. 3). As the authors show, these activities had their most relevant impact in the United States where they sparked research in gravitational theory, both at the individual and at the institutional levels, and triggered the subsequent funding of such programs by the military. But the Cold War environment did not just mean more money for this kind of research. It also meant that more people entered the field of theoretical physics to meet demands for personnel driven by the rivalry of the superpowers (Kaiser 2002, 2012), which in turn led to deep transformations in pedagogical practices and in the teaching of general relativity in the United States (Kaiser 1998).

A different view is held by those who, like Clifford Will himself, put more emphasis on the role of technological advances (often directly related to wartime research activities) in providing the means to empirically test general relativity, or alternative gravitational theories, with an unprecedented degree of precision (see, e.g., Will 1986; Peebles 2017). According to this view, the serendipitous discoveries of astrophysical phenomena that could be interpreted only with a suitable theory of gravitation and related cosmological models, such as that of quasars in 1963, cosmic microwave background radiation in 1964/65, and pulsars in 1967, had a dramatic impact in turning general relativity from a curiosity into a theory essential for understanding, or at least studying, previously unknown physical phenomena (Longair 2006; Bonolis 2017; Bonolis & Leon in Chap. 9). It is sometimes held that only after these discoveries had been made was general relativity recognized as a beautiful theory that had not been employed as it deserved to be—a view we would like to call the "Sleeping Beauty" scenario. Within this scenario, the Cold War environment again becomes one of the most relevant factors for a careful historical reconstruction since it directly provided the technological means for testing gravitation theories and cosmological models (Wilson and Kaiser 2014; De Bianchi 2019).

Besides the Sugar Daddy and Sleeping Beauty scenarios, which are the most widespread, the literature offers various other factors as having sparked the renaissance. These range from the creation of new theoretical tools that significantly simplified calculation in general relativity and also increased the intuitive understanding of its implications (Wright 2014), the role of conferences in bridging previously separated groups and researchers (Lichnerowicz 1992; Schweber 2006; Rickles 2011; and Martinez in Chap. 4), the role of Einstein's death in liberating the space for new approaches and leaders, against the spell of the unified field theory approach promoted by Einstein himself (Schutz 2012), or the role of important pioneers in turning their interest from mainstream research to a marginal field, thus appealing to a younger generation of physicists. This view usually takes as a major example John Wheeler's move from nuclear physics to general relativity in the early 1950s (Misner 2010; Rickles 2018; see also Blum & Brill in Chap. 5).

It is striking to note that all of these accounts have more than some grains of truth in them and yet in some respects are in conflict. Focusing on astrophysical discoveries as the main causes of the renaissance in fact implies entirely different chronologies with respect to focusing on the changing social composition: quasars were discovered only in 1963 while there were already considerable signs of a growth of interest in the field by the late 1950s, such as, to name a few, international conferences exclusively devoted to gravitation theories, an enormous growth of the number of papers on gravitation, and the formation of a specific committee for the promotion of general relativity and gravitation at the international level (Lalli 2017). The difficulties in selecting any one of the mentioned accounts as the full or even the most significant part of the story are obvious. Our work aims to provide both a synthetic perspective and a new chronology that builds on the various existing accounts and reconstructions as well as on those presented in this volume. This should result in a broad narrative that systematically draws connections between socio-contextual changes and the more internal, physics-related advances in theoretical and experimental work.

2 Toward an Integrated Socio-Epistemic Narrative of the Renaissance of General Relativity

The synthetic comprehension of a process as complex as the renaissance of general relativity can only begin with a revision of what is usually understood as the stagnation phase, from the mid-1920s to the mid-1950s, termed by Eisenstaedt the "low-water mark" of general relativity. As detailed elsewhere (Blum et al. 2015, 2016, 2017), one should not characterize this phase as being related to a rapid decrease of interest in general relativity, or to a sudden stop to research in this field. The "low-water mark" period of general relativity was instead essentially characterized by a strong dispersion of research agendas related to Einstein's theory, all of which were becoming marginalized with respect to major advances in physics. Research in cosmology, in unified field theory (understood mostly as field theories unifying gravitational and electromagnetic forces), and in the quantization of Einstein's field equations remained relatively active between the mid-1920s and mid-1950s (Kragh 1996; Goldstein and Ritter 2003; Goenner 2004, 2014; Blum and Rickles 2018). From the mathematical standpoint, research related to general relativity continued to be attractive to experts in differential geometry. Astronomers continued their work on testing the few astronomical predictions of Einstein's theory (Crelinsten 2006). There were also significant advances on problems such as Schwarzschild's singularity, gravitational waves, and the equations of motion in general relativity (Eisenstaedt 1987; Havas 1989; Kennefick 2007). Relativistic cosmology even reached its major breakthroughs during that period with Lemaître's formulation of the expanding universe, which was strongly promoted by the authoritative mathematical physicist Howard P. Robertson in an extremely influential review published in 1933 (Robertson 1933; Nussbaumer et al. 2009; Ellis 2012).

On the face of it, this reinterpretation of the low-water-mark period seems to imply that the renaissance was not preceded by a period of stagnation after all. In this sense, one might find compelling the counter-argument of those who criticize the term "renaissance": the true watershed was the outcome of World War II and the related military employment of nuclear physics and radar technologies for war-related research, which radically changed the priorities of physicists. However, our reconstruction of the low-water-mark period, rather than supporting seamless continuity, highlights the essential dimension in which a discontinuity in the mid-1950s can be observed, namely, the changing role of social connections between individuals and intellectual connections between their research programs. We have argued elsewhere (Blum et al. 2015; Renn et al. 2016; Lalli 2017; Blum et al. 2018)—and it is discussed in detail by Lalli et al. in Chap. 2—that the structure of the general relativity networks, both social and epistemic, changed radically during the 1950s.

Our narrative asserts that scientists—though dispersed in various isolated institutional and national settings, or more often as isolated individuals—continued to pursue studies on topics related to general relativity: unified field theory, mathematical problems, cosmology, and, especially from the late 1940s, quantum gravity *avant la lettre*. These studies provided the potential that would be activated, thanks to radical changes in the socio-economic-political landscape of physics after World War II. This activation can be attributed to the growth in the number of PhDs in physics, which gave individuals working on general relativity–related topics the possibility to form research centers around specific research agendas related to gravitation theories or, later, experiments. These research agendas, in some specific national settings, were promoted with private and military funds that rapidly increased the pursuit of research in this field. The increasing chances of traveling with military assistance, various other funds, and the solidification of the tradition of long postdoctoral education in the post–World War II period created the conditions that enabled research to be shared between recently formed centers at the national and international level—a phenomenon that David Kaiser has called "postdoc cascade" (Kaiser 2005). In this way, isolated research centers became part of a growing and increasingly connected network. The speed of this phenomenon was greatly accelerated by an explicit attempt to build up an international community of scientists working on these topics, especially through the organization of devoted international conferences (Lalli 2017).

The case studies on the national environments of the United States and the Soviet Union presented in this volume by Dean Rickles (Chap. 3) and Jean-Philippe Martinez (Chap. 4) are particularly significant in showing how this phenomenon rapidly assumed a global character. In many respects, the sociocultural and economic contexts of relativity research in the United States and the Soviet Union could not be more different. Indeed, the authors show a quite different panorama. As mentioned above, in the United States, two eccentric businessmen, Roger Babson and Agnew Bahnson, strongly influenced the renaissance process by providing the infrastructural conditions needed to pursue this research in the first place. While this was a specifically American affair, the process had tremendous consequences also at the international level, especially when it led to the organization of the second, and extremely important, international conference on gravitation theory of the renaissance period: the 1957 Chapel Hill conference (DeWitt and Rickles 2011). The conference was organized as the inauguration event of the newly established Institute of Field Physics at the University of North Carolina, the foundation of which was made possible by the generous involvement and support of Agnew Bahnson (see also Rickles 2011).

In the Soviet Union, during the low-water-mark phase, the physicist Vladimir Fock worked hard to defend general relativity against the attacks of dogmatic Marxist philosophers by arguing that some particular interpretations of Einstein's gravitation theory did not contradict the tenets of the official philosophy of the Soviet Communist Party: dialectical materialism (see, e.g., Graham 1982; Gorelik 1993). Even after Fock had been able to officially rehabilitate Einstein's theory in the Soviet Union, discussions on the theory in the early 1950s remained largely within the framework of philosophical debate. This is witnessed by the controversy about the equations of motion between Fock and Leopold Infeld in Poland, a discussion that took place in philosophical journals. Also in Soviet-dominated Eastern Europe, the renaissance radically transformed the dimension of the debate on relativity. It turned this debate from a philosophical-ideological discussion into a deliberation of the physical foundations open to an international audience. Martinez's contribution shows how this change of dimension occurred during and after the 1955 Bern conference, which can be considered a watershed for many different elements of the renaissance process.

By the early 1950s, several centers that dispersed throughout the world were pursuing, albeit in isolation, relevant work in the field with their own specific research agendas. In the United States, the most renowned examples were those built by Peter Bergmann at Syracuse University and John Wheeler at Princeton University, both of whom increasingly devoted themselves and their students to topics related to general relativity, especially in the attempts to quantize Einstein's field equations (Bergmann) or to provide a unified theory that might explain the nature of particulate matter (Wheeler). Both centers became important hubs in the renaissance network, as discussed by Salisbury in Chap. 7 and Blum & Brill in Chap. 5 (see also Chap. 2 by Lalli et al.). These are only two of the various centers that provided the potential for the renaissance, and others are notably the Cambridge University core—centered around Fred Hoyle and Hermann Bondi working on steady state cosmology and also Paul Dirac with his idiosyncratic interests at the time—and the centers built by Pascual Jordan in Hamburg, Leopold Infeld in Warsaw, and André Lichnerowicz in Paris, to name just a few. All of them had many students from the 1950s and rapidly created real research centers devoted to general relativity-related fields.

The process of the renaissance, however, was more complicated than simply connecting existing research agendas and providing the conditions for their existence and extension. The dynamics of the renaissance, evident as early as 1955 at the Bern conference, had a strong impact on the research agendas themselves. These shifted considerably toward problems concerning general relativity proper as the main issue in overcoming any attempt to build alternative or more superior theories. Focusing on the case of gravitational waves, we have shown elsewhere (Blum et al. 2018) that the social transformations of the renaissance had considerable influence on how the theory was understood and on which topics were addressed. Research agendas shifted considerably in many of the above-mentioned groups toward a closer understanding of the physical predictions of general relativity proper, where by "general relativity proper" we essentially mean the theory as originally formulated by Einstein.

With general relativity now being analyzed in great detail for its physical predictions, it was increasingly compared and set in relation to other physical theories, rather than being viewed as constituting its own genre of theory, somewhere between physics, mathematics, and philosophy. Despite its newly won comparability, general relativity was still regarded as an unusual physical theory. Indeed, the direct comparison with other theories perhaps even emphasized the special status of general relativity, leading to what Brian Pitts in Chap. 6 calls "GR exceptionalism." Pitts argues that the overemphasis on the difference to other theories in some instances was detrimental to an understanding of the structure of general relativity and of its physical predictions. At the same time, it was precisely these differences—

the features that made general relativity unique as a physical theory—that made general relativity such a fruitful field once it became an object of study in its own right during the renaissance. We have called this the "untapped potential" of general relativity, and in Chap. 5 Dieter Brill and one of the authors (AB) have analyzed how John Wheeler came to recognize this potential and how it ultimately motivated him to enter the field of general relativity research.

The implication of GR exceptionalism was one of the few issues that could act divisively within an emerging community that otherwise, in the manner in which cooperation and epistemic progress went hand in hand, came rather close to a scientific ideal. But opinions diverged on what general relativity exceptionalism implied for the problem of unifying general relativity with quantum theory. Would general relativity have to accommodate, giving up its exceptional status by shedding, for example, its extravagant four-dimensional formulation? Or would new quantization methods have to be devised to deal with the intrinsic general covariance and other unique features of general relativity? This was the central issue dividing the research centers in Syracuse and Princeton, and Don Salisbury in Chap. 7 shows how the Syracuse view of what might be called "strong general relativity exceptionalism" ultimately lost out to the more conciliatory $3 + 1$ decomposition that lay at the heart of the Wheeler-DeWitt equation. A similarly moderate view prevailed in the question of gravitational waves: while the concept of radiation was overhauled to become adequate to general relativity, it was still recognizably tied to the concept of electromagnetic radiation and far less exceptional than some claims still being made in the mid-1950s that the concept of energy (and energy transmission through waves) would entirely break down in general relativity. In the course of the renaissance, general relativity thus became a bona fide physical theory, with prominent, yet clearly definable and unique features.

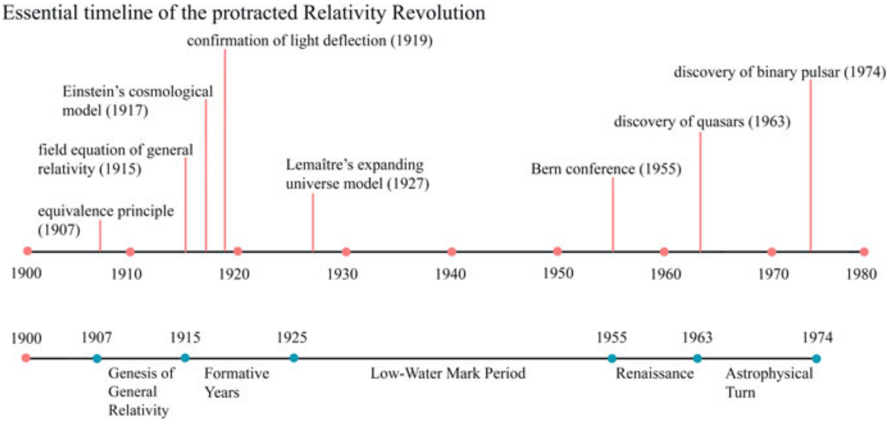
3 The Astrophysical Turn of General Relativity

While this process was evolving and a community already forming around this newly emerging field that was collectively categorized as "general relativity and gravitation," major events disrupted this course. Quasars were discovered and part of the general relativity community joined forces with astrophysicists and astronomers to create a brand new research field named relativistic astrophysics. In a couple of years, another major discovery, cosmic microwave background radiation, sparked the creation of yet another field of research: observational cosmology.

These discoveries, and the subsequent discovery of pulsars in 1967, had a strong impact on the field of general relativity (as emphasized by those who view the astrophysical discoveries of the 1960s as the major driving force in the renaissance phenomenon), yet they cannot be easily interpreted as consequences of the process we have described so far. Quite the contrary, these astrophysical discoveries followed an independent path that was dependent on technological advances and astronomical interests. However, the way in which these serendipitous empirical discoveries were understood and handled by the increasingly connected community of "relativists" was, in fact, strongly dependent on the renaissance process. The creation, almost out of the blue, of the field of relativistic astrophysics was intimately connected with the decision to open a new research center devoted to general relativity in Austin, Texas. Three scientists, Ivor Robinson, Alfred Schild, and Engelbert Schucking, all of them strongly involved in the international relativity community, decided to celebrate the opening of the center with a major inaugural conference on a topic devoted to the new discoveries and designated this topic "relativistic astrophysics" as a new research field. These three "relativists," together with the well-known relativity expert Peter Bergmann, organized this very first conference of the field (Robinson et al. 1965; Schucking 2008). Surprisingly, no astrophysicist was involved in the organization, even though, of course, astrophysicists provided the conceptual ground by connecting the discovery of quasars to gravitation theory (Bonolis 2017; see also Lalli et al. in Chap. 2, and Bonolis & Leon in Chap. 9).

The same might be said for the "discovery" of cosmic microwave background radiation. A prediction of such an effect had already been made 17 years earlier in the framework of the Gamow-Alpher-Herman Big-Bang cosmology program, but it was essentially ignored. When an effect of the predicted magnitude was discovered by Bell Labs scientists Arno Penzias and Robert W. Wilson, it was immediately reconceptualized as an empirical test for alternative cosmologies by scientists who were working intensively on empirical gravitation tests in the renaissance context, especially by Robert Dicke, Jim Peebles, and their group (Kragh 1996). Both examples are similar in that they show a community ready to take empirical discoveries and insert them into an existing framework that was at the same time both conceptual and social.

In this sense, the *interpretation* of the new astrophysical discoveries was an integral part of the renaissance process. Nonetheless, they also had a disruptive influence by sparking new research agendas and providing new connections between different disciplinary fields. As is shown in the bibliographical studies presented in Chap. 2 by Lalli et al., this effect was so great that it requires its own historical categorization, beyond the scope of the renaissance process. The post-1963 general relativity landscape, which began to include relativistic astrophysics and observational cosmology, was quite different from research in the previous period. In order to take this diversity into account, we propose to define a new period called the "astrophysical turn of general relativity," connected with but clearly distinct from the early phase of the renaissance, which covers the mid-1950s to early 1960s (see Fig. 1). This view is consistent with what Jaco de Swart proposes in Chap. 8 when he describes the cosmological turn of the astronomical practices from the 1960s. These practices became more and more concerned with attempts to understand the structure and evolution of the universe and thus entailed a deep disciplinary reconfiguration of astronomical research. As described by de Swart and confirmed by the reconfiguration of the research topics and co-citation network in Chap. 2 by Lalli et al., the astrophysical discoveries provided a strong incentive to increasingly focus general relativity research on the physical predictions of the theory, much to



Timeline of the historical period of research on general relativity, 1907–1974

the detriment of other research agendas still being pursued. Outer space became the playground of relativists and the arena some physicists had being looking for since the mid-1950s to make Einstein's gravitational theory a physical theory with strong connections to other branches of physics. But, as de Swart points out, the spread of an empirical approach to cosmology that dominated the post-quasar cosmological turn still had significant intellectual connections to the approach to cosmology that dominated the early renaissance phases between the mid-1950s and early 1960s. The belief in the "closed universe model" had been solidified during this early-renaissance phase as a consequence of the widespread attitude to the relevance of Mach's principle in cosmological considerations (Barbour and Pfister 1995). This path-dependency, de Swart argues, even provided the basis for early work on the dark matter hypothesis in the early 1970s as the only hypothesis that could save the closed universe model with respect to empirical observation of the density of matter in the universe.

By the same token, experimental gravitational radiation research exploded in the 1970s, essentially as a result of Joseph Weber's previously solitary efforts and his claims to have actually detected gravitational waves. As shown by Bonolis & Leon in Chap. 9 and La Rana in Chap. 10, this announcement had a number of repercussions in different countries and provided strong motivation for experimental physicists to enter the field. Also in this case, research was strongly path-dependent with regard to the preceding renaissance process. Weber was inspired to pursue this work because of theoretical results from the 1950s (Saulson 2011; Trimble 2017; Blum et al. 2018). In turn, his alleged discovery sparked renewed interest. Even though the community quickly reached a consensus on the unreliability of Weber's results, most of the people entering the field in that period continued to be involved in gravitation-wave research. They provided the means, expertise, and personnel for the LIGO, Geo600, and Virgo projects, which collaborated on the recent detection of gravitational waves. On the other hand, gravitational-wave research was soon framed in terms of the astronomical relevance that this discovery might have in line with the new common priorities of the "astrophysical turn."

Gravitational waves were soon understood, and promoted to funding agencies, as a way of acquiring new observational knowledge about the cosmos that would otherwise be impossible, as is shown by the widespread term "gravitational-wave astronomy" (Press and Thorne 1972) and the fact that the LIGO detectors have been defined as being an "observatory." By detailing the work on gravitation-wave detection research in European countries, La Rana (Chap. 10) and Bonolis & Leon (Chap. 9) clearly convey the sense in which, during the astrophysical turn, general relativity had become reliable enough to form the basis of Big Science international enterprises.

4 The Renaissance of General Relativity and the Historical Epistemology of Scientific Change

The overall narrative resulting from our study, from the studies in the volume, and from our assessment of previous studies is a complex picture that takes into account the various dimensions of the renaissance, including elements of continuity that seem to disappear if the term "renaissance" is taken at face value. Still, we have articulated why the strong sense implicit in the use of the "renaissance" term seems inescapable: the increasing speed of information flow, the changing research agendas, the physicalization of the theory, and its rapid connections to observational discoveries were all mostly absent in the preceding period, and it is therefore understandable why scholars have characterized the process as a renaissance.

This analysis offers a new model for scientific transformation understood as a co-evolution of epistemic and social structures that cause disruptions in the architecture of knowledge, but at the same time result from continuous and gradual processes. Although the basic formalism of Einstein's theory remained the same, its conceptual implications and its place within the disciplinary framework changed radically during the renaissance phase. The Einsteinian revolution of 1915, as novel as it was in its reconceptualization of Newtonian gravity, was to some extent superficial, transforming neither epistemic practices nor the social organization of physics, astronomy, and astrophysics. The central position that general relativity has in contemporary physics can only be understood through the further discontinuities brought about by the processes leading up to the renaissance. One might thus consider the entire period (1915–ca.1970) as an extended, though not continual, transformation of our understanding of time, space, and the universe. This one might call the protracted relativity revolution. The transformation was a collective endeavor involving the self-organization of a community. It cannot easily be divided into "normal" scientists pursuing their problem-solving routine and creative innovators worthy of detailed historical investigation.

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The Socio-Epistemic Networks of General Relativity, 1925–1970

Roberto Lalli, Riaz Howey, and Dirk Wintergrün

1 Introduction

Network has long been a catchword in history writing. Until recently, however, network concepts and terminologies have been mostly employed on a purely metaphorical level without engagement with the tools of network theory developed in sociology, biology, physics, engineering, computer science, and so on (Freeman 2004).[1] For decades, *network* has been employed to quickly label complex social and, more rarely, conceptual phenomena with little if any explanatory value other than some generalized recognition of the role of social structures in history.[2]

A new body of literature has gone significantly beyond these early attempts to conceptualize and narrate historical processes at the level of network dynamics. Thanks to increasing computing power, newly available big-data repositories and dedicated software programs, a research field has recently emerged called Historical Network Research (HNR), which promises to make the most of formal methods of Social Network Analysis (SNA) applied to corpora of historical data (Düring 2017).[3] Within this broad field, new approaches have been developed aimed at employing network concepts and tools in the history of science (e.g., Fangerau 2010; Preiser-Kapeller and Daim 2015; Breure and Heiberger 2019).

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[1] See also (Wellman 1988).

[2] We are not referring here to efforts in science and technology studies to produce a social theory of science and technology based on network notions, such as the Actor-Network-Theory (see, e.g., Latour 1987).

[3] For an extensive introduction to the field of SNA, see (Wasserman and Faust 1994).

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These trends have produced a number of insights demonstrating the relevance of applying formal and computational methods to historical data (Lemercier 2015). So far, however, the field of HNR has mostly studied *social networks* and their historical developments (Rollinger et al. 2017). Only rarely has HNR pursued the more ambitious goal of investigating different kinds of networks in the history of science—for example, material or conceptual networks. In the history of science, these exceptions have usually been in the context of scientometric studies aimed at evaluating and analyzing scientific production (for an exception, see Valleriani et al. 2019). By using network-based tools applied to bibliographical data, especially co-citation networks and bibliographical coupling, researchers have explored various historical mechanisms, including identifying emerging research fronts (De Solla Price 1965; Zitt and Bassecoulard 1994; Boyack and Klavans 2010), tracking disciplinary developments (Small 1993; Peirson et al. 2017; Khelfaoui and Gingras 2019; Painter et al. 2019), analyzing theory change (Sullivan et al. 1980), and exploring the impact and diffusion of scientific innovations (Nadel 1981; Herfeld and Doehne 2018). Network theoretical approaches provide also the conceptual and methodological bases of recent attempts at establishing a computational history of science (Laubichler et al. 2013, 2019; Gibson et al. 2019). This field is emerging with the goal of promoting the use of digital technologies in framing both questions and methods as well as in changing the working practices of science historians.

Valuable as all these approaches are, network theory has been usually employed in the history of science to investigate one specific element at a time, be it social actors or publications (for exceptions, see Börner et al. 2004; Mutschke and Haase 2001). Here, we aim for the more ambitious goal of offering a formal, integrated narrative that takes into account social and material factors together with the conceptual transformations of a research area. This framework contributes to recent attempts to provide grand longue durée narratives of the structural dynamics of knowledge evolution (Renn 2020).

To pursue this program, a conceptual framework has been recently developed by two of the authors (RL, DW) along with Manfred Laubichler, Jürgen Renn, and Matteo Valleriani. This framework defines a model in which knowledge networks are composed of three different layers: the social network, the semiotic network, and the semantic network (Renn et al. 2016). The first is defined as the collection of relations involving individuals and institutions, which encompasses any kind of structured organization of individuals. The semiotic network is defined as the collection of the material or formal representations of knowledge, which include a broad variety of entities, such as books, articles, journals, instruments, reports, or also particular institutions that might be considered, under particular circumstances, as embodiments of specific knowledge elements. Finally, the semantic network is a collection of knowledge elements and their semantic relations, where for knowledge elements we specify a more abstract level of knowledge: concepts, topics, research agendas, and methods. We call the set of these three levels *socio-epistemic networks*.

Clearly, each of these layers presents a very complex structure and is better understood as a multilayer network in its own right. An ideal strategy, then, would be based on multilevel approaches that are currently being developed, especially in the study of longitudinal networks in SNA (Dickison et al. 2016; Lazega and Snijders 2016). The theoretical field of multilayer network is rapidly evolving, which, we believe, will soon provide the mathematical and conceptual frameworks needed to realize the full potentiality of our scheme for the investigation of scientific fields' developments. Before we can make full use of these mathematical models, however, the conceptual framework of socio-epistemic networks can be already usefully employed to investigate the dynamical evolution of each of the three network levels separately and then in comparison.

We have applied this formal approach to the analysis of the history of general relativity based on bio-bibliographical data ranging from 1925 to 1970, namely, from the year usually taken as the beginning of the low-water-mark phase of general relativity up to the year recognized by Clifford Will as the moment in which general relativity was indisputably returned to the mainstream of physics (Will 1989, p.7; see also Blum et al. in Chap. 1 of this volume). The next three sections will focus each on one of the three different layers of the socio-epistemic network. The second section deals with collaboration network dynamics. The third addresses the semiotic networks identified by citation and co-citation networks in the scientific literature. The fourth section will tackle the challenges of exploring the network of concepts and research agendas encoded in general relativity publications using a first quantitative approach combining the semiotic layer and the semantic layer. In the conclusion, the connections between the three different layers will be explored, and a new narrative of the renaissance of general relativity based on these results will be provided.[4]

While it is the goal of our research program to investigate causal inferences connecting the three levels, this chapter is dedicated to an analysis of intra-layer structure, laying the foundation for a future formal analysis of causal relations connecting the dynamics of the different levels. Here, we focus on what we see as the moments of change and provide a historical interpretation of these changes. The next step will be a quantitative investigation of the integrated dynamics of the three levels based on the mathematical framework of the multilayer network analysis. This includes, in particular, a deeper analysis of, first, the role played by conferences in this field as triggers of the reconfiguration of the three levels of the socio-epistemic networks and, second, of the way institutions act as aggregators of knowledge both implicitly, by allowing an exchange of thoughts, and explicitly, by written documentation, which can be described as the formation of an institutional memory.

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[4] For the concepts and historiography of the renaissance of general relativity, we refer to the Blum et al. in Chap. 1 of this volume. See also (Blum et al. 2015, 2016, 2017). Excerpts from the results here presented have appeared in (Blum et al. 2018).

2 The Social Layer: The Development of the General Relativity Collaboration Network, 1925–1970

The investigation of the social dynamics of scholars working on general relativity depends crucially on how one defines collaboration and, in a quantitative approach, as here pursued, how one identifies the relevant data. Most studies of scientific collaboration networks limit themselves to the investigation of co-authorship networks retrievable in online repositories such as Web of Science (WoS). This is, of course, an obvious choice in scientometrics because of the ready availability of a reasonably complete set of data and metadata. The operational attitude to equate collaboration to co-authorship has, however, a number of issues that have been raised by scholars reflecting on the nature of scientific collaboration (Katz and Martin 1997; Wray 2002; Boyer-Kassem et al. 2017).

In a study spanning from the mid-1920s to the 1970s, like ours, these problems become even greater as publication, collaboration, and co-authorship practices vary over time and locality. Moreover, online repositories are not complete enough for reliably retrieving co-authorship relations in less recent periods. Limiting the analysis of the historical dynamics of collaboration networks to co-authorship relations in online repositories, such as WoS, is for these reasons misleading, as we have shown elsewhere (Lalli et al. 2020). We have therefore defined and painstakingly collected various kinds of collaboration link data. These include manually retrieved co-authorship relationships as well as other kinds of collaboration not visible from online article repositories including influence relationships such as PhD supervisor-PhD student relations. To this, we added one type of collaboration of a hypothetical nature, *copresence at institution,* derived from the assumption that being at the same institution at the same time constituted a potential for scientific collaboration. This integrated approach is of course made possible by the highly collaborative interdisciplinary environment of our team, which included science historians and data analysts.

The decision of including multiple types of collaboration in the analysis brought on the problem of the appropriate duration of the edges, or links. Again because of the long timespan of our study, we did not assume that co-authorship links, once created, stay forever, as usually done in the analyses of co-authorship networks. Most studies of longitudinal co-authorship networks simply compare co-authorship networks in different time periods. Our perspective required instead a methodology for the steady evolution over time of the collaboration network, which might make evident particular shifts, as those hypothesized for the passage between the low-water-mark phase and the renaissance period.

We pursued a comparative approach with different link durations and definitions, the methodological and conceptual details of which have been published in (Lalli et al. 2020).[5] Here, we only report the main results and their historical interpretation. As illustrations, we have chosen only two typologies of collaboration networks among those investigated by us. The first is a network containing the following kinds of relationships:

- Co-authorship edges, retrieved from WoS, the NASA Astrophysics Data System repository, and manual data-taking from historical sources.
- Collaboration edges, containing all kinds of collaboration that did not result in co-authored publications.
- Influence relationships, including PhD supervisor-student relationship and other kinds of historically documented influence relations.

The second longitudinal network contains all the previous edges plus the copresence at institution edges. For brevity's sake, we will call the first network the *collaboration network* and the second one the *socio-institutional collaboration network*. As mentioned above, the socio-institutional collaboration network is considered as a potential infrastructure for the sharing of information, questions, methods, and agendas, which could have been later fulfilled in the actual realization of collaboration regimes.

Apart from restrictions on the definition of relationships, we made the following assumptions. We used the *basic flattening* rule for a multilayer network (Dickison et al. 2016, 74): two nodes have one, and only one, edge between them each time the linked actors are related through at least one of the collaboration relations listed above. For each scholar a temporal range was defined during which he—or, more rarely, she—effectively worked on topics related to general relativity, called the *GR-activity timespan*. In our analysis, we consider only relations that took place when both actors involved were active in general relativity (GR) research. Each edge has an 8-year trace, which means that two scientists are regarded as disconnected when their last relationship stopped more than 8 years earlier. Finally, for each year, we consider the social network created by all nodes who were active at least 1 year in the previous 8 years.

We studied the evolution over time of some parameters of both networks with the aim of answering the question as to whether it is possible to identify specific moments that signal the formation of a scientific field. We followed the hypothesis put forward in Bettencourt et al. (2008, 2009) that the formation of a giant component of the collaboration network might be considered a proxy for the formation of a scientific field of scientists sharing questions, tools, and methods.[6]

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[5] The detailed description of how the dataset was created and prepared for data analysis is described in (Wintergrün 2019).

[6] In network theory, there is no agreed-on formal or quantitative definition of giant component and we rested on the intuition that a giant component is the largest connected component when it contains most of the nodes and edges of the network in very evident ways.

2.1 The Dynamics of the Collaboration Network

To investigate the possible formation of a giant component, we looked at the evolution over time of the number of nodes and edges of the largest connected component compared with that of the total number of nodes and links. Though a simple analysis, it shows clear and interesting historical patterns. One sees that, as expected, World War II affects the collaboration network, not in the sense of substantially diminishing the number of scientists, but probably interrupting the slow path toward the formation of a giant component. While the number of nodes and edges starts increasing soon after World War II, this does not result in any straightforward way in a recovery of the largest component that could become a giant component. Only at the end of the 1950s, and more specifically between 1959 and 1960, does the pattern change, with a radical increase in the number of edges and nodes in the largest component. We see this passage as the starting point for the formation of a giant component in the collaboration networks (see Fig. 1a). After this shift, the dimension of the largest component of the network steadily increases, as can be seen also by looking at the fraction of the nodes and edges of the largest component over the total number of nodes and edges, respectively (see Fig. 1b). The shift occurring between 1959 and 1960 appears even more clearly by looking at the diagram of the first derivative of the number of edges of the largest component (Fig. 1c).[7] Figure 1c clearly conveys a change in the structure similar to percolation, which suddenly increases the connection between what might appear as previously separate groups.

A closer analysis of the network reveals the major passages in this historical development. We see that right before the war a visible large component forms. In 1938, it is composed of two large groups, as identified by the Girvan-Newman algorithm for community detection (Girvan and Newman 2002). The largest is based in the United States and centered around Einstein (the red cluster in Fig. 2). The other cluster (in blue) is based in England around Arthur S. Eddington. These two major, albeit small, emerging communities are greatly separated in terms of actual collaboration patterns, the only link being British mathematician Gordon Leonard Clark who spent 1 year in Princeton between 1938 and 1939 under Howard P. Robertson after having received a PhD under Eddington (Bonnor 1983; Wintergrün et al. 2019).

Many of the collaboration edges, however, are already quite old at the end of the 1930s, which explains the sudden decline during World War II. In the aftermath of World War II, there was no attempt to re-establish collaboration between different small groups, even in the case where scientists were still active in the field. In 1946, the network looks significantly different, a lot more dispersed, and without a central major component that could be identified as a root of a possible future giant component (Fig. 3). The largest connected component (blue in Fig. 3) is again

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[7] As derivative, we are using the symmetric difference quotient for each point, see standard textbooks on numerical analysis (e.g., Kress 1998).

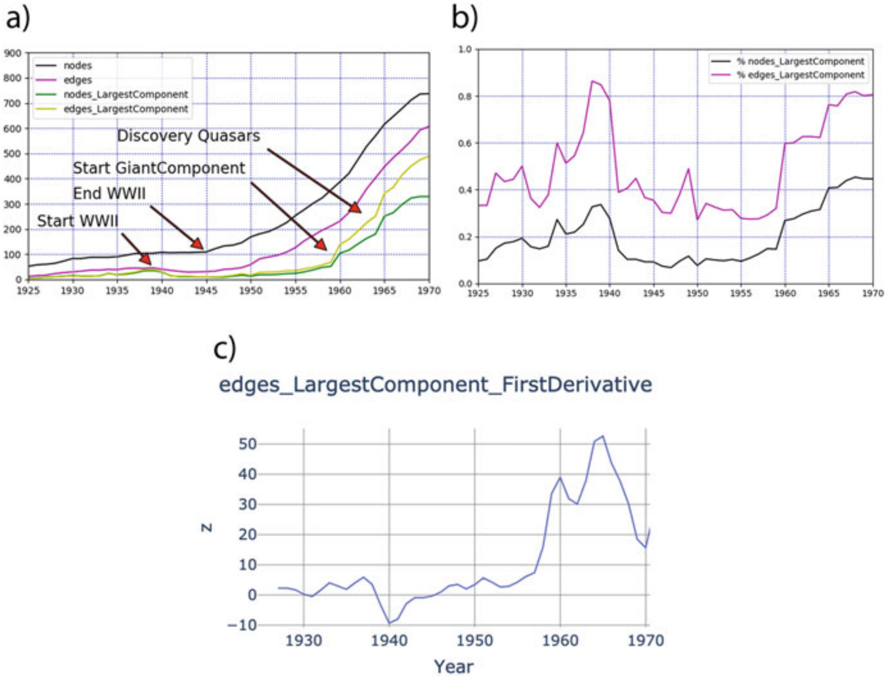

Fig. 1 *Collaboration network, 1925–1970*. (**a**) Total number of nodes and edges, and the number of nodes and edges of the largest component. (**b**) Percentage of the nodes and edges of the largest component over the total number of nodes and edges. (**c**) First derivative of the number of edges of the largest component

the US-based community centered around the aging Einstein. The second largest connected component (red in Fig. 3) is centered around French mathematician André Lichnerowicz who established a school in Paris on differential geometry applied to gravitation theories soon after World War II. The dispersion of the network is shown by the fact that there are many collaboration edges that are not connected to the largest component, which relates to the presence of many minor groups partially active in research on general relativity-related matters.

The diagrams in Fig. 1 indicate that the situation remains stationary until the mid-1950s, when the largest connected component starts becoming more significant with respect to the total number of nodes and edges in the network. But the most significant change occurs only at the end of the 1950s with a doubling of the number of nodes and edges in the largest component in just 1 year (1960). After this year, one sees a steady growth of the largest component in a process that clearly identifies a giant component in the structure of the network.

The collaboration network displays nicely the main events in shaping the doubling of the size of the largest component in 1960. The general relativity collaboration network in 1959 is characterized by the presence of increasingly

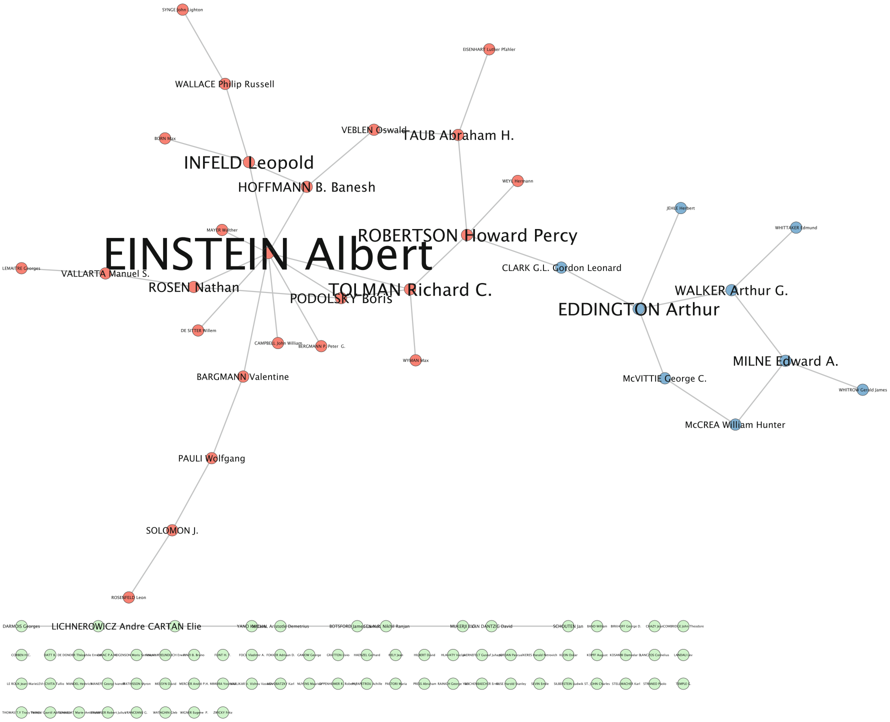

Fig. 2 *Collaboration network* in 1938. Size labels proportional to the degree centrality, that is, the number of edges connected to each node. Colors identify communities detected with the Girvan-Newman algorithm. All isolated nodes are in light green. Visualization with Visone (Brandes and Wagner 2004)

larger groups disconnected between each other (Fig. 4). The two largest connected components have a comparable dimension: The largest component (green) contains 56 scientists, while the second largest component (red) has 53 scientists. The radical shift occurring between 1959 and 1960 is due to the merging of these two large components into one significantly larger connected component, which takes on the features of a giant component.

The 1959 largest connected component centers on scientists based at Cambridge University and at King's College London, but also includes Leopold Infeld's group in Poland and John L. Synge's group in Dublin. The second largest connected component is mostly based on groups established in US universities and grows from an early connection between Wheeler's group in Princeton and Bergmann's group at Syracuse University. This edge is created by physicist Arthur Komar's going from Princeton, where he was a PhD student of John Wheeler's to Syracuse University as a junior associate of Peter Bergmann in 1957. In this image, the brokering role of junior researchers in connecting separate groups (Granovetter 1973) is especially evident as Komar and Pirani have in 1959 a high betweenness centrality comparable,

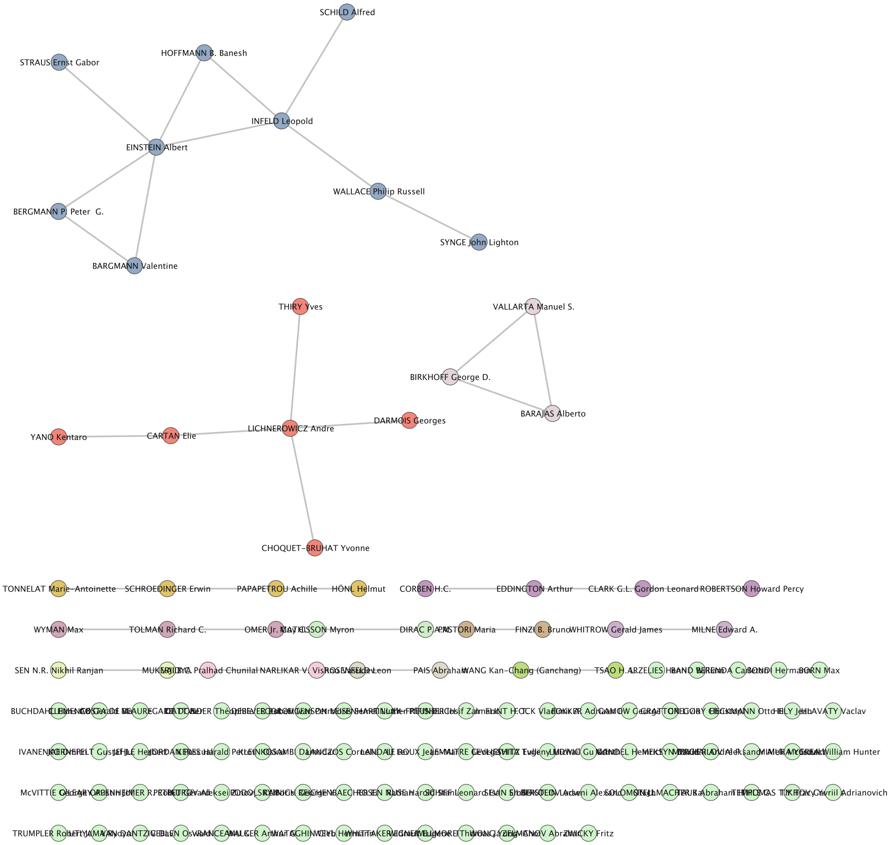

Fig. 3 *Collaboration network* in 1946. Colors identify different components. All isolated nodes are in light green. Visualization with Visone (Brandes and Wagner 2004)

if not higher than, those of research group leaders at the time (third and fourth highest values).[8]

The merging of these two previously disconnected large components in 1960 is due to the career moves of German-born physicist Rainer K. Sachs—who went from Syracuse University where he got a PhD under Bergmann to the German group built around Pascual Jordan in Hamburg—and British mathematician Ivor Robinson—who went from the Warsaw Institute of Theoretical Physics to the recently established Institute for Field Physics at the University of North Carolina (see Lalli 2017 for a description of these groups). Our analysis highlights these

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[8] Betweenness centrality quantifies the relevance of a node as a broker between weakly connected sub-parts of a connected network. More precisely, it is a measure of the number of shortest paths passing through a node (Freeman 1977).

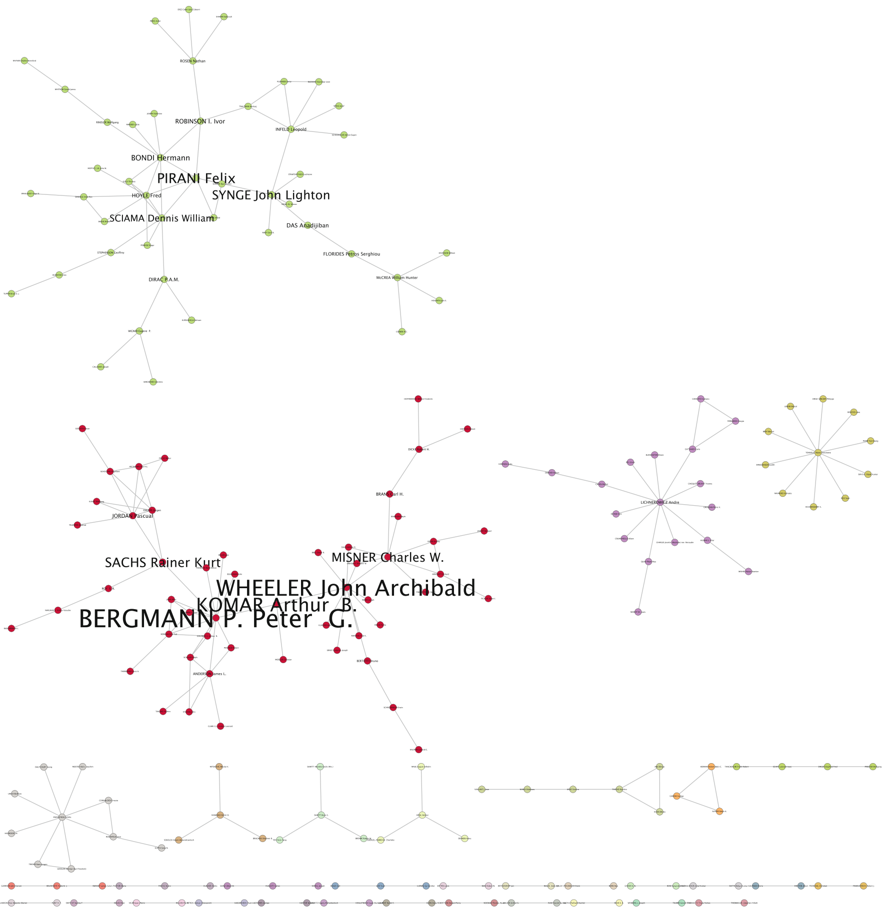

Fig. 4 The *collaboration network* in 1959. Isolated nodes have been excluded. Colors identify different connected components. Size labels proportional to betweenness centrality values. Centrality measures and visualization with Visone (Brandes 2001; Brandes and Wagner 2004)

two scholars as brokers in the formation of a giant connected component out of separated groups, as can been seen by their betweenness centrality measures (Fig. 5). Our analysis also shows that the French groups in Paris around mathematician André Lichnerowicz (orange) and theoretical physicist Marie-Antoinette Tonnelat (purple) are both large and isolated, separated from both each other and the largest connected component. Both Lichnerowicz and Tonnelat have, in 1960, a high degree centrality, but their complete disjunction from the largest component clearly conveys their marginalization within the relativity community being established. Only in 1965, do both Lichnerowicz and Tonnelat enter the largest connected component.

The general picture suggested by the network analysis here presented strongly confirms the framework suggested by the editors of this volume in their previous

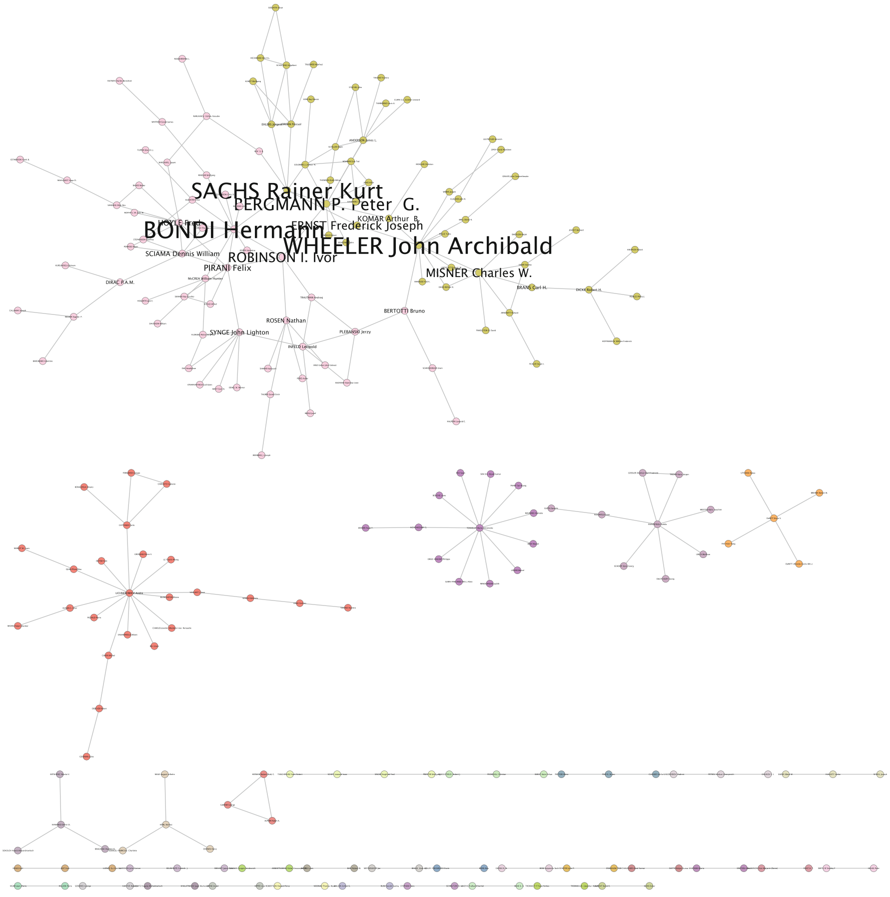

Fig. 5 The *collaboration network* in 1960. Isolated nodes are not shown. Colors identify clusters retrieved with the Girvan-Newman algorithm. In most cases, except for the largest component, the algorithm identifies different connected components. In the case of the largest component, the algorithm identifies two clusters that are almost identical to the two separated components of the 1959 network. Size labels proportional to betweenness centrality values. Centrality measures and visualization with Visone (Brandes 2001; Brandes and Wagner 2004)

works (Blum et al. 2015, 2016, 2017, 2018). After World War II, there was an increase of the number of scientists entering the field and publishing. This numerical growth might have various explanations related to the changing social landscape of research in physics. The talent and money flowing into physics and state support for the discipline certainly created the conditions for this to occur. Gravitation studies were favored by this general trend. Nonetheless, this was not sufficient to spark a topological transition in the network. In network terms, the increasing number of nodes did not lead, alone, to a changing structure of the network. The network remained essentially dispersed in separated groups for about 15 years after the end of the war, with local groups growing stronger, at least in terms of the number of people involved. Even within specific national communities, research groups could remain quite separate, as shown by the lack of connection between the two French groups centered around Lichnerowicz and Tonnelat, respectively.

The collaboration network tends toward increasing connection between the two major groups already during the 1950s, but the greatest change occurs only at the end of the 1950s, when the two largest components of the network merge. The formation of a giant component in the collaboration network can be attributed to the solidification of the postdoctoral education tradition (Kaiser 2005). Only thanks to the increasing possibilities of mobility of young researchers could the different groups be joined. The major connections between leader-centered groups are all related to specific movements of postdocs who went from one center to another, in that way allowing for the circulation of research agendas, questions, and tools.

The chronology of the dynamics of the collaboration network resulting from our analysis rules out the hypothesis that the renaissance of general relativity was a consequence of discoveries in the astrophysical domain starting with the discovery of quasars in 1963. The formation of a large social group working on general relativity was already present before this discovery, which had no major impact on the dynamic of the social network. Even the number of people entering the field was not immediately affected by this event.

2.2 *The Dynamics of the Socio-Institutional Collaboration Network*

The second network we present in this study is constructed by adding to the previous relationships the space of hypothetical copresence at institution edges. These additional collaboration links provide a safeguard against possible missing information, for example, bias due to changing publication practices and information regimes of online repositories such as WoS, or as the result of the retrospective storing of historical records—we know more about those who had a major impact on the field with respect to those who are considered minor figures. While we are here taking into consideration hypothetical collaboration edges, there is substantial evidence that, in a field still small as general relativity research, practitioners tended to meet and discuss with persons working in the same, or geographically closer, institutions (Eisenstaedt 1986).

In the new network, compared to the collaboration network in Fig. 6a, the effects of World War II are more clearly visible wherein its outbreak seems to disrupt institutional connections leading to a decrease in the number of edges, which stops exactly at the end of World War II, where the trend changes direction, as indicated by the diagram of the first derivative of the total number of edges (see Fig. 6b).

In the socio-institutional collaboration network, three relevant passages can be identified. The first one occurs between 1950 and 1951 when the total number of

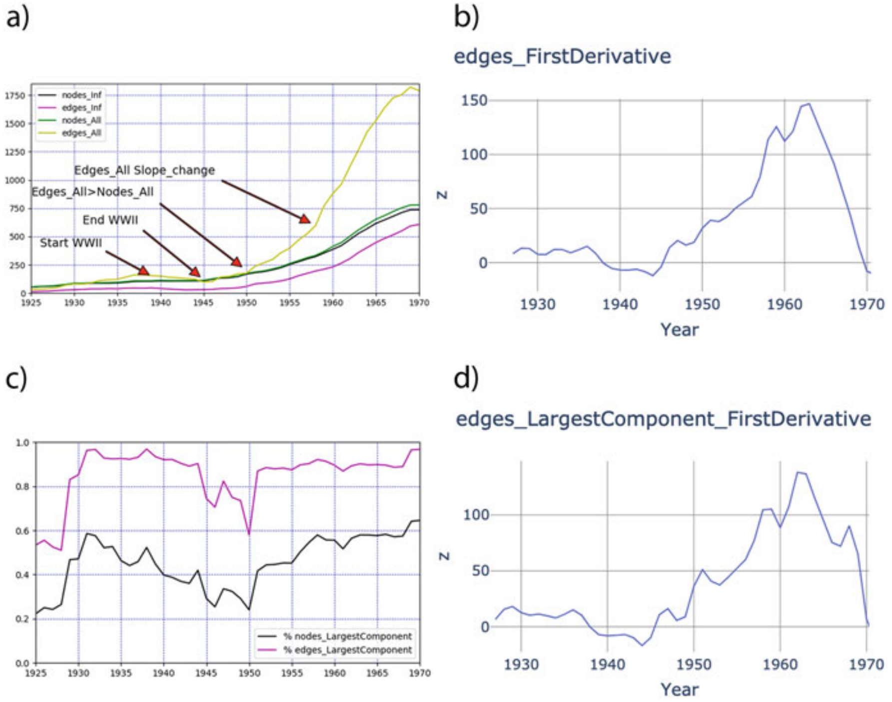

Fig. 6 *Socio-institutional collaboration network*, 1925–1970. (**a**) Comparison between the total number of nodes and edges (_All) versus the same quantities of the *collaboration network* (_Inf). (**b**) First derivative of the total number of edges. (**c**) Percentage of the nodes and edges of the largest component over the total number of nodes and edges. (**d**) First derivative of the number of edges of the largest component

edges becomes greater than the total number of nodes (see Fig. 6a). The diagram of the percentage of the number of edges of the largest component over the total number of edges shows that this passage corresponds to an evident shift in the largest component (Fig. 6c). The second one occurs between 1958 and 1959, when the rate of growth of the total number of edges starts increasing and there is a peak in the diagram of the first derivative of the number of edges (Figs. 6a, b). There is then a third change of slope at the beginning of the 1960s, as evident in the diagram of the first derivative of the number of edges of the entire network as well as of the largest component (Figs. 6b, d).

All together these diagrams provide further support to the hypothesis that a major change in the formation of a socio-institutional network occurs between the late 1950s and the early 1960s, before the discovery of quasars. One major shift occurs in fact much earlier than in the previous analysis and only a few years after the end of World War II. This indicates the emergence of what might be called the socio-institutional preconditions of the renaissance of general relativity, when institutional relations clearly preceded, and most probably favored, actual collaborations. We cannot consider the early 1950s as the beginning of the renaissance phase, at least from the perspective of social dynamics, but we can clearly see that the conditions were being established, so that the community could start forming a more connected collaboration network within the following decade.

A closer look at the network of socio-institutional relations in the years 1950 and 1951 provides a clear picture of key actors in the change occurring between these 2 years: the merging of the two largest components of the 1950 network due, again, to postdoc movements. In the 1951 reconfiguration of the socio-institutional network, a particularly relevant role seems to be played by Felix Pirani who has the second largest betweenness centrality after Alfred Schild who, thanks to his uncommon mobility for a senior scientist, also played a substantial role (Fig. 7). The new perspective reached by including hypothetical relations taking place at institutions shows that there was a strong potentiality in the institutional connections. In most cases, however, this potential did not result in more explicit forms of collaboration relationships. Central figures in this early phase would, in fact, play no big role in the process known as the renaissance of general relativity. Many of them remained quite marginal—apart from Pirani, who would become an important player in the reconfiguration of knowledge related to general relativity (see also Table 10 in Sect. 4.4).

The network in 1957 show instead a giant component already formed soon after the mid-1950s, where emerging leading figures such as Dennis Sciama and Yvonne Choquet-Bruhat assume a particularly central position in connecting different parts of the network, which represent different national-institutional settings (see Fig. 8). The diagrams also show that the division in different schools of general relativity and competition between them is not completely represented in the network structure. Wheeler and Bergmann, for instance, are quite often depicted as leaders of two different research schools (see, e.g., Salisbury's Chap. 7 in this volume). Looking, however, at a more general picture, from a socio-institutional perspective, their intellectual competition did not result in a complete separation. Quite the contrary, they are in the same cluster. In spite of competitive approaches to the topic, in the early renaissance phase, a community was emerging aimed at increasing the strength of the field from a socio-institutional perspective (in line with what argued in Lalli 2017).

The changes between 1957 and 1958 in the composition of the giant component do not seem to be radical (Fig. 9). What increases is the number of edges due to a few movements of people already part of the largest component, increasing its connectivity rather than its dimension. In this passage, one notices a major reconfiguration in closeness centrality, where closeness centrality is a measure of the topological centrality of the nodes.[9] In the first five positions in 1958, one finds only scholars who had received their PhD in general relativity-related matters in the 1950s (Sciama, Pirani, DeWitt, Arnowitt, Deser). This is remarkable, as scientists

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[9] More precisely, it is the sum of the length of the shortest paths between the node and all other nodes in the network (Bavelas 1950).

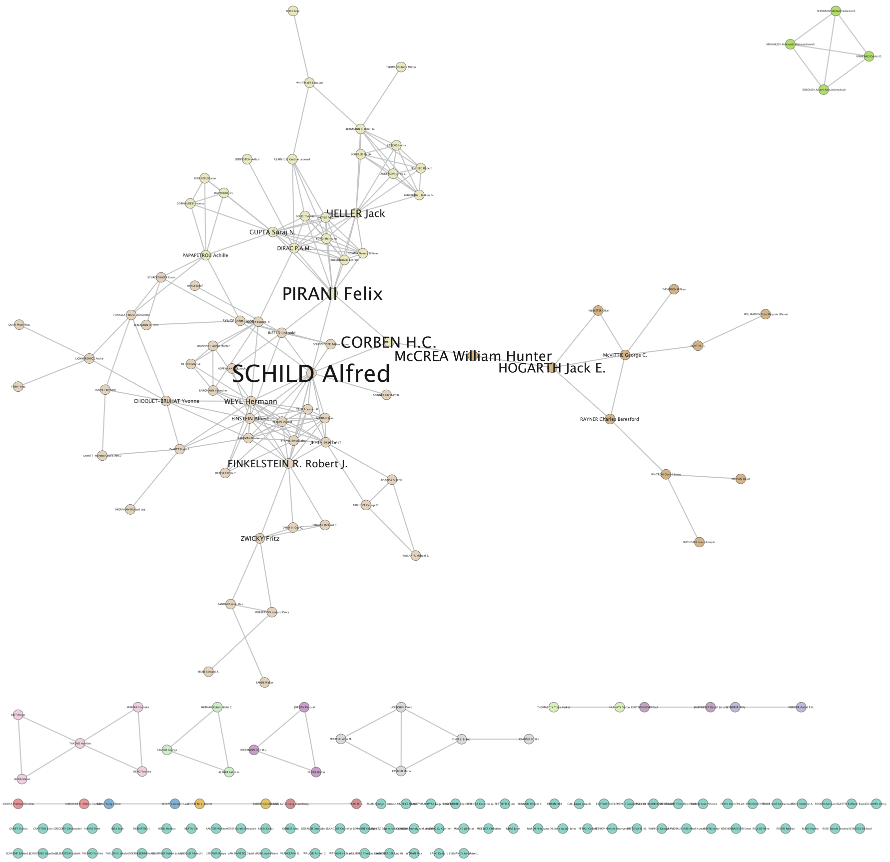

Fig. 7 The *socio-institutional collaboration network* in 1951. Colors identify clusters retrieved with the Girvan-Newman algorithm. All isolated nodes are in light green. In the case of the largest connected component, the algorithm found two clusters that can be identified with local national communities, the U.S. and British, respectively. Size labels are proportional to betweenness centrality values. Centrality measures and visualization with Visone (Brandes 2001; Brandes and Wagner 2004)

are included even if they were active only 1 year in the previous 8 years. The year earlier (1957), one finds instead scholars of an older generation such as Einstein, Gold, and Bondi.

The studies here presented provide again strong empirical basis for the claims advanced in Blum et al. (2015, 2016, 2017, 2018, see also Chap. 1 in this volume). The changing social landscape after World War II provided the conditions for the emergence of the field, which in our network analysis is represented by a growth in the number of practitioners and increasing intra-institutional relations. A major move toward a greater connectivity of the network was given by the increasing mobility of early-career researchers who went from one place to another, in this way

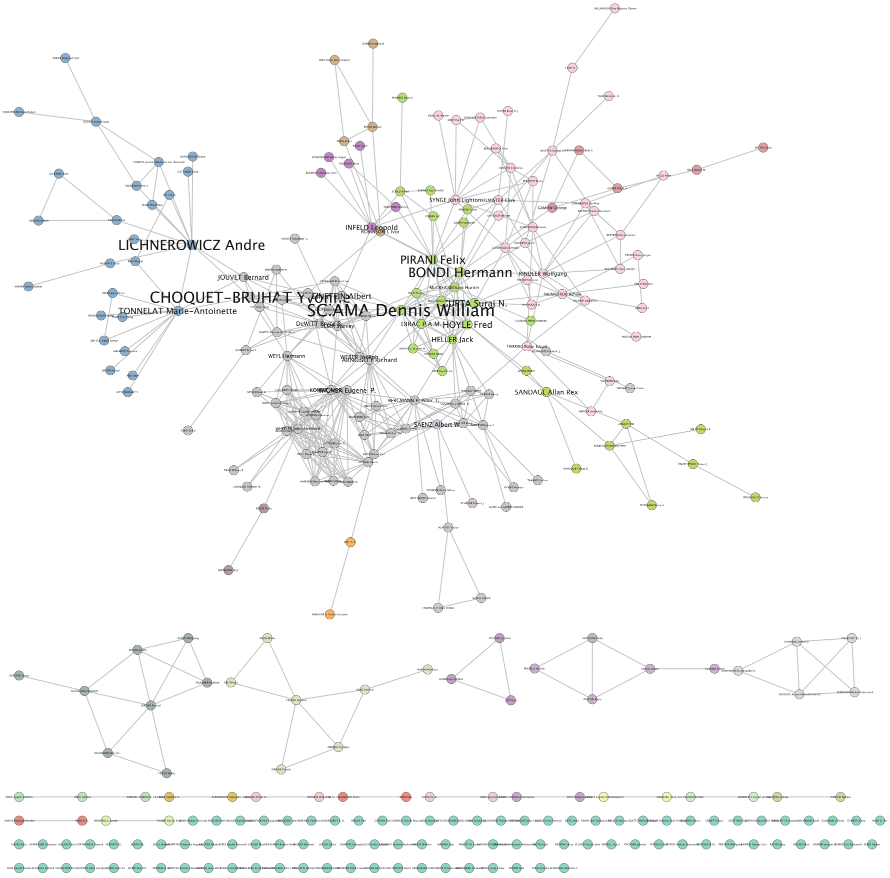

Fig. 8 The *socio-institutional collaboration network* in 1957. Colors identify clusters retrieved with the Girvan-Newman algorithm. All isolated nodes are in light green. Size labels are proportional to betweenness centrality values. Centrality measures and visualization with Visone (Brandes 2001; Brandes and Wagner 2004)

connecting different research centers. One of these early postdoc travelers was Felix Pirani who connects the two largest components of the socio-institutional network in the early 1950s. This change, however, was not sufficient in launching a proper renaissance phenomenon. The analysis of the collaboration network without taking into account the copresence at institution relation shows a later shift, which was realized between 1959 and 1960. In the collaboration network, we see the beginning of a phase of significantly increasing connectivity, interpretable as the starting point of the formation of a giant component in the social sphere, which might imply the formation of a scientific field, a hypothesis we further explored with the analysis of the semiotic and semantic layers.

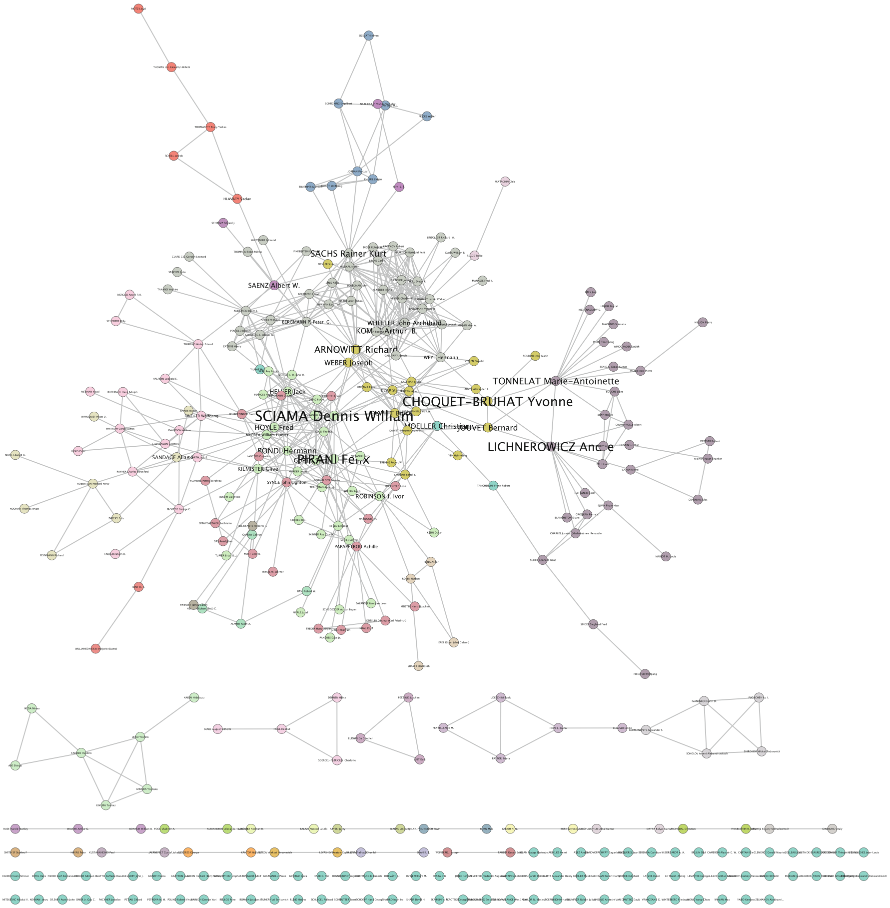

Fig. 9 The *socio-institutional collaboration network* in 1958. Colors identify clusters retrieved with the Girvan-Newman algorithm. All isolated nodes are in light green. Size labels are proportional to betweenness centrality values. Centrality measures and visualization with Visone (Brandes 2001; Brandes and Wagner 2004)

3 The Semiotic Layer: Citation and Co-Citation Analyses of General Relativity Publications, 1915–1975

From the perspective of socio-epistemic networks, a fundamental question is whether and to what extent the change appearing in the social layers of general relativity research appears in the other layers. Said otherwise, whether and to what extent the formation of a giant component in the social sphere was connected to meaningful changes in the semiotic and semantic spheres that could also be interpreted as the formation of a scientific field.

As for the material representation of knowledge production in the scientific literature, the analysis of citation patterns constitutes one of the most frequently employed ways to identify research fields in the twentieth century (de Solla Price 1965; Klavans and Boyack 2017), and are often considered the most clear example of the "information network" (Newman 2003). These approaches have grown tremendously in the last few decades and have become foundational in bibliometric and scientometric studies, also thanks to the availability of data in online repositories such as WoS.

To explore the scientific development of general relativity in terms of citation patterns, we analyzed both citation and co-citation networks, focusing especially on the clustering of co-cited papers, interpreted as a proxy for the taxonomy of a branch of research (Marshakova-Shaikevich 1973; Small 1973; Chen et al. 2010; Hou et al. 2018). Co-citation networks are built by considering two papers as connected if they are cited together in a third paper. The more times papers are cited together, the stronger the connection between these papers is. For historical studies, this method is particularly revealing of knowledge structures because papers are connected at a later stage, and the chronological dynamic of co-citation patterns might reveal the emergence of new research fronts based on intellectual bases constituted by publications appeared in a previous period (Persson 1994; Morris et al. 2003; Chen 2006). In this section, we will use different approaches to the clustering of co-citation networks to determine how research agendas were connected. In addition, we determine important changes interpreted as transformations of the semiotic level of knowledge production.

3.1 The Dataset

We prepared a dataset of papers with two different search strategies, using both citation-based search (Set D) and keyword-based search (Set E). These search strategies, detailed elsewhere (Lalli et al. 2020), are summarized in Table 1.

The *general relativity publication space* (or set F) is the union of sets retrieved using the combined abovementioned search strategies. It includes 8296 WoS items published between 1915 and 1975 (Fig. 10).[10] This set is the basis for the below citation and co-citation analyses.

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[10] We included only items that can be reasonably considered research products: Book reviews, biographical items, and items about individuals have been discarded.

Table 1 Datasets of publications related to general relativity from 1915 to 1975 and indexed in WoS

<table>
<tr><td rowspan="4">Set F

General relativity publication space</td><td rowspan="3">Set D

Einstein's citation space</td><td>Set A
Einstein's papers, 1915–1955</td></tr>
<tr><td>Set B
Papers citing Set A papers, 1915–1975</td></tr>
<tr><td>Set C
Papers citing Set B papers, 1915–1975</td></tr>
<tr><td colspan="2">Set E
Keyword search 1915–1975: "general relativity" OR gravitation* OR "allgemeine Relativitätstheorie" OR "relativité générale" OR "teoria della relatività" OR "Gravité quantique" OR "Gravità quantistica" OR "einheitliche Feldtheorie" OR Quantengravitation OR "champ unifié" OR "unified field" OR "quantum gravity" OR cosmolog* OR Kosmolog*</td></tr>
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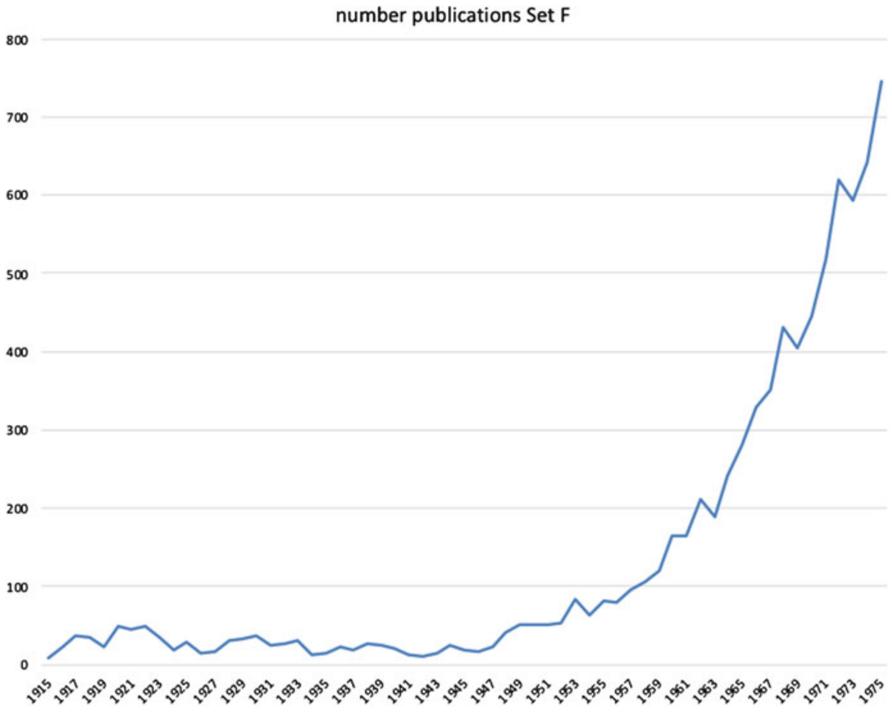

Fig. 10 Number of publications per year in the *general relativity publication space*

3.2 The Citation Network

A first impression of the evolution of the semiotic layer of general relativity might be seen through the visualization of the clustered citation network created using the program CitNetExplorer (van Eck and Waltman 2014). Citation networks are intrinsically historical, or at least chronological (Garfield 2009). Papers almost always cite papers or books that have been published earlier, so that citation links have a natural direction that points to the past. In our analysis, the citation network is used to give a first overview of the field's subdivision in research areas and their historical roots, using a series of criteria and parameters for the selection of cited items to be included in the analysis and visualization.[11] The final set of cited publications retrieved following this methodology includes 3250 items published between 1915 and 1970, which were grouped in clusters using a 1.1 resolution in the community detection embedded in the CitNetExplorer algorithm based on the Girvan-Newman algorithm (Waltman and van Eck 2012). The resolution value is set to find major research areas connected to the field of general relativity rather than for fine-grained research topics. The algorithm identified 9 clusters plus 136 papers that were discarded as not belonging to any cluster.

The 100 items having the highest citation score all belong to the six largest clusters (Fig. 11). The difference in terms of network connections between the low-water-mark phase and the renaissance phase is remarkable. There is a clear difference in inter-cluster connections between papers published in the post-World War II period and those published in the previous decades. The blue cluster is clearly related to cosmology and remains mostly isolated from the other clusters in the pre-World War II years. The other large cluster in the pre-renaissance period is the green one, which might be characterized as the cluster showing a connection between the problem of motion in general relativity, research agendas on the quantization

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[11] In order to be more inclusive we added non-matching references cited more than 5 times in our dataset, namely, cited publications that are not included in our original dataset, for example, books or articles published in journals not indexed by WoS. To reduce the range and diversity, we excluded items that were cited less than 5 times by the papers of our dataset as well as those published before 1915, which returned a total of 4020 items. On the basis of this selection, we looked at possible clusters of papers, understood as proxies of specific research areas. After this stage, we used a relatively high resolution in order to define areas of research more precisely. Criteria used for the clustering algorithm: resolution: 2.00; minimum cluster size: 10; number of random starts: 10; number of iterations: 40; random seed: 1. The community detection algorithm embedded in CitNetExplorer with the abovementioned resolution detected 29 clusters of cited publications, while 324 of them did not belong to any cluster. We then excluded the items not belonging to any cluster as well as two smaller clusters that were only slightly connected with the largest connected component of the citation network of general relativity research. We have excluded from the visualization the cluster of 48 papers connected to the research area in solid state physics concerning the employment of non-Riemannian geometry for the study of dislocations in crystal initiated in (Bilby et al. 1955), as this research area was disconnected from the other clusters; although, interestingly, it also emerged in the mid-1950s renaissance period of general relativity. The second removed cluster was on Earth's gravitation field measurements.

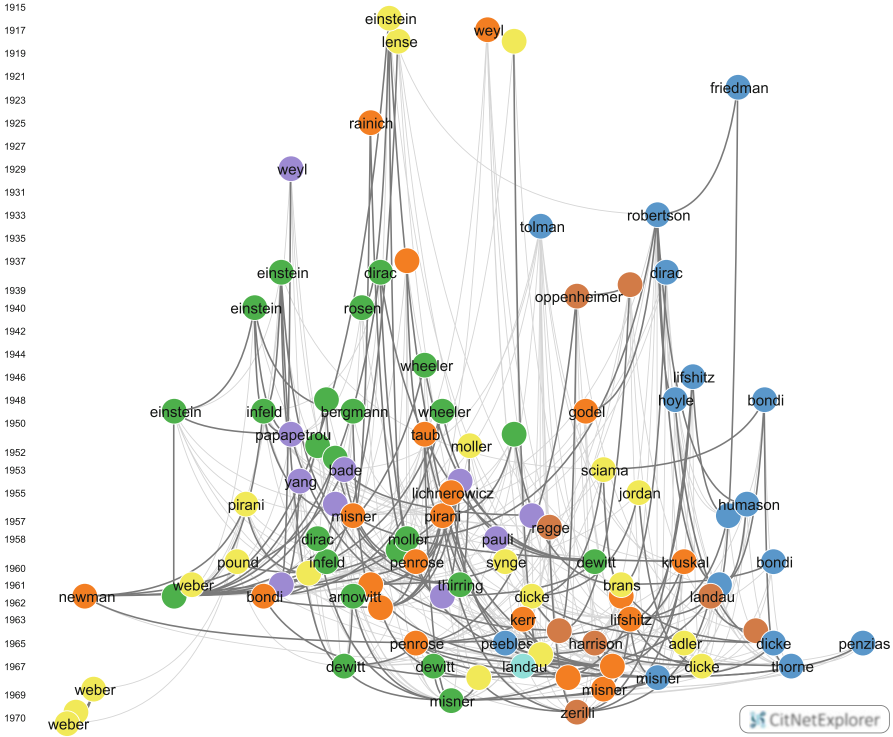

Fig. 11 Citation network of the 100 publications with the highest citation scores published between 1915 and 1970. Colors represent different clusters

of gravitation, and papers on quantum field theories. Figure 11 makes evident the role of the Einstein-Infeld-Hoffmann paper on the equations of motion in general relativity (EIH), and also of a contemporary paper by Dirac on the classical theory of radiating electrons at that time considered unrelated to general relativity (Dirac 1938b; Einstein et al. 1938). The cluster emerges as one of the major research areas in the aftermath of World War II, when it starts including research agendas aiming at quantizing general relativity (Bergmann 1949; Gupta 1952b) as well as the Wheeler-Feynman absorber theory of electrodynamics (Wheeler and Feynman 1949). In later years, the papers on the Arnowitt-Deser-Misner (ADM) formalism and deWitt's papers on the quantization of gravitation figure prominently in this cluster.

In Fig. 11, around the mid-1950s the green cluster is displaced by papers belonging to the purple cluster, which contains publications on the theoretical unification of fundamental forces, mostly electromagnetism and gravitation and, from the 1950s, nuclear forces. When it emerges as a major cluster in the 1950s, the most cited papers of the purple cluster concern especially mathematical tools, such as spinors and the non-Abelian Yang-Mills theory (Bade and Jehle 1953; Yang and Mills 1954).

The yellow cluster is mostly connected to possible observational tests of the theory and starts with Einstein's 1916 paper on the foundations of his theory of gravitation (Einstein 1916). This cluster really emerges only in the 1950s and included a variety of different approaches: the experimental program on gravitational waves starting with Pirani's 1956 paper that would soon be interpreted as a way to measure gravitational ways (Pirani 1956; see also Saulson 2011), the alternative scalar-tensor theories of Jordan and Brans-Dicke (for a history of these theories, see Goenner 2012), the publications of Dicke's group in Princeton (see Peebles 2017) as well as of Leonard Schiff's at Stanford, and the Pound-Rebka experiment on gravitational red-shift (Pound and Rebka 1959). The orange cluster includes theoretical work on classical general relativity, with special reference to gravitational waves (e.g., Einstein and Rosen 1937; Pirani 1957; Bondi et al. 1962), the Schwarzschild singularity (Kruskal 1960; Kerr 1963), the initial value problem of general relativity (Lichnerowicz 1955), and the work on geometrodynamics by Wheeler and his group (Misner and Wheeler 1957). This cluster becomes a set of more connected cited items only immediately after the mid-1950s. The last visible cluster also starts before World War II with the papers by Oppenheimer and co-authors on compact stellar objects and gravitational collapse (Oppenheimer and Snyder 1939; Oppenheimer and Volkoff 1939; for historical analyses see Bonolis 2017; Almeida 2020). This cluster re-emerges again in the early 1960s, clearly as a consequence of the discovery of quasars.

The image suggests that different major branches of research connected to general relativity became more integrated between the late 1950s and the early 1960s. In that period, the green cluster mostly connected to research on the quantization of gravitation merges with the cluster on the theoretical predictions and implications of general relativity proper (orange), which in turn merges with the cluster on the observations of general relativity predictions (especially through Pirani's 1956 and 1957 papers). This last cluster in turn merges with the cosmology cluster a little bit later, and possibly in connection with the proposal of the Brans-Dicke theory (Brans and Dicke 1961), or, most probably, with the discovery of cosmic microwave background radiation (Dicke et al. 1965; Penzias and Wilson 1965).

This representation of the semiotic layer conveys a separation between different branches of research that was overcome only after the mid-1950s. In a first phase, there is a shift toward the theoretical predictions of general relativity merging, first, with observational research and, later, after the early 1960s, with physical cosmology and astrophysics. Monographs such as (Tolman 1934; Synge 1960; Landau and Lifshitz 1962) emerge as very relevant in the citation network as they are among the items with the highest citation scores. Less relevant, in quantitative terms, appear other general treatises, such as (Landau and Lifshitz 1951; Møller 1952; Pauli 1958; Fock 1959). To understand the role of such textbooks or general introductory texts in connecting previously separated parts of the network, we eliminated all these treatises from the analysis. The general historical picture remains essentially the same, apart from a greater relevance to individual papers in connecting different clusters.

As Fig. 11 visualizes clusters among only the 100 most cited papers, the outcome is strongly conditioned by the late rapid increase in the total number of papers with respect to the previous period (see Fig. 10). Given the well-known mechanism that papers tend to cite recently published papers (de Solla Price 1965), the sudden increase of publications starting from the 1950s leads to an overrepresentation of papers published after World War II. In other words, what we see in Fig. 11 is a representation of the general relativity research field from the late 1950s onward. As for the papers published before this period, only those most relevant to the fields dominant in the renaissance phase, are visible. This is shown by the absence, for example, of Arthur Eddington's books and papers, as well as by the total absence of papers from the research field on the unification of gravitation and electromagnetism before and after World War II.[12]

To investigate the evolution of the field in the low-water-mark phase, we have then limited our study to the citation network of items cited by papers published between 1915 and 1954 (Fig. 12).[13] We clusterized the network of the retrieved 1034 cited items by using the same criteria as in the previous network. In spite of the smaller number of nodes (about one third), we retrieved almost the same number of clusters (8 rather than 9). One third of the publications (294) did not belong to any of these clusters, which already gives an idea of the dispersion of the field in the pre-renaissance stage. By limiting the analysis to the 771 publications effectively belonging to identified clusters, the separation between fields appears quite strong except for indirect citations (second-order citations in light gray) and textbooks such as those by Eddington (1924) and Tolman (1934). The separation between clusters is very evident in Fig. 12, which shows the 100 publications with highest citation scores (omitting the two above-mentioned books).

The field of cosmology (green) is the most active in the low-water-mark period, but remains highly disconnected from all the other branches of general relativity research. Connection appears in the immediate post-World War II period with a different cluster (blue) that is, in fact, still related to cosmological research. The blue cluster captures early Big Bang cosmological models and their connections to nuclear physics, including topics such as the abundance of elements and stellar energy (Unsöld 1948; Gamow 1949; Alpher and Herman 1950). The third largest cluster (purple) is very sparse and regards mostly quantum electrodynamics and its possible extension to gravitation. In this cluster, the papers more connected to general relativity relate to the topic of the equations of motions in the fast-motion approximation scheme (Havas 1989). Its role as the connecting cluster between the others is uniquely due to the presence of very broad review articles. The cluster concerning a branch of classical unified theory of gravitation and electromagnetism (orange), based on the Kaluza-Klein theory and the introduction of a fifth dimension

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[12] They are part of the purple cluster, but did not belong to most cited 100 papers.

[13] Since the numbers of citations are much less than in the previous analysis, we have kept a very low threshold concerning the number of publication: 1 citation for non-matching entries, and 2 citation score.

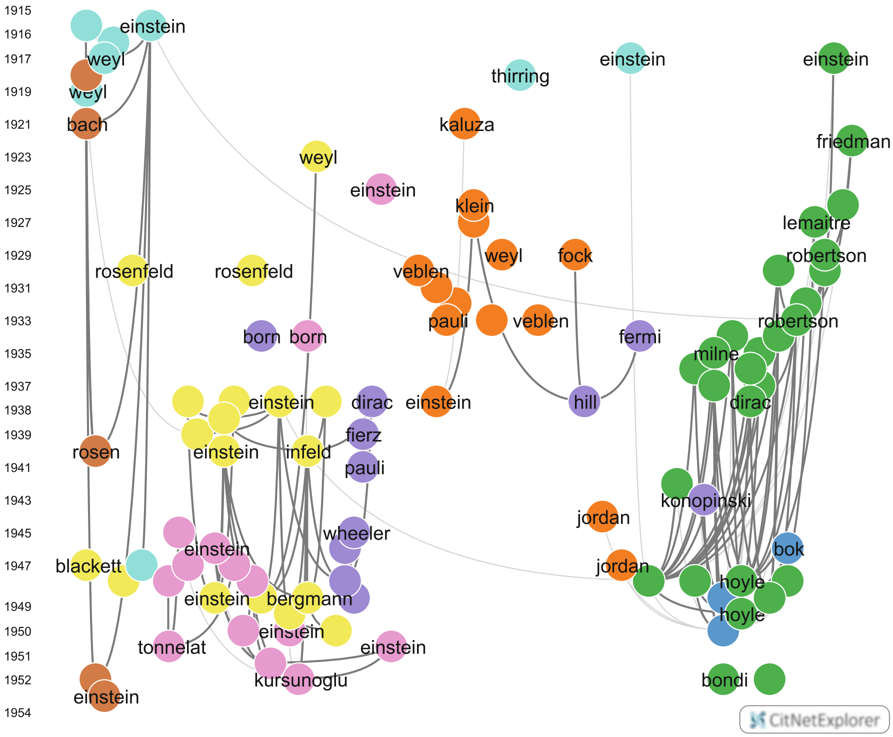

Fig. 12 Citation network of the *general relativity publication space* between 1915 and 1954. The figure shows the 100 publications with the highest citation scores excluding textbooks. Colors represent different clusters

(Kaluza 1921; Klein 1926), remains largely isolated from the other clusters and without further historical development in the period under consideration (for a historical study and bibliography, see, Goenner 2004).

The yellow cluster shows the strong connection between the EIH equation of motion and the branches of research related to the quantization of gravity, as well as the growing connection with the purple cluster, especially after World War II. At the time, a new cluster emerges (pink) that concerns a novel approach, the metric affine one, to the classical (unquantized) unified theory of gravitation and electromagnetism (e.g., Einstein 1945; Schrödinger 1947, for a historical overview of these theories see Goenner 2014).

The light blue cluster concerns general relativity proper, its physical predictions, and implications, including the Schwarzschild solution and other solutions of Einstein's gravitational equations. As is clearly shown in Fig. 12, such a cluster is essentially unnoticeable in the low-water-mark period. In fact, it is a cluster connecting papers in the pre-low-water-mark period (before the mid-1920s), and a few scattered contributions in the following years. The last cluster (brown) is also on general relativity, with a focus on the problem of energy. It shows a similar pattern to the light blue cluster. All papers concerning gravitational waves, that in some cases still belong to this brown cluster, are also absent from this picture of the 100 most cited items, confirming the scant impact of this research strand on major research agendas in general relativity in the low-water-mark phase.

Our analysis of the citation network suggests a change in the way previous knowledge products were re-connected at a time of increasing publication outputs. Since the mid-1950s, research strands that had been separate, or with very limited connection, in the previous decades started to connect with each other. By connecting these results to our previous analysis of the social network, one might draw a qualitative similarity, reinforcing the view that the period between the late 1950s and the early 1960s was particularly relevant in a reconfiguration of knowledge, giving a precise input on the knowledge dynamics related to social dynamics. The first reconfiguration is represented by a return of interest to the theoretical predictions of general relativity (orange cluster in Fig. 11), connected with quantum gravity *avant la lettre* (green) and experimental testing of general relativity physical predictions (yellow cluster in Fig. 11). The second reconfiguration, occurring after the early 1960s, regards the merging of cosmology (blue) and astrophysical issues, including stellar collapse (brown).

3.3 The Dynamics of Co-Citation Networks

The second approach we pursued was to analyze the dynamic of the co-citation network of the *general relativity publication space* (set F). Our strategy was to divide our corpus in temporal slices between 1925 and 1970 and compare the parameters of the retrieved networks with the program VOSviewer (van Eck and Waltman 2010).[14] The major methodological problem with this is that the number of published papers and cited references vary enormously within the entire period of the analysis, making it difficult to define meaningful terms of comparison.[15] Still, we wished to compare these highly diversified periods in order to understand differences between the research agendas related to general relativity in different periods. To make the analysis feasible, co-citation networks in the low-water-mark and renaissance phases were created using different selection rules and thresholds resulting in co-citation networks of roughly similar dimension.

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[14] The total number of cited references for the entire period is 78,936 items.

[15] To give an idea of the quantitative difference, the cited references in a 30-year period between 1926 and 1955 are only 6949, while the number of cited references of the 5-year period 1966–1970 is 21,745. In the same periods, the number of references that meet the threshold of 3 citations is only 398 for the first 30-year period, while it is 1975 for the second one.

3.3.1 Period 1926–1940

The original idea of dividing the timespan of our analysis in equal 5-year long slices was, however, unfeasible for the pre-World War I period of the low-water-mark phase. The number of cited references was simply too small to give any meaningful result. We had to content ourselves with giving a more general illustration of this phase by investigating the longer 15-year period between 1926 and 1940.[16] We also had to use a very low threshold for the number of citations (two), which gave only 140 items in the largest connected component, which we hypothesize to be the core semiotic space of general relativity in the period 1926–1940. With such a small number of items, one can of course expect a great dispersion of the co-cited network, as is confirmed by Fig. 13.

The VOSviewer clustering algorithm groups these items into 8 clusters (van Eck and Waltman 2017). The largest cluster (red) includes 23 items on the five-dimensional unified field theory of gravitational and electromagnetism, strongly based on the Kaluza-Klein theory, which is connected, on one side, with the yellow cluster and, on the opposite side, with the blue cluster and a few unclustered items (gray). The yellow cluster is very sparse, and its main topic is not easily identifiable. It includes research on Einstein's field equations and their solutions, such as the Schwarzschild metric (Schwarzschild 1916), as well as other approaches to a unified field theory of matter and electromagnetism (e.g., Born and Infeld 1934). The blue cluster seems to be more coherent and internally connected: It concerns primarily unified field theoretical approaches based on gauge transformations starting with (Weyl 1929).

On the opposite side of the red cluster on unified field theory, one finds the second largest cluster (green), which includes 24 items on relativistic cosmology, from the first paper by Einstein on the cosmological implications of his theory of relativity (Einstein 1917) up to the development of evolving universe models in the early 1930s. The green cluster appears much more internally connected than other clusters and has significant connections with neighboring clusters. These neighboring clusters also contain papers with cosmological considerations: the cluster on Milne's kinematic theory and its connection with the development of the Friedmann-Lemaître-Robertson-Walker (FLRW) metric (light orange); Tolman's papers on the thermodynamics of general relativity (light blue); and a more varied (purple) mid-1920s cluster that includes topics such as cosmological implications of extra-galactic nebulae, Dirac's and Jordan's large number hypothesis cosmological considerations as well as the first attempt at quantizing gravitation (Rosenfeld 1930).[17]

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[16] During this period, 2029 references were cited by the papers in our dataset, but only 170 papers met the threshold of two citations.

[17] The small brown cluster does not seem to be easily identifiable with a specific research topic, but mostly concerns tensor analysis.

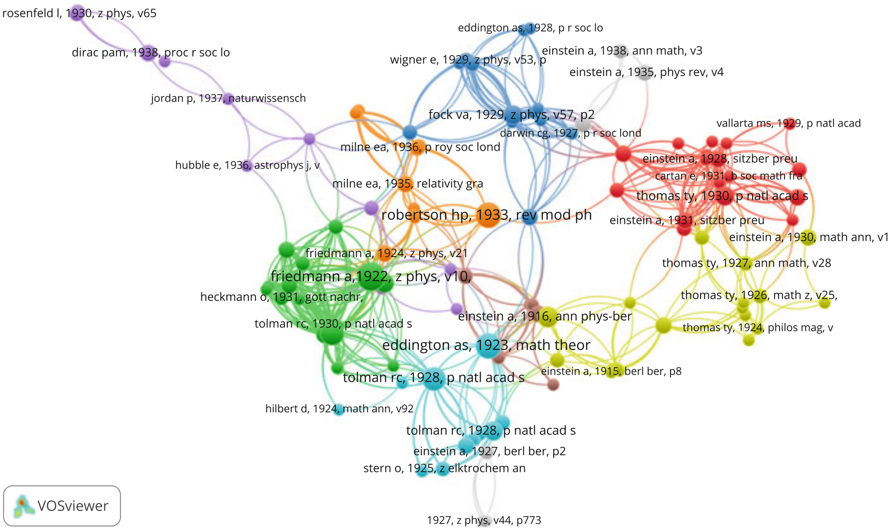

Fig. 13 Co-citation network of the *general relativity publication space,* 1926–1940. Colors represent different clusters. The size of the nodes and of the labels are proportional to the number of citations in the explored subset

The co-citation network in the period 1915–1945 is very similar, confirming that the period during World War II did not change the research arena in general relativity in any significant way.[18] It is to be acknowledged that the low dimension of the network strongly impacts the reliability of the results. Also because of the low number of records, we were forced to take a long timespan, which brings into doubt the co-citation network as the representation of the intellectual base of dynamic research fronts.[19] We also expect that the method of retrieval (WoS) might be considerably biased for this early period, while it might be more robust starting from the post-World War II period. Even with these reservations, the illustration provided by the present analysis is informative enough to conclude that during the pre-World War II phase, research on general relativity was essentially divided along two major branches: cosmology and unified field theory with very little connections between each other and even with significant dispersion between different approaches within these main areas of investigation.

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[18] 2403 references, of which 225 meet the threshold of two citations. 171 papers in the largest components divided in 9 clusters, which largely correspond to those of the period 1915–1940.

[19] We have tested the 5-year periods (1926–1930, 1931–1935, 1936–1940, and 1941–1945). All of these periods have less than 55 papers (as the maximum) meeting the threshold of two citations. Of these, more than 20 percent did not belong to the largest connected component. Furthermore, textbooks (Eddington 1924, Tolman 1934) and reviews (Robertson 1933) had a fundamental relevance in connecting different parts of the largest components.

3.3.2 Structural Changes in the Co-Citation Network, 1945–1975

Meaningful quantitative changes occur instead soon after World War II. The number of publications and cited references (at least those available in WoS) increases significantly and allows for comparison between consecutive 5-year periods. We assume such 5-year periods to be an acceptable choice for investigating historically developing research fronts and exploring the co-citation networks as the intellectual bases of these research fronts. As one might expect, the number of cited references changes dramatically between periods in the timespan of our analysis. In order to have a meaningful number of references in the first two periods, we needed to fix a very low threshold for the number of citations, namely, two. Using the same threshold in the first two periods (1946–1950 and 1951–1955) results in having double as many cited references in the second period with respect to the first one (395 versus 214). As for the following periods, we chose the threshold in order to keep the number of cited references meeting the threshold comparable with that of the period 1951–1955 (see Table 2). By using these criteria, the comparison between the 5-year co-citation networks in the period 1946–970 becomes feasible and meaningful.[20]

In all cases, we limited the analysis to the largest connected components, which we assume to represent the core of the intellectual base of the research fronts on topics related to general relativity. In the majority of cases, the number of references included in the largest connected component nearly corresponds to the totality of the cited references meeting the chosen threshold, being always greater than 97% and

Table 2 Cited references and thresholds used for the analysis of the 5-year co-citation networks in 1946–1970

	1946–1950	1951–1955	1956–1960	1961–1965	1966–1970	1971–1975
Cited references	1672	3292	11,024	11,941	21,733	36,111
Meet threshold 2	214	395	991	2035	3915	6578
Used threshold	2	2	3	5	8	11
Meet the used threshold	214	395	387	379	391	407
Largest component percentage	98.1%	94.7%	98.7%	97.1%	100%	100%

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[20] Parameters used for the visualization: attraction: 10; repulsion: 1. Parameters used in the clustering algorithm: resolution: 1.20; random starts: 100; iteration: 40; random seeds: 1; minimum cluster size: 5.

Table 3 Largest component's parameters of the general relativity co-citation network in the six 5-year periods between 1946 and 1970. The weighted degree takes into account multiple connections between node pairs, while these connections are merged in the degree measures (Kleinberg 1999; Brandes 2001)

	1946–1950	1951–1955	1956–1960	1961–1965	1966–1970	1971–1975
Nodes	210	374	382	368	391	407
Edges	5052	4893	6147	7805	15,915	14,397
Average degree	48.114	26.166	32.183	42.418	81.407	70.747
Average weighted degree	63.41	33.914	44.67	71.489	154.169	148.339
Diameter	6	6	5	5	3	3
Density	0.23	0.07	0.084	0.116	0.209	0.174
Modularity	0.254	0.603	0.472	0.531	0.325	0.429
Average clustering coefficient	0.791	0.699	0.6	0.516	0.537	0.488
Average path length	2.419	2.69	2.204	2.112	1.839	1.855
Clusters	6	13	11	9	8	8

at times equal to 100%. The only significant difference occurs in the period 1951–1955, where it is a little less than 95%.

The study of the network parameters over these six periods uncovers structural changes in the network (see Table 3). The largest components change radically in the first three periods, while they stabilize in the last two (1966–1970 and 1971–1975), in which the networks also show considerable signs of small-world behavior.[21] This suggests, first, that the semiotic level of research on general relativity changes considerably between 1946 and 1970, and, second, that this transformation is not a radical or sudden reconfiguration, for example, after reaching a tipping point, but presents varied dynamics.

Surprisingly, in the first period immediately after World War II, the semiotic representation of general relativity research reaches a relatively high density, high-average weighted degree, and high clustering coefficient. The study of the internal structure of the network based on the hypothesis of a subdivision in communities—or in our case, hypothetical research fields—also provides unexpected results. In applying algorithms for the identification of clusters of co-cited papers, the value of the modularity provides a measure of how well a network is compartmentalized in sub-networks (or communities). The low modularity of the 1946–1950 network can then be interpreted as research in this period being non-compartmentalized, where the possible sub-networks representing research areas are significantly connected to each other (Blondel et al. 2008).

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[21] See the small diameter and short average path length of the network in the last two periods. The density of the network is also considerably greater than the previous periods.

Such a situation changes radically in the 1951–1955 period. This period is characterized by a large growth in the number of papers and citations (see the number of nodes meeting the threshold). This growth is accompanied by a large dispersion in the semiotic layer of the research field. This is shown by the density, which is much lower than that in 1946–1950, as well as by the modularity and the number of clusters, which are both much higher than in the previous period.[22] The dynamic implied by these values is that of a disordered growth in many different and disconnected research branches.

The period 1956–1960 sees this trend of disordered expansion changed and reverted. This period sees an extreme increase in the number of cited references and in the number of edges in the largest connected component. This network has, compared with the 1951–1955 network, a smaller number of clusters and a lower modularity, which implies a stronger connection between the various sub-networks retrieved with the clustering algorithm. The visual representation of the network in these three periods and their clusters (represented by different colors) conveys the formation of a strong connected core between 1956 and 1960, in addition to a few smaller clusters representing active research areas rather marginal to this core group (see Fig. 14).[23]

These results confirm and extend the result of our analysis of the social layer (section 2). The early 1950s was a period of growth in research related to general relativity, most plausibly related to the general growth of physics in the same period. The analysis of the social layer showed that there was an institutionally based increase of research in topics related to general relativity (section 2.2). But, just as the growth of nodes did not lead to a reconfiguration of the structure in the largest connected component in the social layer, the co-citation network shows that there was a great dispersion of such material representations of knowledge, too. According to this quantitative result, one hypothesizes that the first half of the 1950s was characterized by the presence of various research branches weakly connected to each other. This dispersion was overcome only after the mid-1950s. Our analysis of the semiotic layer confirms the view that the second half of the 1950s was a particularly important moment in the field of general relativity, as in this period the growth of the field coalesced around a core of research topics (see section 4 for their content).

In many ways, the parameters of the following period (1961–1965) show a continuation of the trend initiated previously, in terms of greater weighted average

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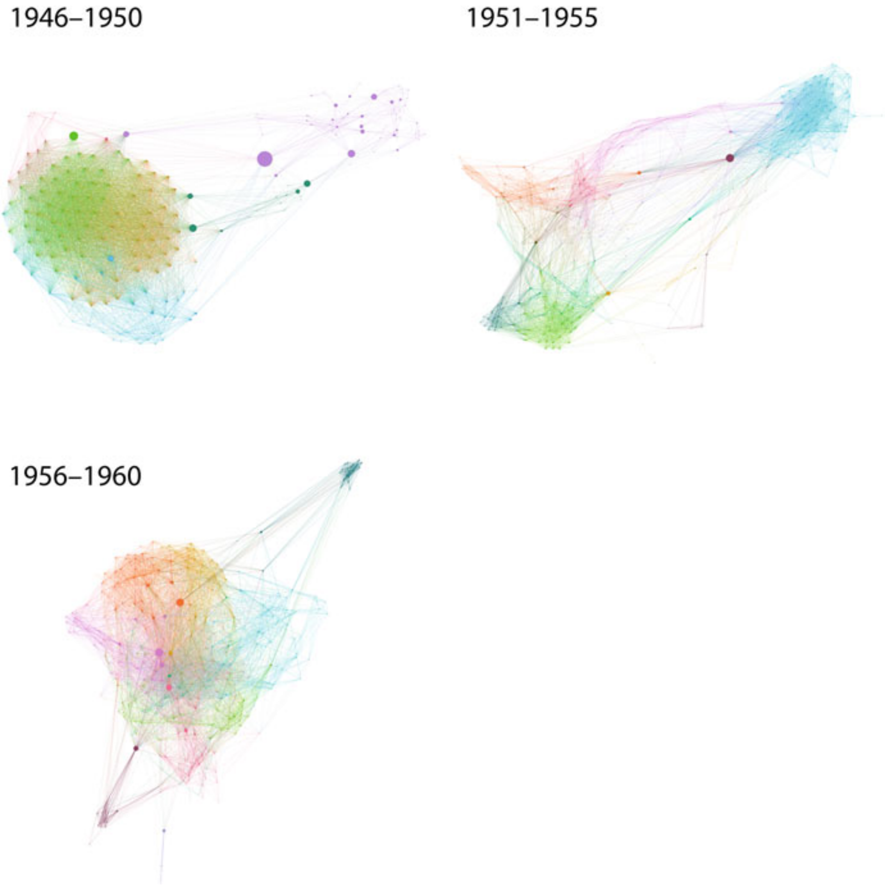
[22] This might of course be expected given the increase in the number of nodes. However, we tested the behavior of the 1951–1955 co-citation network using a higher threshold for citations (3 citations), which resulted in a network with 168 items. In spite of the fact that the number of nodes were significantly lower than in the previous period, the values indicating the cohesiveness of the field and its internal structure clearly indicate much greater dispersion with respect to the previous period (clusters: 7, density: 0.12, modularity: 0.588).

[23] While the content of this network will be discussed in Sect. 4.3, we can anticipate that the peripheral clusters in 1956–1960 in Figure 14 have only a very weak connection with the field of general relativity.



1946–1950

1951–1955

1956–1960

Fig. 14 Structures of the co-citation networks of the *general relativity publication space* in 1946–1950, 1951–1955, and 1956–1960. Colors represent different clusters. The size of nodes is proportional to betweenness centrality. Centrality measures and visualization with Gephi (Bastian et al. 2009)

degree and density as well as the decrease of average path length and number of clusters. The major element that indicates a disruption of the trend is the modularity increase, which shows a greater division (and separation) in identifiable sub-communities of co-cited papers, or research areas.

The following period shows, instead, a second important historical transformation, with a strong decrease in diameter and average path length as well as a significant increase in density and weighted average degree. In spite of the greater number of nodes, the number of clusters decreases with respect to the earlier period. The modularity in the 1966–1970 network is also significantly lower, signaling a greater cohesiveness of the entire connected component within one unique macro-research area. The following period 1971–1975 presents essentially the same features, which signal the stabilization of the general relativity semiotic structure, although with some signs of further modification, such as a better subdivision of communities (higher modularity and lower density).

The semiotic network layers in the six periods of our analysis offer the image of a very dynamic research area with clear moment of passages between 1946 and 1975. Our results powerfully indicate that we need a much better chronological description of the transformations in general relativity research than a simple binary division between the low-water-mark phase and the renaissance phase. Our quantitative analysis suggests, instead, that there was a first renaissance phase after the mid-1950s in which a greater cohesiveness of the field was achieved. After the early 1960s, there was a second phase with the formation of a more compact field with features similar to small-world networks. We hypothesize, then, that in the 1960s a second radical passage in the development of research in general relativity occurred, following and distinct from the previous renaissance phase, which we limit to the period between the second half of the 1950s and the early 1960s. Our methodological choice based on 5-year periods does not allow for a definition of a more granular chronology and neat historical passages in a specific year as our analysis of the social network dynamics did. But the division of the post-mid-1950s renaissance phase in two major periods appears in an evident manner, as it appears evident the dynamic transformation of the last part of the low-water-mark phase.

4 The Semio-Semantic Layer of General Relativity, 1946–1975: A Formal Approach to Combining the Co-Citation Network with Topic Analysis

To investigate the semantic layer, we identified the central topics of the clusters of co-cited papers in the different periods, as well as the positions, in terms of centrality measures, of specific papers in the co-citation network. To define the topics of the clusters we used a formal approach combining our informed knowledge of articles' scientific content with quantitative methods based on the calculation of eigenvector centrality in topic networks extracted from the full-texts of the cited papers.[24] This research is based on a subset of 935 co-cited publications where we could obtain full-texts. This set covers on average 41% of the co-cited items for each time period. We OCRed[25] all these texts and used grobid[26] to create a structured text, which

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[24] Eigenvector centrality is a measure of how influential a node is in the network, giving relevance to the number of edges of each node (degree centrality) combined with how much a node is connected with other high-degree nodes (see, e.g., Newman 2010, 154–156). The table with the 1010 most central words for each cluster in each 5-year period is available at hdl.handle.net/21.11103/dataverse.XIVXCW.

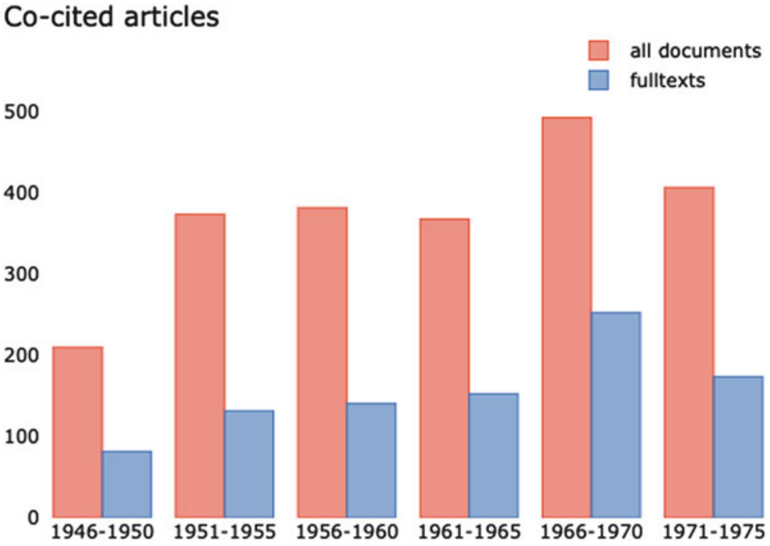
[25] We used the Abbyy recognition server for OCR.

[26] 'GROBID - GeneRation Of BIbliographic Data'. Accessed 15 December 2019. https://grobid.readthedocs.io/en/latest/Introduction/.

Table 4 Number of retrieved full-texts compared with the total number of selected papers per 5-year period

Periods	All documents	Full-texts	percentage
1946–1950	210	82	39%
1951–1955	374	132	35%
1956–1960	382	141	37%
1961–1965	368	153	42%
1966–1970	493	253	51%
1971–1975	407	174	43%
Total	2024	935	46%

separates abstracts and full-texts from additional information such as the references. The texts were processed with textacy (removing punctuation and hyphenation, and normalizing the text with Unicode equivalence), which was employed to create the semantic network using only word tags (noun or proper noun).[27] This formal methodology combining quantitative and qualitative approaches allows us to track the links between the semiotic layer of co-cited papers and the semantic network made by topic contents in the co-citation network (Table 4).



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[27] More about textacy at 'Textacy: NLP, before and after SpaCy — Textacy 0.9.1 Documentation'. https://chartbeat-labs.github.io/textacy/. Accessed 15 December 2019. As a base for the network, we merged all texts that are part of one cluster and applied textacy's function *terms_to_semantic_network*. Links are created if they are in a window of 10 words. The script we used can be found at https://gitlab.gwdg.de/MPIWG/BZML/exoplanets/toolsfortextmining/blob/master/Analyse Papers.py.

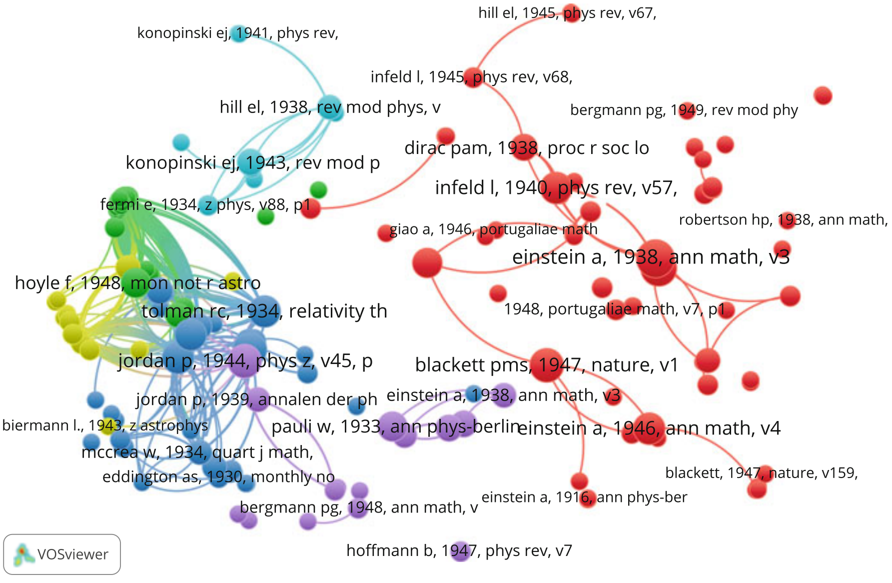

Fig. 15 Co-citation network of the *general relativity publication space,* 1946–1950. Colors represent different clusters. The size of nodes and labels is proportional to the number of citations in the explored subset

4.1 Period 1946–1950

We have seen that the first post-World War II period is characterized by a relatively high density and cohesiveness of the field (Table 3). The clustering algorithm employed identifies only 6 clusters (see Fig. 15).[28] The largest cluster (red) contains 68 items that represent the bulk of papers related to general relativity, as confirmed by the fact that Einstein's papers in this network are all but one contained in this cluster. Many items concern, however, the extension of classical and quantum field theories both in gravitation and electrodynamics, such as the Wheeler-Feynman absorber theory (Wheeler and Feynman 1945, 1949) and Bergmann's canonical approach to the quantization of general relativity (Bergmann 1949; Bergmann and Brunings 1949) as well as quantum electrodynamics (Dirac 1938b; Schwinger 1948, 1949).

Particularly relevant in this cluster is the problem of motion in general relativity in post-Newtonian formalism and its extension to electrodynamics, starting with the EIH paper (Einstein et al. 1938) and including a series of contributions by Einstein, Infeld, Robertson, and others (Robertson 1938; Infeld and Wallace 1940; Einstein

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[28] The dataset of publications contained in all clusters for all periods of our analysis is available at hdl.handle.net/21.11103/dataverse.XIVXCW.

Table 5 Research areas represented by the retrieved clusters in Fig. 15. As modularity in this period was very low (see Table 3), we should expect this representation to be strongly approximate

Cluster	Research area
1—red	Relations between general relativity and relativistic quantum field theory/quantization of gravitation/problem of motion in GR
2—green	Cosmology/nuclear physics/abundance of elements
3—blue	Pre-World War II relativistic cosmology/alternative cosmological models
4—yellow	Astrophysics/nucleo-synthesis/stellar energy/cosmic rays
5—purple	Higher-dimensional unified field theory (UFT)/Kaluza-Klein type
6—light blue	Nuclear theory

and Infeld 1949). This set of quite diverse papers seems to constitute the intellectual base of the attempts to theoretically unify general relativity and quantum mechanics, which was at the time a major research area within the yet small and marginal field of general relativity research (Table 5).

This largest cluster is strongly separated from the other ones. The three other major clusters are much more connected to each other and are, in fact, about cosmology and related topics. The second largest cluster (green) contains theoretical papers on the two major models of the universe developed in the second half of the 1940s: the theory by Gamow, Alpher, and Herman that was later called the Big Bang theory, and Hoyle's version of the steady-state cosmology (Gamow 1948, 1949; Hoyle 1948; Alpher and Herman 1950). The cluster also contains various papers on the cosmological implications of the relative abundance of elements and advances in nuclear theory. Strongly connected with this cluster is the yellow cluster, which instead contains especially papers on astrophysics from a previous period, from the late 1930s to the early 1940s, including the nucleo-synthesis of chemical elements (Chandrasekhar and Henrich 1942), the production of stellar energy (Gamow and Teller 1939; Zwicky 1940) and the origin of cosmic rays (Fermi 1949).

More distinct from these two clusters that are clearly based on the connection between cosmology, astrophysics, and nuclear physics, one finds the blue cluster. It is also on cosmology, but is more concerned with alternative approaches less connected with astrophysics. These include mostly pre-World War II works on relativistic cosmology, the development of the FLRW metric, and their connections with Hubble's observations (Robertson 1933; Hubble and Tolman 1935; Lemaître 1997). It also includes alternative approaches, most prominently, Bondi and Gold's version of the steady-state cosmological model (Bondi and Gold 1948), Milne's kinematical theory (Milne 1935), and Dirac's large number hypothesis (Dirac 1938a).

The fifth cluster (purple) is on higher-dimensional unified field theory of gravitation and electromagnetism based on the Kaluza-Klein approach including Einstein and his collaborators' works on the subject (Kaluza 1921; Klein 1926; Einstein and Bergmann 1938; Bergmann 1948) as well as work on projective relativity (Veblen 1933; Jordan 1947; Hoffmann 1948). The last and very small cluster (light blue) is instead about nuclear theory and is mostly connected with the green cluster on the Big Bang cosmology.

The semantic analysis of this network elucidates the meaning of the increase of research on general relativity in the aftermath of World War II. This effect is due to new fields entering the scene, which are strongly connected to the development of nuclear theory, especially in astrophysics, and its implications for a theory of the universe. In this sense, this effect has little to do with the previous low-water-mark phase research areas, which are relegated to just two clusters (the blue on relativistic cosmological theories and the purple on unified theories of gravitation and electromagnetism). While the red cluster is also related to general relativity and has its roots in the later pre-World War II period, it shows the emergence of a new research direction. This is related to the intersection of quantum mechanics and general relativity and is deeply shaped by the development of quantum electrodynamics and the possibilities offered by the EIH equations of motion. Keeping in mind that this representation is due to the choice of a small threshold for the number of citations, what one finds is the image of a small and low-impact field that is, moreover, strongly unbalanced because of the relevance of physics topics not directly concerned with general relativity theory.

A comparison between the list of the ten most central papers using two different centrality measures (betweenness and closeness) offers further insights to the content of this network (Table 6). The two lists are remarkably different. Only three items maintain high centrality in both measures: Pauli's reviews on relativistic field theories of elementary particles, Tolman's textbook on relativity, and a paper by Jordan on his cosmological and astrophysical implementation of Dirac's large number hypothesis (Tolman 1934; Pauli 1941; Jordan 1944). This gives an indication of the role of general publications such as textbooks and reviews in the connectedness of the network and of its thematic contents. The other publications with high betweenness centrality are to be interpreted as the brokers of the semiotic network, as they connect the different subfields identified by the

Table 6 The ten most central papers in the 1946–1950 co-citation network using the closeness and betweenness centrality measures. For each publication, the cluster to which it belongs is shown

Betweenness	Closeness
(Pauli 1941)—red cluster	(Tolman 1934)—blue cluster
(Tolman 1938)—green cluster	(Dirac 1937)—blue cluster
(Jordan 1944)—purple cluster	(Hubble 1937)—blue cluster
(Blackett 1947)—red cluster	(Pauli 1941)—red cluster
(Veblen 1933)—purple cluster	(Jordan 1944)—purple cluster
(Tolman 1934)—blue cluster	(Fermi 1949)—light blue cluster
(Infeld and Wallace 1940)—red cluster	(Hoyle 1948)—green cluster
(Tolman 1949)—red cluster	(Weizsäcker 1938)—yellow cluster
(Jordan 1947)—purple cluster	(Alfvén 1937)—yellow cluster
(Klein 1926)—purple cluster	(Spitzer 1946)—yellow cluster

clustering algorithm. The large majority of these papers belong to the red and the purple clusters. The high number of papers with high betweenness in the red cluster does not indicate only that the cluster was a connecting research area, but is also a result of the diversity of topics *within* the cluster itself. As for the purple cluster, the high number of papers among the most central papers depends on the fact that the field of unified theory of gravitation and electromagnetism is related to other research areas by very few publications. These are Klein's version of the Kaluza-Klein theory and Veblen's treatise on projective relativity, as well as Jordan's papers on cosmological theory following Dirac's large number hypothesis, which connects the purple cluster with the three clusters on cosmology (Klein 1926; Veblen 1933; Jordan 1944, 1947). Tolman's treatise on statistical mechanics (Tolman 1938) also has a very high betweenness centrality, as it connects the green cluster, to which it belongs, with all other clusters, except the cluster on unified theories of electromagnetism and gravitations. The measures of the closeness centrality give a completely different picture. The publications on cosmology appear as particularly central, with a strong role for some publications in the blue cluster on non-Big Bang approaches to cosmology and observational research with cosmological implications.

This analysis uncovers a development within the low-water-mark phase under-recognized by previous historical reconstructions: the entrance of nuclear theoretical developments in the resurgence of interest in the theory. The analysis of the next periods, however, shows that this did not have immediate implications for the so-called renaissance process.

4.2 Period 1951–1955

The first half of the 1950s saw an increase of the number of published papers (albeit not particularly dramatic) and, consequently, of the number of cited papers. The network of publications in the largest connected component is much higher than in the previous period and is divided in many more clusters (Fig. 16). The range of themes is also considerably different than that of the previous period. The largest cluster (red) is composed of 72 items, all of them connected to cosmology. It contains both cosmological models developed in the late 1940s (Big Bang theory and steady-state models) as well as alternative models and previous work on the topic (e.g., Milne 1935; Hubble 1937; Jordan 1944). Strongly connected to this cluster one finds a very small cluster (sand) on non-relativistic cosmological models. Also connected to the red cluster, there is the blue cluster composed of 40 items weakly connected with each other that concerns, on the one hand, cosmological approaches inspired by Dirac's large number hypothesis (Dirac 1937)—such as Jordan's gravitation theory with varying gravitational constant (Jordan 1952; Just 1954)—and, on the other hand, papers on the quantization of gravitational equations (Gupta 1952a; Bergmann and Schiller 1953). Works on nuclear physics and astrophysics characterizing the previous period in its connection to cosmological papers disappear altogether if not for a few papers in the red cluster (Table 7).

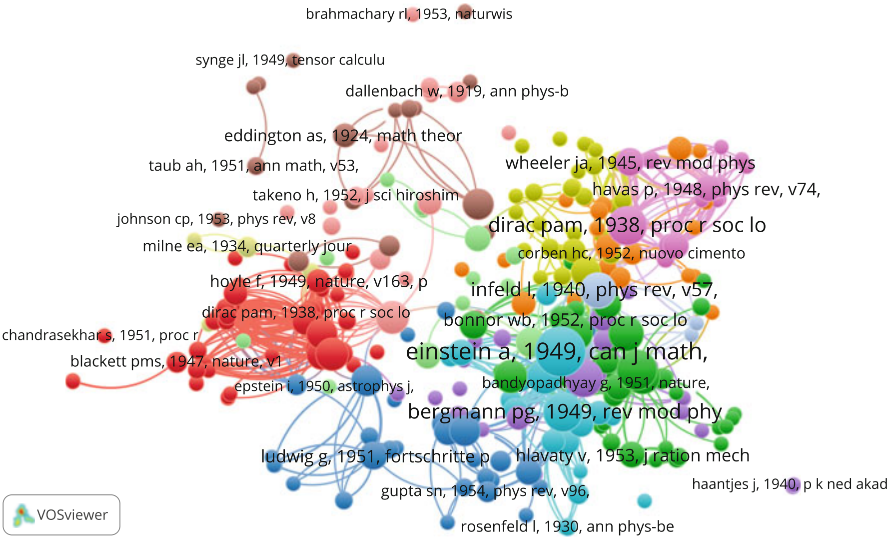

Fig. 16 Co-citation network of the *general relativity publication space,* 1951–1955. Colors represent different clusters. The size of nodes and labels is proportional to the number of citations in the explored subset

Table 7 Research areas represented by the retrieved clusters in Fig. 16. As modularity in this period was relatively high (see Table 3), we should expect the clustering to be effective in identifying sub-research areas

Cluster	Research area
1—red	Cosmology
2—green	Affine UFT
3—blue	Large number hypothesis in cosmology/quantization of gravitation
4—yellow	Quantum field theory/quantum electrodynamics
5—purple	Higher-dimensional Kaluza-Klein UFT
6—light blue	Equations of motion in GR/quantization of gravitation
7—orange	Group theoretical particle physics/representations Lorentz group
8—brown	Lense-Thirring effect/Taub solution
9—pink	Equations of motion in field theories (Wheeler-Feynman approach)
10—light pink	Specific solutions in cosmology/general literature
11—light green	Equations of motion in field theories
12—gray	Equations of motion in GR
13—sand	Non-relativistic cosmological models

The second largest cluster is on the opposite side of the network with respect to the cosmology cluster. It is the green cluster composed of 49 publications mostly concerning unified affine theories of electromagnetism and gravitation developed in the late 1940s and early 1950s including Einstein's, Schroedinger's, Tonnelat's, and Hlavatý's approaches (Schrödinger 1947; Tonnelat 1951; Hlavatý 1952; Kursunoglu

1952; see Goenner 2014 for a review). Between this and the blue cluster on the quantization of gravitation is a cluster (light blue) containing papers on the equations of motion in general relativity (Einstein and Infeld 1949; Papapetrou 1951) and approaches to the quantization of gravitation (Bergmann and Brunings 1949; Pirani and Schild 1950) as well as papers relevant to this research agenda (especially, Dirac 1950). The purple cluster is mostly about higher-dimensional approach to the unified theory of electromagnetism and gravitation based on the Kaluza-Klein theory. The yellow cluster is, instead, clearly connected to quantum field theory and quantum electrodynamics with little explicit connection to gravitation, while the orange cluster seems to include group theoretical approaches to elementary interactions, with a special focus on the representations of the Lorentz group. Close to this, one finds the pink cluster mostly on the equations of motions in field theories, especially within the framework of the Wheeler-Feynman absorber theory, which is very similar to the topics characterizing the gray and light green clusters.

Between these two conglomerates, one finds some minor clusters (brown and light pink) that are both more focused on general relativity proper. The brown cluster is related to theoretical considerations on general relativity, including the Lense-Thirring effect and Taub’s solution, while the light pink cluster contains various publications on general relativity, especially of a general nature, such as textbooks and reviews (Robertson 1933; Møller 1952), but also connected to cosmology and special solutions (Gödel 1949; Takeno 1951).

The general picture provided by this analysis is that in the early 1950s the field was growing in disparate directions. Work on general relativity proper in this period was strongly marginal, relegated to two very small clusters (brown and light pink). The bulk of the research was in cosmology—albeit with different and sometimes incompatible, or at least unconnected in terms of co-citation patterns, approaches—and, on the other, on the quantization of gravitation and unified theory of gravitation and electromagnetism. Especially relevant was the work on the equations of motion that had relations with both agendas as well as with electrodynamics and theories of particle interactions, which both figure prominently in this network. This network clearly represents a shift with respect to the previous period, due to an enormous growth of approaches informed by the advances in quantum field theory and with little resemblance to the 1946–1950 network.

The analysis of centrality measures confirms that some deep changes occur in this period. Consistent with our analysis of clusters’ topics, the betweenness centrality measures show that papers on the equations of motions, including the EIH paper, become relevant brokers between different research agendas, which otherwise look quite separated from each other (Table 8). Tolman’s (1934) textbook also figures prominently as the publication with highest centrality, both betweenness and closeness, which indicates its relevance in keeping the field together in this phase characterized by high dispersion. Another important aspect in this dispersed network is the centrality, both betweenness and closeness, of Eddington’s (1946) controversial theory on the fundamental unification of elementary interactions as well as of Bergmann and Brunings’s review (1949) of non-linear field theories. Jordan’s alternative cosmological theory (1952) has also a high betweenness

Table 8 The most central items using the betweenness and closeness centrality measures in the co-citation network 1951–1955. For each publication, the cluster to which it belongs is shown

Betweenness	Closeness
(Tolman 1934)—light pink cluster	(Tolman 1934)—light pink cluster
(Weyl 1917)—brown cluster	(Einstein 1951)—green cluster
(Einstein 1951)—green cluster	(Einstein and Infeld 1949)—light blue cluster
(Eddington 1946)—orange cluster	(Infeld and Wallace 1940)—gray cluster
(Infeld and Wallace 1940)—gray cluster	(Eddington 1946)—orange cluster
(Jordan 1952)—blue cluster	(Einstein and Infeld 1940)—light blue cluster
(Einstein and Infeld 1949)—light blue cluster	(Infeld 1938)—gray cluster
(Bergmann and Brunings 1949)—light blue cluster	(Bergmann and Brunings 1949)—light blue cluster
(Einstein et al. 1938)—purple cluster	(Einstein et al. 1938)—purple cluster
(Dirac 1938a, 1938b)—pink cluster	(Weyl 1917)—brown cluster

centrality. These results strongly convey the sense of a dispersed field where work on relativistic cosmology, though active, was not central, while connections in the network were created by alternative and controversial approaches, and above all, by the equations of motion that could be useful for a variety of research agendas. Finally, one notices the increasing relevance of Einstein's own publications in this network with respect to the previous one, where his name was essentially absent. The high centrality of Einstein's publications might indicate the relevance of the new editions of the book *The Meaning of Relati*vity (Einstein 1951) on this reconfiguration of research agendas in this period, as well as the disappearance from the picture of agendas less connected with general relativity characterizing the period 1946–1950 (for a historical analysis of Einstein's text see Gutfreund and Renn 2017). We interpret this disruption as a demonstration of the low influence of nuclear theory and Big Bang cosmology in the 1950s development of research related to general relativity.

4.3 Period 1956–1960

Figure 17 shows the formation of a large core of connected papers to which are related three small and much more peripheral clusters. The algorithm identifies 11 clusters, eight of which belong to the core that formed in this period. The topic of the largest cluster (red) is difficult to identify, as it deals with a variety of subjects, and the cluster is also the more marginal within the core group of papers. The majority of the papers concern quantum field theoretical approaches to particle physics and, in some cases, the extension to gravitation. The second largest cluster (green) is instead purely related to general relativity and represents

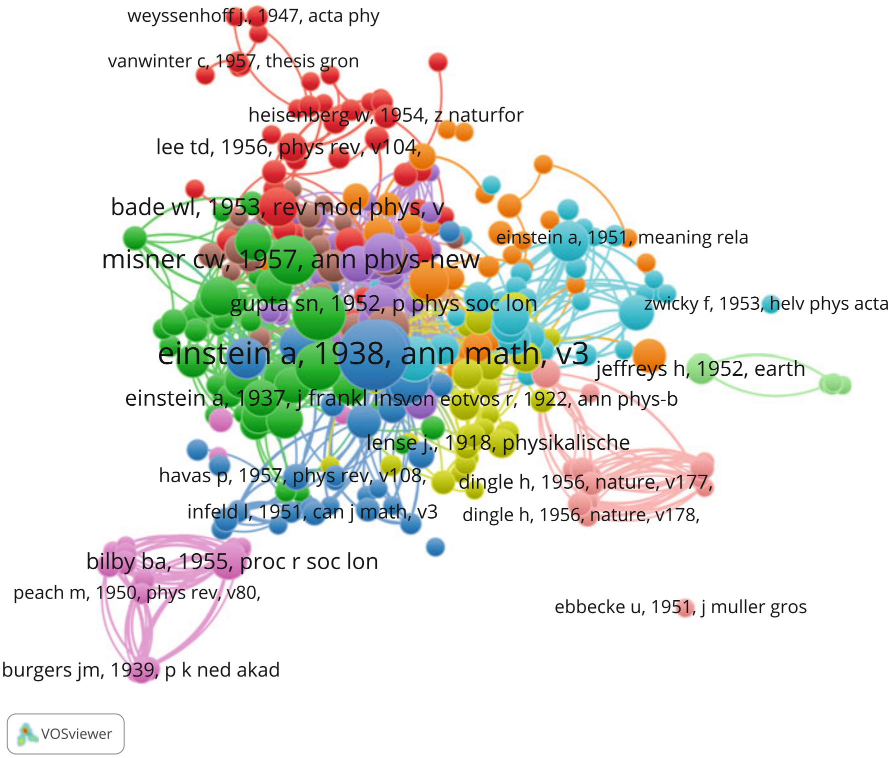

Fig. 17 Co-citation network of the *general relativity publication space,* 1956–1960. Colors represent different clusters. The size of nodes and labels is proportional to the number of citations in the explored subset

a momentous change. In this cluster, the majority of papers deal with theoretical issues concerning general relativity proper, the most relevant of which concern gravitational waves (e.g., Pirani 1956, 1957) and their connections with a variety of studies on general relativity including the initial value problem (Lichnerowicz 1955), conservation laws (Goldberg 1958), ADM formalism (Arnowitt et al. 1959), and the geometrodynamic approach of Misner and Wheeler (Misner and Wheeler 1957). As a confirmation of the interpretation of this cluster as the representation of the return of focus to general relativity proper, (Einstein 1916) is part of this cluster (Table 9).

The blue cluster is also strongly related to general relativity proper, as it is composed, almost uniquely, of papers concerning the equations of motion in general relativity and gravitational damping, as well as their connections with the issue of the existence of gravitational waves. The yellow cluster is connected to general relativity proper as well, and it might be considered the cluster on the proposed testing of general relativistic effects in connection with alternative theories considered in terms of predicted observational effects (e.g., Birkhoff 1943;

Table 9 Research areas represented by the retrieved clusters in Fig. 17. As modularity in this period was lower than in 1951–1955 (see Table 3), we should expect the clustering to be less effective in clearly identifying sub-research areas

Cluster	Research area
1—red	Relations between GR and relativistic quantum field theory
2—green	Theory gravitational waves/initial value problem/conservation laws/ADM formalism/geometrodynamics
3—blue	Equations of motion in GR
4—yellow	Proposal tests in GR/alternative theories for testing
5—purple	Quantization of gravitation
6—light blue	Non-Big Bang cosmology
7—orange	Affine UFT
8—brown	Initial value problem in GR/space-time singularity
9—pink	Crystal dislocation with non-Riemannian geometries
10—light pink	Twin paradox
11—light green	Earth gravitational field

Clemence 1947; Corinaldesi et al. 1951; Synge 1952; Dicke 1957). The purple cluster represents the research area on the quantization of gravitation, centered especially on the papers by Peter Bergmann and Suraj N. Gupta, as well as Dirac's paper on Hamiltonian dynamics (Bergmann and Brunings 1949; Dirac 1950; Gupta 1952a, b; Bergmann and Schiller 1953). The light blue cluster is about cosmology and includes the steady-state theory, Jordan's cosmology (Jordan 1952), and papers by Robertson and Walker on the FLRW metric (Robertson 1935; Walker 1937). Big Bang cosmology, which dominated co-citation networks in the previous periods, is totally absent from this cluster, as well as from the entire network. The orange cluster represents instead the research area on affine unified theory of gravitation and electromagnetism in the approaches by Einstein, Schroedinger, and Marie-Antoinette Tonnelat, but even in this case, there are papers not immediately related to this general theme. The brown cluster seems to contain especially papers on the initial value problem in general relativity and also includes work on space-time singularities.

The three last, smallest, clusters are in the periphery of the network and are neatly separated from the network's core. In agreement with this structural position, all are about topics that are marginal in the renaissance phenomenon characterizing the central part of the clusters. These three clusters are also more easily identifiable as a representation of specific topics, such as the employment of non-Riemannian geometry for the study of dislocations in crystals (pink cluster), the controversy about the twin paradox in relativity theories (light pink), and the gravitational field of the earth (light green).

This analysis of cluster topics clearly shows a thematic shift occurring in the second half of the 1950s. Not only did issues that were completely absent in the previous two periods—such as gravitational waves and the initial value problem—become topics of entire clusters of papers, but also those parts that were

Table 10 The ten most central items using the betweenness and closeness centrality measures in the co-citation network 1956–1960. For each publication, the cluster to which it belongs is shown

Betweenness	Closeness
(Einstein et al. 1938)—blue cluster	(Einstein et al. 1938)—blue cluster
(Landau and Lifshitz 1951)— green cluster	(Einstein and Infeld 1949)—blue cluster
(Tolman 1934)—light blue cluster	(Infeld and Schild 1949)—brown cluster
(Einstein and Infeld 1949)—blue cluster	(Tolman 1934)—light blue cluster
(Bondi 1952)—light blue cluster	(Einstein and Infeld 1940)— blue cluster
(Infeld and Schild 1949)—brown cluster	(Bergmann and Brunings 1949)—purple cluster
(Pirani 1957)—green cluster	(Einstein 1953)—orange cluster
(Misner and Wheeler 1957)—green cluster	(Bergmann and Schiller 1953)—purple cluster
(Einstein 1953)— orange cluster	(Dirac 1950)—purple cluster
(Lichnerowicz 1955)—green cluster	(Landau and Lifshitz 1951)—green cluster

already present in the previous two periods underwent a major reconfiguration. The three research agendas aiming at going beyond general relativity (quantization of gravitation, affine unified theory, and cosmology) became so intrinsically connected to the topics of classical general relativity that papers on one topic are easily found in clusters different from the one representing their topic. Moreover, the field of cosmology was significantly changed from the previous period, with the complete absence of the post-World War II papers on Big Bang cosmology and its interrelation with nuclear physics. In this sense, the steady-state theory tradition seems to be clearly the research agenda in cosmology that actually participated in the renaissance process, both in interrelation to the other contemporary agendas and to the pre-World War II development of relativistic cosmology. Finally, what we see is essentially a theoretical reconfiguration, as also the cluster with observational themes (yellow) contains above all papers that provided theoretical predictions of physical phenomena within general relativity in comparison with alternative theories.

The analysis of most central papers also presents clear and meaningful changes with respect to the previous period (Table 10).[29] The EIH paper on the equations of motion becomes the most central paper in all centrality measures, signaling the relevance of the problem of motion as the unifying theme between the various research agendas that appeared in this 5-year period. Using the closeness centrality measures, this relevance is even stronger as four of the most central papers are about equations of motions (three of which in the blue cluster). More interesting novelties appear, however, in relation to new papers that assume a relatively high betweenness

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[29] In Table 10 we have not included the peripheral clusters 9, 10, and 11. These clusters were not strongly related to the field of general relativity and appear as a parallel phenomenon. Their presence would have had a great impact on the betweenness centrality measures, as these clusters were connected to the center of the network through very few papers. Excluding them gives a more precise idea of the relevance of papers in connecting the areas of research more related to the renaissance phenomenon.

centrality value, indicating the emergence of new fields. These are Pirani's paper on the invariant formulation of gravitational radiation (Pirani 1957) and Misner and Wheeler's paper on geometrodynamics (Misner and Wheeler 1957). Pirani's paper clearly emerges as an important contribution in the field of gravitational radiation (green cluster), and it also connects significantly with the cluster on the equations of motion (blue), the cluster on the initial value problem (brown), and, less strongly, the cluster on the quantization of gravitation (purple). The position of (Misner and Wheeler 1957) in the network shows a similar picture, but with more connections with the cluster on quantum field theory (red) and a much stronger connection with the cluster on the quantization of gravitation (purple). Significantly, both papers are, instead, not connected to the field of cosmology (light blue) and the cluster on affine unified theories (orange). These measures show how the topic of gravitational waves (green cluster) becomes the connecting element between different, but increasingly close, research agendas, which are also more and more connected to general relativity proper. The closeness centrality measure, instead, identifies the research papers on the quantization of gravitation as the most central to the core together with papers and books on the equations of motion. The centrality measures indicate that new books were the connecting elements between different research areas, either on specific topics such as Bondi's book on cosmology (Bondi 1952) and Lichnerowicz's book on the initial value problem in general relativity (Lichnerowicz 1955), or more generally, such as the English translation of the extremely influential treatise on the classical theories of fields by Landau and Lifshitz (1951).

New papers, new books, and new research areas came to dominate the field of general relativity in the period 1956–1960. This process was not simply a return of interest to general relativity proper creating a more interconnected core in the semiotic network. It also showed a significant degree of variation between this core and the areas previously related to general relativity, such as cosmology, unified theory of gravitation and electromagnetism, and, more recently, the quantization of gravitation. It clearly appears that the research area on the quantization of gravity was still important in this period in shaping the field of general relativity tout court, albeit now more strongly interconnected with those general relativity problems that needed to be solved to pursue the quantization programs. Other areas, such as cosmology and unified theory of electromagnetism and gravitation, while still part of the picture and of the central core, were evidently less central to the main approaches dominating this period.

4.4 Period 1961–1965

The research arena in the following period shows ten different clusters (Fig. 18). It is in some respect a solidification of the process evident in the previous period, as there is an even clearer focalization on general relativity issues. The largest cluster (red) is fully related to general relativity proper and concerns primarily theoretical work on gravitational waves (e.g., Sachs 1961; Bondi et al. 1962). This

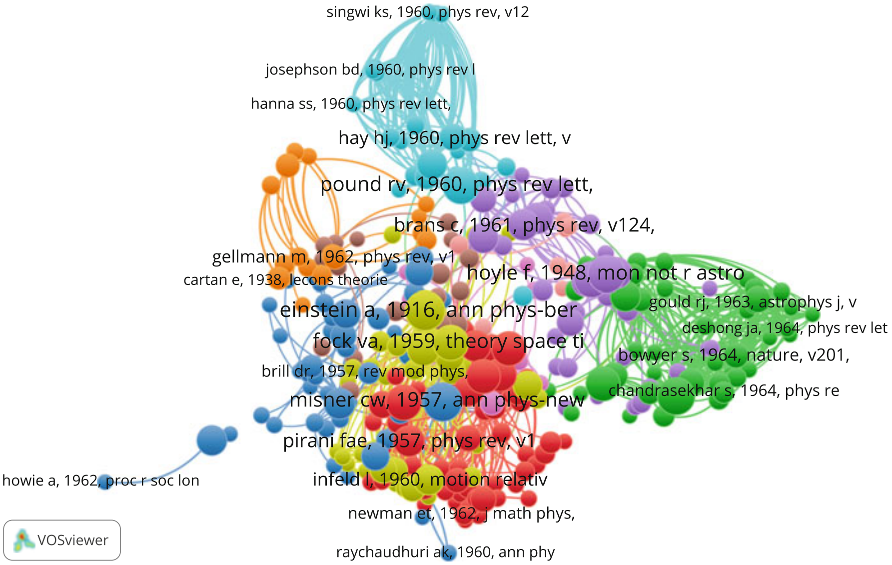

Fig. 18 Co-citation network of the *general relativity publication space*, 1961–1965. Colors represent different clusters. The size of nodes and labels is proportional to the number of citations in the explored subset

cluster is very much integrated with the yellow cluster representing research on the problem of motion in general relativity. Also close to these two clusters one finds the blue cluster, which contains a variety of different papers whose main topic might be identified in the relation between gravitation and particle physics, including Misner-Wheeler geometrodynamics (Misner and Wheeler 1957), the Yang-Mills theory (Yang and Mills 1954), and Utiyama's gauge-invariant unified theory of all interactions (Utiyama 1956). The small brown cluster is also connected to the general relativity theory core composed of the red and yellow clusters. It contains alternative theories of gravitation including attempts to unify general relativity and quantum mechanics (Gupta 1952b; Bergmann 1956; Yilmaz 1958) plus the experimental program put forward by Leonard Schiff (Schiff 1960), which in turn is strongly related to the small cluster (light pink) that includes theoretical papers on the Lense-Thirring effect in cosmology (Table 11).

The other clusters are sensibly less connected to this core and, in some cases, weakly connected between each other. The orange cluster is related to the theory of strong interactions, especially with group theoretical approaches, and is, unsurprisingly, prevalently connected with the blue cluster. Particularly central appears the purple cluster, which is the cosmology cluster and includes papers on steady-state cosmology, the Brans-Dicke theory (Brans and Dicke 1961), other alternative cosmological theories as well as Dicke's papers on observational tests in cosmology (Dicke 1962a). This cluster might be understood as the initial, still weak, core of what would soon become observational cosmology. Connected to

Table 11 Research areas represented by the retrieved clusters in Fig. 18. As modularity in this period was relatively high (see Table 3), we should expect the clustering to be quite effective in individuating sub-research areas

Cluster	Research area
1—red	Theory gravitational waves
2—green	Relativistic astrophysics
3—blue	Relations between general relativity and relativistic quantum field theory
4—yellow	Problem of motion in GR
5—purple	Cosmology
6—light blue	Pound-Rebka-type experiments
7—orange	Strong interactions
8—brown	Alternative theories of gravitation/Schiff experimental program
9—pink	Quantization of gravitation
10—light pink	Theoretical Lense-Thirring effect in cosmology

this cluster, but not with each other, are the large green cluster and, on the other side, the light blue cluster. The green cluster is the representation of the emerging field of relativistic astrophysics sparked mainly by the discovery of quasars in 1963. Particularly central appear the 1939 works by Oppenheimer and co-authors on stellar collapse as well papers launching the field of relativistic astrophysics in 1964 (Chiu 1964; Hoyle et al. 1964). The light blue cluster is instead on experimental tests of general relativity based on the Mossbauer effect and dominated by the Pound-Rebka terrestrial experiments on the gravitational red-shift (Pound and Rebka 1959).

In terms of research agendas, the period 1961–1965 is different from the previous period, albeit not in the same dramatic way in which the period 1956–1960 is different from the previous two. There is a solidification of the research areas that came to dominate the field in the previous period, but, to the other side (quite literally in Fig. 18), one finds emerging trends not present in the previous period: the cluster on relativistic astrophysics, the reformulation of cosmology in closer connection with observational tests, and new tests of the theory.

The analysis of the most central papers, especially using betweenness centrality, makes this change appear more neatly (Table 12). (Einstein 1916) becomes extremely central, which might be considered a clear confirmation of the return of general relativity in its original formulation at the center of interest of an increasingly large community. This centrality is most probably connected with the emergence of observational research on general relativity (see also the centrality of the Pound-Rebka experiment). The other important transformation in this period is the emergence of relativistic astrophysics, which is emphasized by the high betweenness centrality of pre-World War II research on stellar compact objects and collapse (Oppenheimer and Snyder 1939; Oppenheimer and Volkoff 1939) as well as of more recent work on the space-time singularity (Kruskal 1960). The papers by Oppenheimer and colleagues are particularly relevant in connecting the relativistic astrophysics cluster with the red cluster on theoretical work on general relativity proper and gravitational waves, the purple cluster on cosmology, and the brown

Table 12 The ten most central items using betweenness and closeness centrality measures in the co-citation network 1961–1965. For each publication, the cluster to which it belongs is shown

Betweenness	Closeness
(Fock 1959)—yellow cluster	(Fock 1959)—yellow cluster
(Einstein 1916)—yellow cluster	(Landau and Lifshitz 1951)—red cluster
(Oppenheimer and Snyder 1939)—green cluster	(Einstein 1916)—yellow cluster
(Papapetrou and Peierls 1951)—brown cluster	(Arnowitt et al. 1961)—red cluster
(Kruskal 1960)—red cluster	(Misner and Wheeler 1957)—blue cluster
(Pound and Rebka 1960)—light blue cluster	(Einstein et al. 1938)—yellow cluster
(Landau and Lifshitz 1951)—red cluster	(Kruskal 1960)—red cluster
(Oppenheimer and Volkoff 1939)—green cluster	(Møller 1958)—red cluster
(Tolman 1934)—yellow cluster	(Wheeler 1962)—red cluster
(Misner and Wheeler 1957)—blue cluster	(Synge 1960)—red cluster

cluster on alternative theories of gravitation. Kruskal's paper also plays the role of a semio-semantic broker between general relativity proper and astrophysics (red and green clusters), and it also has relevance in connecting the yellow cluster on the equations of motion, and the orange one on group theoretical approaches to strong interactions.[30] The red cluster on general relativity proper is the most central in terms of closeness centrality, with papers on the definition of energy in general relativity, but the yellow cluster on the problem of motion also appears prominently in both centrality measures, indicating the relevance of this topic for the various research areas that were taking shape at the time.

4.5 Period 1966–1970

Table 3 suggests that the period 1966–1970 was characterized by a greater transformation, which is confirmed and clarified through a scrutiny of the themes related to the eight different clusters of co-cited publications (Fig. 19). There was a momentous reconfiguration of the semio-semantic network, understood as a set of connected research agendas, especially evident when compared with the period 1955–1960. The field more related to general relativity proper is still the largest one (red), but it includes different agendas that were previously divided in different clusters. In fact, it contains most of the works that we consider the core of the theoretical renaissance process after the mid-1950s, on gravitational waves, on the equations of motion, and on the quantization of Einstein's field equations. Connected to this cluster, there is the yellow cluster, which is mostly related to

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[30] The high centrality of the book (Fock 1959) is certainly meaningful and it deserves further scrutiny. A quick survey leads to the conclusion that it was particularly central in connecting the red cluster on gravitational waves, the yellow cluster on the equations of motions, the purple cluster on cosmology, and the brown cluster on alternative theories of gravitation.

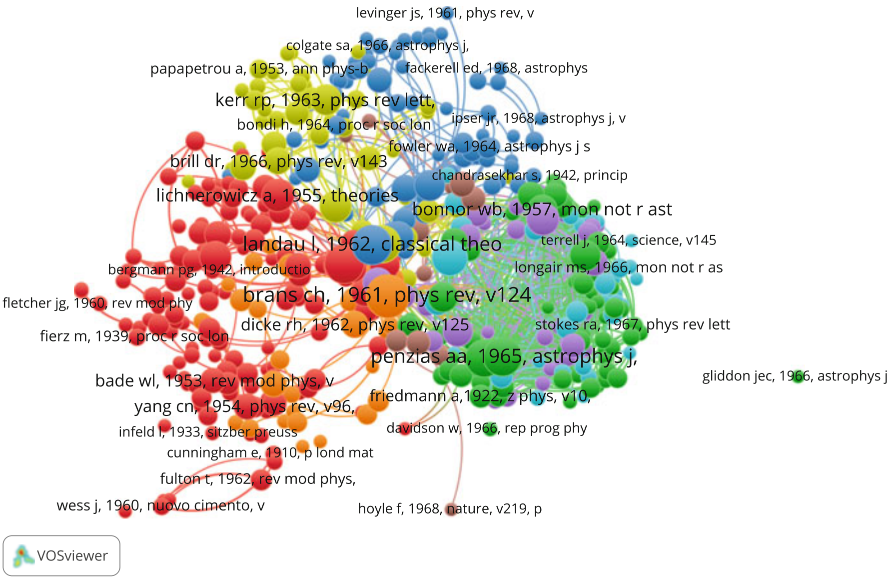

Fig. 19 Co-citation network of the *general relativity publication space,* 1966–1970. Colors represent different clusters. The sizes of nodes and labels is proportional to the number of citations in the explored subset

theoretical research on the Schwarzschild solution and on the problem of the space-time singularity.

The core of the new network is, however, Big Bang cosmology. The second largest cluster (green) concerns the discovery of the Cosmic Microwave Background radiation (CMBR) and its interpretation as an empirical confirmation in favor of the Big Bang cosmological model against the steady-state theory (Dicke et al. 1965; Penzias and Wilson 1965). Strongly interconnected with this cluster, one finds the purple cluster, which contains theoretical papers on Big Bang cosmological models, and the light blue cluster, which is instead more concerned with other astrophysical observations that have implications for the evolving universe models. Less integrated, but still connected to this group of clusters on the evolving universe model, one finds the smaller brown cluster, which concerns primarily the general relativistic treatment of gas and its implications in astrophysics and cosmology (Table 13).

The blue cluster is just between the set of clusters on Big Bang cosmology and the yellow cluster concerning theoretical work on the space-time singularity. It is related to relativistic astrophysics, with special focus on stellar collapse. The last cluster (orange) also appears in an intermediate position between larger groups, namely, between the set of clusters on Big Bang cosmology and the large red cluster on theoretical general relativity. This cluster is clearly connected to the experimental

Table 13 Research areas represented by the retrieved clusters in Fig. 19. As modularity was low (see Table 3), we should expect the clustering not to be highly precise in clearly individuating sub-research areas

Cluster	Research area
1—red	Gravitational waves/equations of motion/quantization of GR
2—green	CMBR and Big Bang cosmology
3—blue	Relativistic astrophysics
4—yellow	Space-time singularity/Schwarzschild solution
5—purple	Theory Big Bang cosmology
6—light blue	Astrophysical observations with implications in Big Bang cosmology
7—orange	Observational and experimental tests/Brans-Dicke theory
8—brown	General relativity theory of gas with cosmological implications

Table 14 The ten most central items using betweenness and closeness centrality measures in the co-citation network 1966–1970. For each publication, the cluster to which it belongs is shown

Betweenness	Closeness
(Penrose 1965)—yellow cluster	(Penrose 1965)—yellow cluster
(Synge 1960)—red cluster	(Harrison et al. 1965)—blue cluster
(Brans and Dicke 1961)—orange cluster	(Penzias and Wilson 1965)—green cluster
(Harrison et al. 1965)—blue cluster	(Synge 1960)—red cluster
(Tolman 1934)—red cluster	(Dicke et al. 1965)
(Landau and Lifshitz 1962)—blue cluster	(Sandage 1961)—light blue cluster
(Weber 1961)—red cluster	(Brans and Dicke 1961)—orange cluster
(Dicke 1962b)—orange cluster	(Lifshitz and Khalatnikov 1963)—green cluster
(Sandage 1961)—light blue cluster	(Tolman 1934)—red cluster
(Penzias and Wilson 1965)—green cluster	(Peebles 1965)—green cluster

and observational tests on general relativity and alternative theories of gravitation and cosmology, where the Brans-Dicke theory assumes an evident central position.

The list of most central publications reflects this momentous reconfiguration of research agendas toward relativistic astrophysics and physical cosmology (Table 14). Penrose's (1965) singularity theorem emerges as the most central item, and in fact, it was a fundamental theoretical advancement in the theory of stellar collapse and in the emergence of black hole physics, and was later extended to Big Bang cosmology (Hawking and Penrose 1970). A large number of most central papers using closeness centrality measures belong to the green cluster, while the betweenness centrality measures show the relevance of in-between clusters such as the one on observational tests on gravitation theories (orange). In this phase, textbooks (even older textbooks such as Tolman 1934) also emerge as particularly relevant in connecting different clusters, as they assumed a new role of providing a body of literature capable of connecting the new emerging core of research on cosmology and astrophysics with the previous research areas of the renaissance phase.

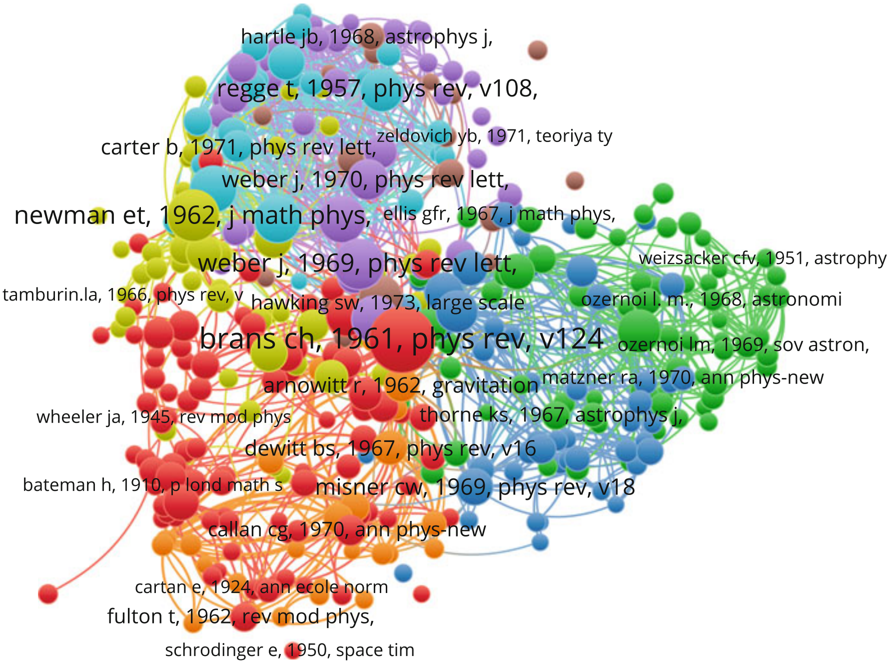

Fig. 20 Co-citation network of the *general relativity publication space,* 1971–1975. Colors represent different clusters. The size of nodes and labels is proportional to the number of citations in the explored subset

4.6 Period 1971–1975

While outside our main temporal focus on the renaissance process, it is interesting to look at the further change in the following 5 years (Fig. 20). According to the analysis of network parameters (Table 3), the 1971–1975 period tended to be a stabilization of the process initiated by the momentous shift seen in the previous period. The analysis of the semantic layer shows that the structural stabilization did not completely correspond to a stabilization of research topics. The largest cluster (red) combines different kinds of papers whose topics range from approaches to quantum gravity up to empirical tests of general relativity, with special reference to the Brans-Dicke theory as an alternative theory for the design and interpretation of experimental tests. The orange cluster is strongly connected with the red cluster and contains theoretical papers on quantum gravity. In this cluster, particularly prominent appears DeWitt's 1967 canonical approach to quantum gravity based on ADM formalism (DeWitt 1967a) and his covariant theory (DeWitt 1967b, c). In Fig. 20, two groups of clusters are connected to this cluster. The first is composed of two large clusters, both related to cosmology (blue and, more distantly, green). The blue cluster contains primarily papers about the cosmological solutions of

Table 15 Research areas represented by the retrieved clusters in Fig. 20. As modularity was higher than in the previous period (see Table 3), we should expect the clustering to be more precise in clearly individuating sub-research areas

Cluster	Research area
1—red	Quantization of GR/equations of motion in GR/ experimental tests GR
2—green	Evolving universe cosmology
3—blue	Cosmological solutions of GR
4—yellow	Theory gravitational waves
5—purple	Observation-driven research on gravitational waves
6—light blue	Black hole theory
7—orange	Quantum gravity
8—brown	Gravitational collapse

general relativity, including the problem of merging general relativity and quantum mechanics in the cosmological realm, and the problem of cosmological singularity (Misner 1969a, b; Hawking and Penrose 1970), while the green cluster is more broadly related to the evolving universe cosmological model (Table 15).

The agglomerate of the last four clusters signal the predominance in this period of gravitational wave and black hole physics. These two fields emerge as strongly related after Weber's announcement of the detection of gravitational-wave signals in 1969, afterward considered as spurious (Weber 1969; see also Collins 2004; Trimble 2017). The role of Weber's announcement is evident from the emerging interconnection between these two areas of research, which resulted from the black holes being understood as hypothetical astrophysical sources of gravitational waves signals observable on earth. The yellow cluster is mostly related to theoretical work on gravitational radiation in general relativity, while the purple cluster is instead directly connected to observational research, either by Weber, or by other scientists entering the field after his announcement. It contains papers on experimental results as well as theoretical papers on the astrophysics of gravitational-wave sources. Part of this research concerns the study of black holes as gravitational radiation sources (Hawking 1971), which is strongly connected with the light blue cluster, more neatly focused on the astrophysical theory of black holes.

The analysis of centrality measures conveys the relevance of textbooks in this phase of increasing and more specialized research, with the extremely relevant "big black book" by Misner, Thorne, and Wheeler (Misner et al. 1973; see also Kaiser 2012) immediately becoming a central node in the semio-semantic network of general relativity (Table 16). Papers that were particularly relevant are Misner's closed-space generic cosmological model's implications for particle horizons (Misner 1969a), the Kerr metric (Kerr 1963), the Hawking-Penrose singularity theorem in cosmology (Hawking and Penrose 1970), and Richard A. Isaacson's work on the theoretical calculation of gravitational waves' properties at high energy (Isaacson 1968), the latter serving especially as a semio-semantic broker between the cluster on the cosmological solutions of general relativity (blue) and the observational gravitational-wave cluster (purple). Weber's announcement of the

Table 16 The ten most central items using betweenness and closeness centrality measures in the co-citation network, 1971–1975. For each publication, the cluster to which it belongs is shown

Betweenness	Closeness
(Brans and Dicke 1961)—red cluster	(Brans and Dicke 1961)—red cluster
(Misner et al. 1973)—purple cluster	(Misner et al. 1973)—purple cluster
(Landau and Lifshitz 1962)— brown cluster	(Landau and Lifshitz 1962)—brown cluster
(Tolman 1934)—green cluster	(Misner 1969a)—blue cluster
(Misner 1969a)—blue cluster	(Synge 1960)—red cluster
(Synge 1960)—red cluster	(Kerr 1963)—light blue cluster
(Isaacson 1968)—blue cluster	(Tolman 1934)—green cluster
(Kerr 1963)—light blue cluster	(Hawking and Penrose 1970)—blue cluster
(Hawking and Penrose 1970)—blue cluster	(Lifshitz and Khalatnikov 1963)—green cluster
(Weber 1969)—purple cluster	(Weber 1969)—purple cluster

discovery of gravitational waves also assumes a high centrality in shaping research agendas in this period. More surprising is the still relevant role of the Brans-Dicke theory in the structure of the co-citation network. The most central in all centrality measures is (Brans and Dicke 1961). This centrality is due to the theory's remarkable property that it connects all the clusters.

4.7 The Dynamics of Co-Citation Network with Citespace, 1946–1975

We conclude this section on the semio-semantic network by exploring a different strategy for clusterizing co-cited papers of the *general relativity publication space* using the program *Citespace* (Chen 2006; Chen 2017; Chen and Song 2019), which also provide an automatic definition of cluster topics by associating labels found with algorithms applied to the titles of citing papers. *Citespace* is designed to perform this kind of historical analysis of the dynamics of research fronts based on clusters of co-cited papers, understood as intellectual bases. After having tested that using similar criteria for the choice of parameters provides a similar result to our previous analysis, we explored the co-citation network in our period of interest using a smaller timespan (3 years instead of 5). We made this choice in order to provide greater granularity. Since we are exploring the entire network from 1946 to 1975, we considered only the 50 most cited papers for each 3-year temporal slice.[31]

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[31] This number of cited items slightly changes from slice to slice because of the number of items that have the same number of citations. Since the number of cited items changes enormously in this period (497 in the first slice against 23,907 in the last slice), this method gives an overrepresentation of research agendas in the early periods. For the historical questions we want to address in this chapter, this is not a major problem because we are interested in understanding the passage between periods, rather than a more granular perspective of the variety of research fields after the

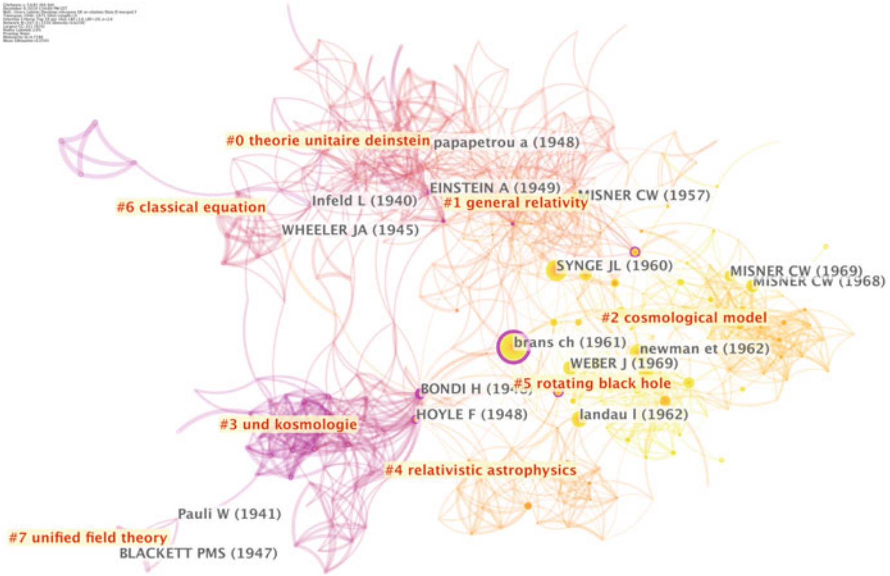

Fig. 21 Co-citation network of the most cited paper in the *general relativity publication space* published between 1946 and 1975. Colors represent different year slices of the citing papers. The numbered labels in red are the clusters' topics as automatically retrieved by *Citespace's* community detection algorithm applied to the titles of citing papers. The black labels identify the two most cited papers per cluster

Figure 21 summarizes the result of the analysis. While the result is of course different from our previous analysis of the 5-year co-citation networks—given the different parameters and methodology followed—the general pattern is confirmed. The labels identifying the topic of the seven clusters are found automatically by the software program employed and, in most cases, are very close to defining the core of the topics in the cluster.[32] The number gives an idea of the relative dimension of the clusters of co-cited papers from 0 (the largest cluster), to 7 (the smallest cluster). The color represents the year slices of the citing papers from the colder color (purple) representing earlier periods to hotter colors representing more recent periods. It is evident that a large cluster (number 1) formed around the late 1950s (mean year of the cited papers 1957) whose main topic is recognized to be *general relativity*. This cluster emerged from two clusters in the previous period, cluster 0 and cluster 6, which are, respectively, the cluster grouping a variety of

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astrophysical turn occurring in the 1960s (for the astrophysical turn see Chap. 1 in this volume). The co-citation network includes only items that were published less than 20 years before the citing articles and that were cited more than three times in the papers of the *general relativity publication space*.

[32] We did not try to clean the results in any way, by, for example, translating the labels or giving a more appealing label.

approaches to the unification of general relativity and quantum mechanics (*theorie unitaire deinstein*) and the connected cluster on the equations of motion (*classical equation*). Parallel and weakly connected to clusters 0 and 6, one finds other clusters (number 3 and the very small number 7) that feed into the large cluster on general relativity, but much more weakly than cluster 0 and 6. Cluster 3 is about cosmology (*und kosmologie*), while cluster 7 is on specific approaches to unified field theories (*unified field theory*). The emergence of the cluster labeled *general relativity* corresponds to what we have previously considered the major mechanism characterizing the renaissance process, namely, the return of interest in problems of general relativity proper, rather than attempts at overcoming Einstein's theory of gravitation in different research realms, such as cosmology, the unified field theory of gravitation and electromagnetism, and the quantization of Einstein's equations.

From the early 1960s the situation changes again, with the formation of clusters focusing on astrophysical and cosmological topics: cluster 4, which is connected to the discovery of quasars and represents the emergence of the field of relativistic astrophysics, (mean year 1963); cluster 2—labeled *cosmological model*—which is clearly related to the emergence of physical cosmology in the framework of the Big Bang model and in connection with the discovery of the CMBR (mean year 1966); and cluster 5 (*rotating black hole*), which is related to the theoretical study of black holes in connection with an observational program on the detection of gravitational waves. Even in the case of cluster 5, there are clear connections with experimental programs, as is shown by the relevance of Weber's announcement of the detection of gravitational waves (Weber 1969). This analysis further confirms the highly central position of (Brans and Dicke 1961) as well as the relative marginality of cosmological research strands in the first phase of the renaissance process in the second half of the 1950s. Only later did cosmology connect to other strands by feeding into relativistic astrophysics.[33]

5 Conclusion

The formal approach based on the framework of socio-epistemic networks leads us to some historical conclusions concerning the definition, chronology, and major features of the process dubbed the renaissance of general relativity. We have here provided enough evidence to support the claim put forward in (Blum et al. 2015, 2016, 2017) that the process of the renaissance of general relativity has to be interpreted as a return of focus to general relativity proper, while the low-water-mark period was characterized by dispersion among various research agendas, as

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[33] One might notice some differences between this network and that presented in (Blum et al. 2018, Fig. 4). Such differences depend on different year slices, different thresholds and parameters, and slightly different choice concerning the original dataset of papers. However, the general historical pattern defining the renaissance process and its later transformation into areas in astrophysics and cosmology is largely confirmed.

social as it was epistemic. We have also provided strong evidence that the major transformative phase of this process occurred between the second half of the 1950s and the early years of the 1960s—before discoveries in the astrophysical domain, starting with the discovery of quasars. The renaissance phase was characterized by a social reconfiguration defined as the emerging of the giant component in the collaboration network (Fig. 1). But it was a conceptual reconfiguration in research agendas, too. In the same period, previously weakly connected research agendas fed into a much more connected core focused on theoretical topics related to general relativity proper with a special role for the theory of gravitational waves (see the 1956–1960 co-citation network in Fig. 17). This process emerges with all the methodologies employed to analyze the co-citation networks, but it is visible in a very striking manner in Fig. 21, where the emergence of a large cluster in the late 1950s is identified exactly with the label "*general relativity*" by an automatic algorithm.

We have also elucidated the claim advanced in (Blum et al. 2018) that the renaissance was a complex process divided, in fact, in two major and quite distinct phases: the theoretical renaissance of general relativity from the mid-1950s to the early 1960s and the astrophysical turn from 1963 onward. The first period was characterized by the socio-epistemic mechanism described above of a return of interest to general relativity proper. Our analysis confirms that the "daring conservatism" program pursued most prominently by Wheeler (see Blum and Brill's Chap. 5 in this volume) was not an individual case, but was strongly embedded in, and reinforced by, a socio-institutional reconfiguration. More speculatively, one might argue that it was the social structure of a network of increasingly connected research centers that made this conservative approach to general relativity the backbone of the renaissance process. It was socially necessary to carry on more conservative approaches based on common problems in order to sustain this increasing connectivity. Some research leaders could continue with their own old and, in some cases, idiosyncratic programs (see, e.g., Jordan's work on his alternative theory of gravitation and Hoyle's work on the steady-state cosmological model), but the connectivity of the network itself was sustained by traveling postdocs who were more prone to shifting programs. They tended to build their relationships, and their career, on the topics in common for the entire network, which after the 1950s, could only be general relativity proper given the small dimension of the social network.

The analyses of the co-citation networks identified a second phase that was substantially different from the previous one. We call this phase *the astrophysical turn*, for it was a period characterized by a specialization of research agendas in astrophysical and cosmological topics. Contrary to the previous phase, which was theory-driven, this phase was experiment-driven. The astrophysical turn clearly occurred after and in relation to discoveries in the astrophysical domain in the 1960s (especially that of quasars in 1963 and of CMBR in 1964/1965) as well as Weber's announcement of the (spurious) detection of gravitational waves. The connection to these experiments is made evident by the fact that clusters of the co-cited papers in 1965–1970 and 1970–1975 (Figs. 19 and 20) as well as clusters 2, 4, and 5 in

Fig. 21 are on topics related to one specific discovery among those cited above: either quasars or the CMBR or Weber's spurious discovery. From the perspective of social dynamics, though, the astrophysical turn did not appear in such an evident manner as a disruption. It appears instead as the incorporation of emerging research trends within a socio-institutional structure that was already being built around other topics.

Apart from a better and more precise characterization and periodization of the renaissance of general relativity as a dynamical two-step process, our analysis of the socio-epistemic networks of general relativity better characterizes the role of specific research agendas to the last part of the low-water-mark period before the renaissance phase, giving then more granularity to our understanding of this passage. The period immediately after World War II saw an increase of people and persons entering research connected to general relativity.[34] Not only does our analysis confirm the quite obvious hypothesis that the quantitative growth in general relativity was mostly an effect of the general increase of the size of physics during and after World War II, but it also gives a precise thematic characterization of this early phase of growth. The field of general relativity was connected with topics of nuclear physics and stellar energy, especially with the work of Alpher, Gamow, and Herman on Big Bang cosmology and Fred Hoyle's work in connecting astrophysics with gravitation theory in the cosmological arena. However, these early trends did not feed into the renaissance. The first one simply disappears from the semio-semantic network in the following period (see Figs. 17 and 18) and does not contribute to the socio-institutional conditions for the re-emergence of general relativity after the mid-1950s. Thematically, the steady-state theory remained connected to the renaissance of general relativity, but the stronger connections were at the social level, especially through Bondi and his group at King's College London, rather than at the level of topics and agendas. In fact, the sector related to astrophysics and cosmology was more weakly related to the topical core than the research agenda on the quantization of general relativity. In Fig. 21, this mechanism emerges with striking clarity, as the astrophysics/steady-state-cosmology tradition runs parallel and only weakly connected to the large central cluster on general relativity until the early 1960s. Only then does this tradition re-emerge with a vengeance, as a consequence of the discovery of quasars, becoming the cluster on relativistic astrophysics (cluster 4 in Fig. 21). Big Bang cosmology, not visible between the mid-1950s and early 1960s, re-emerges even later and with even more of a vengeance after the discovery of CMBR (see cluster 2 in Fig. 21).

The early 1950s saw a change both in the semio-sematic layers and in the socio-institutional layer. According to our social network including the hypothetical copresence at institution relationships, a first structural shift occurred around 1950 (see Fig. 6). A change appears also in the semio-semantic network with the increasing and diversifying of research agendas related to general relativity. The emergence of a more connected institutional relational infrastructure allowed for

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[34] See the rise of the total number of nodes in the collaboration networks in Figs. 1 and 6.

research related to general relativity to be pursued. However, such a research remained dispersed at the level of agendas and could hardly be considered a research field at all (Fig. 16).

There is also a clear difference of status, within the renaissance process, of the other two major research agendas besides cosmology that we hypothesized as feeding the renaissance. While from the social perspective they were all relevant in maintaining the socio-institutional basis of small groups dedicated to different topics, our analysis of the semio-semantic network shows the dominant role of the program on the quantization of Einstein's gravitational equations in the renaissance phase during the period 1955–1960, as it was the major topic cluster alongside work on general relativity proper (see Figs. 17, 18, and 21). It is fairly evident that cosmology and unified field theories were less relevant, or at least less central, in the reconfiguration of the core of research agendas in the renaissance phase. Accordingly, one might conclude that it was the emergence of the program of quantization of gravitation in the late 1940s-early 1950s and the relevance of specific unsolved issues in general relativity for this program—mostly the problem of the equations of motion, but also the initial value problem, gravitational waves, and the definition of energy—which created a major impulse to shift the interest toward problems of general relativity proper.

This analysis, then, characterizes the renaissance phenomenon occurring between the mid-1950s and the early 1960s as a theoretical change related to a social reconfiguration. In this chapter, we showed that there was a parallel chronological development in the three layers. In the future, we will put forward a quantitative analysis with the aim to uncover more hidden causal relations between the three layers employing multilayer network analysis. At the social level, we uncovered that postdoc travelers played the role of "weak ties" (Granovetter 1973) in this socio-epistemic reconfigurations. We further hypothesize that an important role was played by attempts at self-organization of the emerging community of relativity experts starting exactly in mid-1950s with the first ever international conference dedicated solely to general relativity (Lalli 2017). These self-organization activities might have triggered changes at the social level and favored transformations then occurring at the other two levels of our analytical taxonomy. The next step of our project will be to test this hypothesis with a quantitative assessment making full use of the framework of socio-epistemic networks.

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A.1 Appendix 1

Comparison of items' betweenness centralities in the semiotic network (Table 17)

B.1 Appendix 2

Comparison of items' closeness centralities in the semiotic network (Table 18)

Table A.17 The ten most central items using betweenness centrality measures in all 5-year co-citation networks, 1946–1975

1946–1950	1951–1955	1956–1960	1961–1965	1966–1970	1971–1975
(Pauli 1941)	(Tolman 1934)	(Einstein et al. 1938)	(Fock 1959)	(Penrose 1965)	(Brans and Dicke 1961) –
(Tolman 1938)	(Weyl 1917)	(Landau and Lifshitz 1951)	(Einstein 1916)	(Synge 1960)	(Misner et al. 1973)
(Jordan 1944)	(Einstein 1951)	(Tolman 1934)	(Oppenheimer and Snyder 1939)	(Brans and Dicke 1961)	(Landau and Lifshitz 1962)
(Blackett 1947)	(Eddington 1946)	(Einstein and Infeld 1949)	(Papapetrou and Peierls 1951)	(Harrison et al. 1965)	(Tolman 1934)
(Veblen 1933)	(Infeld and Wallace 1940)	(Bondi 1952)	(Kruskal 1960)	(Tolman 1934)	(Misner 1969a)
(Tolman 1934)	(Jordan 1952)	(Infeld and Schild 1949)	(Pound and Rebka 1960)	(Landau and Lifshitz 1962)	(Synge 1960)
(Infeld and Wallace 1940)	(Einstein and Infeld 1949)	(Pirani 1957)	(Landau and Lifshitz 1951)	(Weber 1961)	(Isaacson 1968)
(Tolman 1949)	(Bergmann and Brunings 1949)	(Misner and Wheeler 1957)	(Oppenheimer and Volkoff 1939)	(Dicke 1962b)	(Kerr 1963)
(Jordan 1947)	(Einstein et al. 1938)	(Einstein 1953)	(Tolman 1934)	(Sandage 1961)	(Hawking and Penrose 1970)
(Klein 1926)	(Dirac 1938b)	(Lichnerowicz 1955)	(Misner and Wheeler 1957)	(Penzias and Wilson 1965)	(Weber 1969)

Table B.18 The ten most central items using closeness centrality measures in all 5-year co-citation networks, 1946–1975

1946–1950	1951–1955	1956–1960	1961–1965	1966–1970	1971–1975
(Tolman 1934)	(Tolman 1934)	(Einstein et al. 1938)	(Fock 1959)	(Penrose 1965)	(Brans and Dicke 1961)
(Dirac 1937)	(Einstein 1951)	(Einstein and Infeld 1949)	(Landau and Lifshitz 1951)	(Harrison et al. 1965)	(Misner et al. 1973)
(Hubble 1937)	(Einstein and Infeld 1949)	(Infeld and Schild 1949)	(Einstein 1916)	(Penzias and Wilson 1965)	(Landau and Lifshitz 1962)
(Pauli 1941)	(Infeld and Wallace 1940)	(Tolman 1934)	(Arnowitt et al. 1961)	(Synge 1960)	(Misner 1969a)
(Jordan 1944)	(Eddington 1946)	(Einstein and Infeld 1940)	(Misner and Wheeler 1957)	(Dicke et al. 1965)	(Synge 1960)
(Fermi 1949)	(Einstein and Infeld 1940)	(Bergmann and Brunings 1949)	(Einstein et al. 1938)	(Sandage 1961)	(Kerr 1963)
(Hoyle 1948)	(Infeld 1938)	(Einstein 1953)	(Kruskal 1960)	(Brans and Dicke 1961)	(Tolman 1934)
(Weizsäcker 1938)	(Bergmann and Brunings 1949)	(Bergmann and Schiller 1953)	(Møller 1958)	(Lifshitz and Khalatnikov 1963)	(Hawking and Penrose 1970)
(Alfvén 1937)	(Einstein et al. 1938)	(Dirac 1950)	(Wheeler 1962)	(Tolman 1934)	(Lifshitz and Khalatnikov 1963)
(Spitzer 1946)	(Weyl 1917)	(Landau and Lifshitz 1951)	(Synge 1960)	(Peebles 1965)	(Weber 1969)

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Patronage of Gravitational Physics and the Relativity Community in the USA (1949–1959)

Dean Rickles

> *Remind me not to come to any more gravity conferences!*
>
> Richard Feynman[1]

1 Introduction

In order for good, sustained gravitational research to be carried out, there needs to be a suitable environment in which to do so. General relativity got off to a roaring start, with many of the top people investigating the theory, but it had very quickly stagnated by the 1920s.[2] By the late 1940s, universities were, by and large, indifferent to the subject. The journals were suspicious.[3] There were no dedicated conferences. Supervisors warned their students to avoid the subject, lest they render

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[1] From a letter to his wife from what would be called the GR3 conference in Warsaw and Jablonna in 1962 (reproduced in *What do you Care what other People Think?* W. W. Norton, 1988, p. 91)

[2] Recall Pauli's remark that "classical field theory is a completely squeezed out lemon that can't possibly produce anything new" (Pauli letter to Einstein, September 19, 1946: Collected Papers of Albert Einstein (CPAE) 19–182; reproduced in Pauli 1993, 383). See Chapter 11 of Eisenstaedt (2006) for a clear account of this stagnation, along with its reasons.

[3] For example, as Bryce DeWitt noted, even in the mid-1950s, Samuel Goudsmit, then Editor-in-Chief of *Physical Review*, indicated that he would release an editorial pointing out that "papers on gravitation or other fundamental theory" would no longer be accepted by the journal (nor by *Physical Review Letters*)—it was due to John Wheeler that this decision was reversed (DeWitt 2009, 414). One must suppose that by "fundamental theory" he had in mind *unified* theory (or something of the same sort Eddington produced in his final work), which was by then looked upon as a fool's errand—see, e.g. Richard Tolman's review of the book in which he speaks of Eddington's method as pulling "rabbits of physical principle out of the hat of epistemology"—in The Papers of Richard Chase Tolman, 1735–1958, Caltech Archives, Box 8, Folder 2.

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themselves unemployable.[4] General relativity and gravitational physics became associated with crackpot research. Einstein's own search for a unified field theory was considered a physical dead-end, given its isolation from quantum concerns,[5] and, as mentioned, Eddington's fundamental theory was considered the work of someone who's become thoroughly detached from reality. This all changed in a rather dramatic way in the 1950s, and, indeed, this state of affairs and the transition were well-known to various people operating within in this period: the "neglect" of gravitational physics was often raised as an issue to be tackled—again, see the introductory chapter to DeWitt and Rickles (2011) for more on this.

Despite its strong early public profile following its announcement, then, research on general relativity had degenerated to a trickle by the early 1950s. It didn't really generate a thriving research programme until the second half of the twentieth century, with a pivot point around the middle of the 1950s.[6] Today, of course, it finds itself once again at the centre of attention and is intermingled with both cosmology and particle physics. Indeed, it is hard to believe that it was ever in trouble. Yet it was: it spent a sizeable portion of its early life in mathematics departments, inspired few PhD theses and was more or less experimentally dormant. Many of the kinds of phenomenon we now naturally associate with general relativity—black holes, gravitational waves, etc.—were only established after this pivot point.

An important historical question prompted by this state of affairs is: what triggered this remarkable turnaround?[7] There have been various explanations

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[4] Stanley Deser noted that "the most antagonistic [to GR] that I ever met was Oppenheimer, who when I arrived as a postdoc to the Institute in '53 warned me not to have anything to do with that subject" (Interview with Stanley Deser, by Donald Salisbury and Dean Rickles at Caltech University, March 21, 2011: Niels Bohr Library & Archives, American Institute of Physics [Call number OH 34507]).

[5] See, e.g. Goenner's detailed history of unified field theories for more on this point (Goenner 2004). That there was a reaction against Einstein's approach can be seen quite clearly in the persistent efforts of the founders of the Institute of Field Physics to distance their own efforts from unified field theory proposals in the Einstein mould (see the introductory chapter to DeWitt and Rickles (2011) for more on this point).

[6] Of course, one has to be a little careful here since the rise might simply be due to a general overall rise in activity after the war—see Goenner (2017) for a critical analysis of the concepts of "renaissance" (and "golden age"). However, even correcting for this (by examining publications on other fields of physics in the same way), one finds a significant increase in the publication rate beyond the overall increase. We should also note that there are many significant papers missing from this analysis as it stands with topics and titles somewhat at odds with our search terms. But it is enough to see the general trend of rise and fall and subsequent rise again (see also Lalli et al. in Chap. 2 in this volume).

[7] There are really two components to the puzzle: (1) what caused the initial stagnation and (2) what caused the subsequent awakening of interest. Jean Eisenstaedt (1986) has labelled the (1) "the low-water mark", while Clifford Will (1989) labels (2) "the renaissance" of general relativity. Naturally, we might expect that some of the reasons for the low-water mark will, when "put right", influence the renaissance. Elements of this can certainly be seen; but not all of the renaissance can be thus explained—see Blum et al. (2016, 2017) and Lalli (2017) for recent work on this issue that takes the complexity of the problem more seriously.

offered in the literature (see Blum et al. in Chap. 1 for a review of the literature on this subject), none of which strike me as complete (even when taken together). While acknowledging that there is some degree of truth to each of these stories, in this paper I argue that they ignore an important contribution in the form of private patronage and other funding sources connected to the military-industrial complex.[8] In this case the impact was unusually (for physics) significant and had much to do with the emergence of precisely the kind of environment that allowed the kinds of advances mentioned in Eisenstaedt et al.'s responses. We aim also to reveal the remarkable contingencies (and curiosities) in the development of gravitational physics and the emergence of institutions in which gravitational research was carried out. In this paper, then, we are more concerned with the development of basic *infrastructure* than the renaissance itself, which is dealt with elsewhere in this volume. A significant portion of this infrastructure was American and given the nature of the support offered had an enormous impact on the wider resurgence of interest in general relativity.

2 The Role of Private Patronage: Babson's Dragon

As hard to believe though it might seem, I argue here that general relativity growth, from its period of stagnation, was in an unusually large measure spurred on, at least initially, by two American philanthropists: Roger Babson and Agnew Bahnson—the former more indirectly than the latter. They founded two institutions, the Gravity Research Foundation and the Institute for Field Physics, respectively, that would provide a shot of funds and infrastructure at a time the discipline most needed it. Let us begin with Babson's contribution.

> Lots of people, including Einstein, talk about gravity, the restraining force which makes people walk on floors instead of floating in midair. What worries Roger Babson, 74, economic oracle and head of the Babson Institute, is that no one does anything about it. (*Time Magazine*, January 2, 1950)

Roger Babson (1875–1967) was a man obsessed with gravity. He wanted to control it, to exploit it to solve the world's energy problems but, most of all, *to screen it out*. As he saw it, gravity was a natural evil responsible for the death of his sister and his grandson, both of whom drowned. Writing of his sister's death he wrote:

> When I was a boy my oldest sister drowned while bathing in Annisquam River, Gloucester. Mass. Yes, they say she was "drowned", but the fact is that, through temporary paralysis, or

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[8] I should note that Daniel Kennefick refers to the "unusual military and industrial interest in gravity that emerged in the postwar period, much to the benefit of the field" (Kennefick 2007, 118)—unfortunately, however, he doesn't follow up on his remark: we fill in this remarkable story here. A companion paper, covering similar ground, is Kaiser and Rickles (2018); however, the present paper goes beyond this by explicitly examining how private patronage, and the work associated with it, interacted with the military-industrial complex.

> some other cause (she was a good swimmer) she was unable to fight gravity, which came up and seized her like a dragon and brought her to the bottom. (Babson 1948, 828)

The "dragon" gravity would once again rear its ugly head in the summer of 1947, snatching his grandson Michael as he attempted to save someone's life. This event brought gravity back into Babson's mind with a sense of urgency. Gravity was a menace, something to be battled with and conquered. However, as Don Howard has noted, the real culprit was not deemed to be gravity per se, but our ignorance of its workings (Howard 2005, 2).[9] By this time, Babson had accumulated the financial resources to do something about it.

As Babson explains in his memoirs, *Actions and Reactions*, he began the Gravity Research Foundation [GRF] in January 1949 "as a non-profit institution to collect and distribute information in connection with gravitation" (Babson 1950, 340). He clearly modelled the GRF on the company that made him a millionaire, a financial analysis company called *Babson's Statistical Organization*.[10] This company provided the latest and best financial and business information in a simple digest that banks and investors could subscribe to.[11]

In a suitably symmetrical fashion, physics entered into his approach to finance—indeed, this is an interesting early example of econophysics. One of his tools, the so-called Babsonchart (of American Business Conditions), was based directly on Newton's third law, the principle of action and reaction. Babson used this dubious tool to predict the 1929 Wall Street Crash and the Great Depression that the best financial minds had failed to see (see *The Commercial and Financial Chronicle*, Sept. 7, 1929). This seems to have been pure luck on Babson's part, for there is no plausible mechanism involved in his prediction. Babson's contemporaries thought so too. In his book on the Great Crash, J. K. Galbraith writes of Babson's business acumen that:

> [He] was not a man who inspired confidence as a prophet in the manner of Irving Fisher or the Harvard Economic Society. As an educator, philosopher, theologian, statistician, forecaster and friend of the law of gravity he had sometimes been thought to have spread himself too thin. The methods by which he reached his conclusions were a problem. They involved a hocus-pocus of lines and areas on a chart. Intuition, and possibly even

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[9] Howard argues that Babson's quest can be understood in terms he labels "Physics as Theodicy": battling with the evils of nature by seeking to better understand its workings. This can clearly be seen in the section entitled "Purposes, Objects and Powers" of the document of incorporation of the Gravity Research Foundation formed by Babson. Clause (1) states that the purpose of the corporation is to "Observe the phenomena of nature and encourage, promote and support investigations in search of underlying knowledge of these phenomena. Conduct theoretical and experimental studies to discover the laws which affect them and evolve new technological concepts for the improvement and welfare of mankind" (Document from the personal archive of Cecilé DeWitt-Morette [CDWA]—note that the documents here cited as CDWA are without box numbers, though the Cécile DeWitt-Morette Papers are now held at Dolph Briscoe Center for American History, University of Texas at Austin).

[10] The company still exists as *Babson-United Investment Reports*.

[11] Babson started his career in finance working on the trading floor as it were. Apparently, his overly ethical attitude, involving him questioning his superiors' practices, resulted in his dismissal.

> mysticism, played a part. Those who employed rational, objective, and scientific methods were naturally uneasy about Babson, although their methods failed to foretell the crash. In these matters, as so often in our culture, it is far, far better to be wrong in a respectable way than to be right for the wrong reasons. Wall St. was not at a loss as what to do about Babson. It promptly and soundly denounced him. (Galbraith 1997, 85)

Babson was resolute: his successful prediction was nothing but the work of his hero, the great Isaac Newton, acting at a distance. His penchant for all things Newtonian, already strong, from when he was first properly introduced to Newton's life and work in a course at M.I.T., increased manyfold after this.[12] It is highly probable that this denouncement by the scientific establishment considerably influenced Babson's future interactions in which he displays a marked tendency to avoid academic scientists and "experts" in favour of inventors and entrepreneurs. This impacts directly on the institutional model adopted by the GRF and ultimately explains its failure to thrive.

The GRF was based in New Boston, New Hampshire, located for maximal safety in the event of a nuclear attack in World War III! The site was set up to achieve self-sufficiency "free from industrial plants, able to feed, clothe and heat itself under any circumstances, and independent of uncertain water, light and power utilities" (Babson 1950, 340). Babson had several goals for his GRF. The *primary* purpose of the GRF was to house the most complete library on gravity in existence (Babson 1950, 341). In this he seems to have been successful.

Babson was certainly correct in writing that gravity in this period was a neglected area of research.[13] He notes that "[t]he mention of gravity too often brings a smile as if the enquiry were not taken seriously"—all too true, of course, but Babson hardly helped matters in this regard, as we will see. This provides the second purpose of the GRF: "to get in touch with interested people scattered throughout the world and let them know that they have a sympathetic friend" (Babson 1950, 341). Crucially, Babson did not much care about the credentials of such interested people, and his vision of "cancelling out" and controlling gravity meant a pre-selection bias that rejected experts (and favoured the more crackpot element).

A further task of the GRF had to do with the relationship between gravity and health, harking back to the dragon. He notes that for those over 60 years of age "Gravity may be called Enemy Number One" (Babson 1950, 342). Credulity is stretched somewhat by some of the research foci that fall within this aim. For example, Babson states that some time will be devoted to probing a possible

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[12] This included the establishment of one of the largest collections of "Newtonia" (as he called it: Babson 1950, 340) in the world, on the campus of the Babson Institute at Wellesley, Massachusetts. One of the library's rooms is an actual room used by Newton while in his final years in London. This was purchased by Babson's wife when she discovered the building was being demolished, shipped over from the UK, and rebuilt on site as Newton would have used it "with the same walls, doors and even the identical shutters containing the hole through which he carried on his first experiments in connection with the diffusion of light" (Babson 1950, 340).

[13] This harks back to my earlier remark about the "low-water mark" period being a well-known phenomenon at the time, rather than a recent historical construct.

correlation between the phases of the moon and the incidence of accidents and disease:[14]

> [O]ne of the tasks of our Gravity Foundation is to collect from hospitals, insurance companies and physicians the day and, if possible, the hour, of a fracture and learn how this time correlates with the phases of the moon. Not only does the pull of the moon and sun counteract (or relieve) at certain times the downward pull of the Earth on an individual, but this same gravity may affect the temporary judgment or awareness of the individual. After ascertaining definite data on the above, it must further be recognized that the variation in this gravity pull of the sun and moon may affect the judgment of individuals differently according to their mental capacity and development. (Babson 1950, 342)

In addition to such "gravitational astrology", Babson was also deeply concerned with the *harnessing* of the power of gravity, to develop "free" power:

> The general impression is that gravity will not be harnessed until a partial insulator, reflector or absorber is discovered to develop a *differential*. It is further believed that this discovery will be accomplished through stumbling on some alloy which will give the desired results. Hence, the Foundation is encouraging all engineers and chemists who work with alloys to be on the watch for such a discovery. It surely would be a great blessing to mankind. (Babson 1950, 343)

This vision—amounting in Babson's mind to the creation of perpetual motion (a vision influenced by his conversations with Edison)—would guide the entry requirements for essays in the GRF's competition (which we turn to below), threatening to further bury the enterprise. Babson was profiled in *Time* magazine in 1950, just after he had established the GRF (and the same year he revised his autobiography). In this article the reporter clearly adopts a mocking tone. But though Babson was certainly viewed as a crank, by both economists and physicists, he had the connections to be able to attract external funds for the GRF. As the *Time* article notes:

> A leading shoe manufacturer offered Babson $100,000 for "something that can be put into the sole of a shoe to insulate against gravity." Floor-covering manufacturers showed a lively interest in the possibility of "flying" carpets.

We strongly suspect that the GRF's patrons had a similar distrust of "scientific experts", being mostly self-made men. A GRF conference was held in 1951, at which attendees included Clarence Birdseye of frozen food fame who was also one of the original trustees of the GRF, himself having a very keen interest in anti-gravity. The conference employed specially designed "anti-gravity" chairs that reclined their occupants in such a way as to aid circulation. A bulletin read that "It is the hope that New Boston will gradually become the center where physicists, engineers, metallurgists and others especially interested in the causes and the

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[14] This overestimation clearly shows Babson's profound lack of understanding of gravitational physics. Though we might look more charitably upon such studies as early examples of the effects of microgravity environments on such things as bone density, so essential for subsequent space missions.

possibilities of gravitation will come as a mecca in the summer" (quoted in Gardner 1957, 94).

But the GRF could never really gain the prestige it desired. Babson held too much control over which lines of research were investigated. Since he was no scientist, these tended to be at odds with scientific orthodoxy. For example, in one GRF bulletin, the biblical miracle of Jesus walking on water was offered up as *evidence* in the possibility of anti-gravity shields, as was the ability of angels to defy gravity! The GRF stood no chance (at least not in this form). Martin Gardner famously mocked Babson's own gravity ideas in an article entitled "Sir Isaac Babson", calling the GRF "perhaps the most useless scientific project of the twentieth century" (Gardner 1957, 93). He was referring to the stated aims of the GRF *and* its annual essay competition, namely, to discover some kind of gravity screen ("the right kind of alloy"). Gardner rightly points out that the concept of a material that is opaque to gravitational interactions was made obsolete by the shift to the general theory of relativity. He clinically demolishes other views of Babson and his associates. In Gardner's opinion, however strong Babson's love of Newton was, "he … failed to emulate the great scientist" for "he has failed to acquire more than an elementary knowledge of physics". He goes on to note: "there is surely a touch of pride in his refusal to accept advice from competent physicists on how money could best be spent for the good of science and humanity" (Gardner 1957, 100). That rather gets to the psychological heart of the matter and is the crucial contrast between Babson and Bahnson.[15]

Agnew Bahnson was a close friend of George Rideout (the president of Babson's GRF and the person who had initially suggested the idea of a GRF to Babson). As we shall see in a moment, on the basis of how the initial organizational meetings went, Gardner's critique offered up a recipe for a more successful venture, primarily bankrolled by Bahnson, in which expert advice on how to spend money is well-taken. To get to that story, we must first consider an important transition in the GRF's essay competition which led, curiously, to a positive snowballing effect for the fortunes of quantum gravity and directly influenced the more positive venture.

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[15] I would add that ego (and, indeed, more than a dash of selfishness) was at the root of many of the GRF's chosen topics. For example, in his autobiography he notes that the "Foundation is interested in the ocean tides at Gloucester, Massachusetts" (Babson 1950, 344), which is where Babson just happened to spend his summers—not tides *in general*, but where the tides might affect *him*. We should, however, not be quite so hard on Babson, and perhaps Gardner is a little too harsh. Babson's concerns were outwardly directed as well as inwardly. He seems to have genuinely wanted to have a large positive impact on the world and was simply misguided in his methods—my thanks to David Kaiser for impressing this point on me.

3 The GRF Essay Contest

The GRF would have probably suffered a well-justified early demise had it not been for the essay competition it established “for the best two thousand word essays on the possibilities of discovering some partial insulator, reflector or absorber of gravity waves” (Babson 1950, 344). Even this competition, had it not been hijacked by quality people who tore apart or simply ignored the basic theme of the competition, would have quickly been consigned to the dustbin of history. This competition is all that remains of the foundation, at least in the public eye,[16] and it has, for some time now, basked in the mantle of scientific respectability as a result of the respectability of its entrants (now encompassing virtually all of the great general relativists >1960).

The winnings for first prize in the early years was $1000, a not inconsiderable sum in those days (roughly equivalent to a postdoctoral fellow’s annual salary) and pretty good even today! David Wittry (from the University of Wisconsin) won the first ever first prize, in 1949, for his historical essay describing (failed) attempts to produce an anti-gravity screen. The second competition’s first prize in 1950 was awarded to a graduate student at Princeton University. This was covered by *Time* magazine, which suggests that at least in the public eye gravity still held some fascination—likely through its association with Einstein, who was then, and perhaps still is now, the epitome of a scientist for most people and would be mentioned periodically in the popular press as he pursued his unified field theory. There was evidently seen to be something mysterious and difficult about gravity that had the power to hook a general audience. For our purposes, 1953 and 1954 were important years for the essay competition, though for very different reasons.

Not long after Babson had been profiled in *Time* magazine, at the end of 1950, a young postdoc named Bryce DeWitt, who had just completed his PhD on the then very unfashionable topic of “quantum gravity”[17] (under the rather minimal supervision of Julian Schwinger, at Harvard), was making his way from the ETH in Zurich to Bombay (now Mumbai), where he was taking up a Fulbright scholarship at the Tata Institute—where he would write his first published paper on quantum gravity (involving spinors in a gravitational field: DeWitt and Morette DeWitt 1952), co-authored with his new wife Cécile Morette. The two would soon connect, albeit indirectly, through the essay competition.

Bryce DeWitt was born Carl Bryce Seligman, on the January 8, 1923, in Dinuba, California. He was the eldest of four boys. His father was a country doctor, of

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[16] Aside, that is, from the curious GRF monuments scattered around various US campuses, installed to “remind students of the blessings forthcoming when science determines what gravity is, how it works, and how it can be controlled”, as the engravings point out.

[17] Here I take “quantum gravity” to refer to attempts to bring within a single framework the principles of quantum theory and those of general relativity. In the case of DeWitt’s thesis, this involved a fairly orthodox quantization of the gravitational field along similar lines to the electromagnetic field.

German Jewish descent.[18] His mother was a teacher of latin and mathematics of French Huguenot ancestry—she had written an undergraduate thesis on the history of the calculus (a copy of which remains in the Bryce S. DeWitt papers, Dolph Briscoe Center for American History, University of Austin at Texas). Bryce DeWitt was something of a maverick. He wrote (at Harvard, where he had also been an undergraduate) what is arguably the first ever PhD (in the west: Bronstein's being the only other such PhD) devoted to the subject of quantum gravity.[19] He had just gotten engaged, to Cécile Morette, when he set off for the ETH to work with Pauli. Morette had previously worked on meson physics under Heitler and de Broglie, but when she met Bryce at the Institute for Advanced Study at Princeton, 1949, she had fallen under the spell of Feynman's path integral approach to the formulation of quantum theory and was busy mastering its mathematical details, initially with Freeman Dyson, and then directly from Feynman himself, in Santa Fe. Bryce was struggling somewhat career-wise and was having serious trouble getting employment to work on quantum gravity—it is evident that supervisors' warnings to their students to avoid gravitation were not misplaced at this time. He was, in the years following the completion of his doctoral thesis, on the verge of giving up quantum gravitation, but was encouraged by Freeman Dyson not to throw in the towel. As he explains in a letter to various department heads:

> Owing to the difficult and tedious nature of research in gravitational theory, and also owing to the apparent complete lack of any immediate practical application of its results, I was, until recently, strongly resolved to discontinue further work along these lines and to turn my attention elsewhere. A conversation I had with F. J. Dyson this summer, however, has left me with somewhat altered views. He stressed to me the urgent need for workers in field theory who have a thorough understanding of gravitational theory and its problems.[20]

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[18] Bryce dropped the Seligman part of his name as a result of anti-semitic incidents surrounding him and his three brothers—all of the them changed their names on the instructions of their father, Emil—Steven Weinberg claims that Felix Bloch vetoed DeWitt's professorship at Stanford in 1972 as a result of his change of name, which Bloch viewed as an anti-semitic decision (Weinberg 2009, 10). Bryce was raised in the Presbyterian church; the only Jewish remnants were, as he put it himself, the matzos his grandfather brought over at Passover—see DeWitt's own rendition of his obituary (Bryce S. DeWitt papers, Dolph Briscoe Center for American History, University of Austin at Texas: Box 4RM174).

[19] DeWitt's pre-doctoral thesis had absolutely nothing to do with gravity, quantum or classical. It was entitled "An outline of the theory of specific heats (20 May 1946 – a term paper)" 106pp—with a bibliography of six items! This includes a quantum mechanical theory of specific heats. It is, however, brilliantly written and shows his penchant for extreme number crunching. But certainly no sense of the QG work to come—though it will have set him up well for his work on hydrodynamical detonation (also at Harvard!). Curiously, the latter was heavily computational, and the skills he built up here would later be used in the service of quantum gravity (though primarily colliding black hole and gravitational two-body problem) simulations. Also, one might wonder whether DeWitt's decision to pursue hydrodynamics was influenced by the work that was recently being undertaken at Harvard on underwater ballistics (for the Navy—see Rees 1980, 612). DeWitt later began work on (Lagrangian) hydrodynamics at the Lawrence Livermore Laboratory—he discusses his time at Livermore (the "Rad Lab") in DeWitt (1985).

[20] Bryce DeWitt, letter to Raymond Thayer Birge, November 11, 1951: CDWA.

But the letter fell on deaf ears, and the situation in gravitation remained as dire as before for several more years. The best DeWitt managed, after the Tata Institute trip, was to get a position at Livermore working on the hydrogen bomb under Edward Teller's general direction.

Bryce entered Babson's radar (or, at least, George Rideout's) in 1953 when he submitted an essay for the GRF's essay competition. Though not considered a respectable institution, given Bryce's frustration with the lack of interest in gravitation, it must nonetheless have pleased him to find anything at all on the subject, even if ill-conceived in its aims. Recall that the condition was in place that submissions must deal in some way with the notion of anti-gravity and a gravity shield. DeWitt's article attended to this point, but tore apart the very possibility of such an anti-gravity shield, "essentially giving them hell for such a stupid … way it had been phrased in those early years".[21] Surprisingly, the essay won first prize. DeWitt wrote the essay in a single evening calling it "the quickest $1000 I ever earned". (Note that in this essay, DeWitt views the problem of gravity's quantization as all but completed, using Hamiltonian methods, save for a few remaining details.)

Rideout clearly saw DeWitt as a person who could lift the respectability of the GRF, working as he was in the Rad Lab at Berkeley. Indeed, several people have remarked that this seemed to be the case, arguing that whereas the prize was previously avoided by "serious" physicists, after DeWitt won, the floodgates opened. Cécile herself made just this point: "until Bryce, no real physicist wanted to touch it, because it was considered as a total crackpot project". It is indeed true that the next year saw Richard Arnowitt and Stanley Deser (both students of Julian Schwinger, as was DeWitt) take first prize,[22] but Bryce DeWitt is less sanguine about his role in shifting the competition's reputation, writing that "it took probably five or six years for the atmosphere to change". It is something of a myth that the GRF attracted no notable people until DeWitt won the competition. In fact, there were people *more* distinguished than DeWitt (at that time) who were entering

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[21] Interview of Bryce DeWitt and Cécile DeWitt-Morette by Kenneth W. Ford on February 28, 1995, Niels Bohr Library & Archives, American Institute of Physics: www.aip.org/history-programs/niels-bohr-library/oral-histories/23199 (accessed January 22, 2019). DeWitt would reiterate this point in a reassessment essay he submitted to the 1960 round of the essay competition, writing: "any frontal attack on the problem of 'harnessing' the power of gravity along lines based on analogy with our experiences with electromagnetism is a waste of time" (DeWitt 1960, 1). As David Kaiser has noted, Rideout was very serious about respecting this gravity-shield criterion writing (in his second annual report to the trustees, from August 1, 1950) that "We will not accept any essays simply on the subject of Gravity. Some of them sound just like a text book. We are insisting on adherence to the subject, namely, the objective of discovering some partial insulator, reflector or absorber of gravity" (GRF, Gravity Research Foundation records, Special Collections, Babson College, Wellesley, Massachusetts: Box 1, Folder 5—as cited in Kaiser (2000), 575).

[22] Much to the chagrin of Robert Oppenheimer who was supervising both, as postdocs, at the Princeton Institute for Advanced Study. Oppenheimer thought that entering the competition and accepting the prize brought the IAS into disrepute. He believed that Arnowitt and Deser exploited their positions at the institute (Interview with Stanley Deser, by Donald Salisbury and Dean Rickles at Caltech University, March 21, 2011: Niels Bohr Library & Archives, American Institute of Physics, oral history archives [Call number OH 34507])—more on this below.

before 1953, including J. M. Luttinger (fourth prize in 1951[23]) and Martin Perl (in 1952; later winning the Nobel Prize for his discovery of the tau lepton)—E. T. Jaynes applied the same year as DeWitt. It is true that there was an emerging overall increase in the rate of quality submissions (e.g. Felix Pirani's submitted version of his paper demonstrating definitively the transport of energy by gravitational radiation by 1957, presented as a means of controlling gravity to a certain extent), but remember that there was an overall increase in the production of work on general relativity *simpliciter*.

In any case, Richard Arnowitt and Stanley Deser's 1954 entry was, it seems, something of a physicist's joke, playing with several then trendy topics in a way that sounded like a somewhat plausible method of gravity manipulation linking gravity and nuclear physics. In a letter to Oppenheimer, following a mention of their paper in the *New York Herald Tribune* and elsewhere, Deser made light of their ill-gotten gains:

> Such little experience as I have had with publicity inclines me to the view that it might be wisest, since there has apparently been little echo of the articles, to forget the whole thing; scientists would either laugh at the joke (as people at Princeton did when they heard we had won with that essay) or dismiss it as another example of garbled science reporting. The non-scientific public, I would imagine, skim all rocket-to-the-moon stuff and then forget it. (Stanley Deser, letter to Robert Oppenheimer, December 13, 1955: J. Robert Oppenheimer Papers, Library of Congress, Washington: Box 30)

Deser was likely right that the "non-scientific public" would pay no heed; but there were other eyes on the article, and the "echoes" would be heard by what would become important funding agencies for gravitation. We will see below that what was nothing more than a silly joke to Arnowitt and Deser had an unexpected and rather fruitful punch line. The idea expressed in the competition essay, that gravitational phenomena could be converted to nuclear energy, suggested to some less knowledgeable folk that particle accelerators could be useful for "gravity manipulation" (i.e. for anti-gravity). For example, Agnew Bahnson, who we will meet in the next section, appears to have assumed that UNC's new accelerator might be useful for the study of gravity, and it seems highly likely that he was motivated by this paper through his association with George Rideout—he also explicitly refers to the paper in several "memoranda" from the DeWitts' Institute of Field Physics he helped found and bankroll.

In 1954, the GRF and its essay competition, despite having been won by Bryce DeWitt the year prior, was still considered something of a fringe endeavour, notable only for the lucrative prize money it afforded young scholars. However, in addition to being taken over by a higher calibre of researchers in the mid-1950s, the GRF also triggered, via the personal network of its vice-president George Rideout, several new directions which would be taken far more seriously and lead to a far more significant

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[23] Curiously, Luttinger was Pauli's own pick for the potential husband of Cécile Morette and would try to engineer such a coupling—against Luttinger's wishes (Interview of Cécile DeWitt, by Dean Rickles and Donald Salisbury, Austin, Texas, March 4, 2011).

regeneration of gravitational physics. It would turn out that the source of the fringe nature of the GRF (namely, the promise of anti-gravity: gravitation's answer to the atom bomb) would trigger the interest of a whole range of sponsors who, as mentioned above, *inadvertently* (with the promise of anti-gravity) funded pure, basic research in classical and quantum gravity. We consider these sponsors in subsequent sections. In the next section, we see how several examples of institutional design and funding were indirectly spawned by the GRF, via George Rideout. This all happened at roughly the same time, in the middle part of 1955—it may or may not be a coincidence that Einstein died shortly before, thus shining a public spotlight on his theory of gravitation.

4 Institute of Field Physics, Inc.: Bahnson Contra Babson

The Institute of Field Physics set much of the gravitational physics agenda during the late 1950s and 1960s. It was formed to "provide a place where a number of physicists can work quietly, in financial and professional security, in a presently neglected field". Even in 1955, when the institute was being set up, general relativity and quantum gravity were treated as a province of less reputable folk. As Bryce DeWitt put it in the opening passages of one of the founding statements of the Institute of Field Physics, "Remarks on a Presently Neglected Area of Physical Research":

> The modern theory of gravitation, as formulated by Einstein in 1915, represented the high point of a profound revolution in human ideas as to the nature of the physical universe. The fruits of that revolution and of simultaneous upheaval occasioned by the advent of the quantum theory are today everywhere to be found—**except**, strangely enough, at the summit itself. The general theory of relativity (i.e. Einstein's gravitational theory) remains almost totally barren, its only applications so far being cosmological theory and in the interpretation of certain minute astronomical effects.
>
> This situation did not come about through any lack of interest in gravitation on the part of physicists immediately following Einstein's formulation. Indeed the foremost physicists of the older generation entered the arena of general relativity theory with enthusiasm, hoping to bind the phenomenon of gravitation to the rest of physics (or vice versa) in an intimate, fundamental way. However, these men failed, and those who followed them, being thus discouraged from making similar attempts, gradually left the field to the cranks and crackpots.[24]

Amongst the reasons DeWitt presents for the lack of interest was, as he had claimed in his GRF essay, the lack of *incentives*. Without some hope of reward (financial or otherwise) and esteem, to pursue GR is rendered a lonely journey. In addition to such psychological factors impeding progress, he also mentions previous failures to extend GR, the lack of experimental input, and the difficulty of the mathematics

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[24] Document dated October 5, 1955: CDWA, p. 1—emphasis in the original.

employed.[25] Curiously, in his motivation for the study of GR, DeWitt mentions that the "Golden Rule of science" (to base theory on empirical input) might not be as inflexible as is often supposed—"it is not clear how tight a restriction it imposes on the theorist". Citing the history of Dirac equation, he argues that in order to bridge "the gap between gravitation and the rest of Nature we will be forced to go out on a limb in order to attain success".[26] It is certainly true that DeWitt went out on a limb in working together with Bahnson, who himself fell into the "cranks and crackpots" category.

Despite DeWitt's clear-headed proclamations, the Institute of Field Physics was formed in rather strange circumstances. The story will require that we regress through some back-stories. As mentioned, Rideout is the point of origination of some of these through his connections to others interested in spearheading gravitational projects of their own, including Bahnson. We begin with the aviation company, the Glenn L. Martin Company[27] (now Lockheed Martin). Almost simultaneously with the running of the Bern conference in 1955, George Trimble, vice-president of the Glenn Martin Company, wrote Bryce DeWitt, then still a Rad Lab postdoc, that:

> During a recent conversation with Mr. George Rideout, president of Roger Babson's Gravity Research Foundation, we were commiserating on the unfortunate state of the affairs that knowledgeable folks do not wish to get "mixed up" in the field of gravity research. During the course of the conversation he reviewed with me your suggestion that perhaps his Gravity Research Foundation might be transformed from its present function into an active center

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[25] Note how closely these align with the various recent resolutions of the puzzle of general relativity's renaissance mentioned earlier.

[26] "Remarks on a Presently Neglected Area of Physical Research", October 5, 1955: CDWA, p. 4.

[27] Note that Agnew Bahnson's later anti-gravity collaborator Thomas Townsend Brown worked for the Glenn Martin Company in the late 1930s, as a materials engineer. Bahnson formed a laboratory, not long after the Institute of Field Physics received its certificate of incorporation, in which he and Brown attempted to build flying saucers based on Brown's idea that anti-gravity can be generated through strong electric fields (i.e. that strong electric fields had gravitational effects so that the latter could be controlled by manipulating the former)—he was guided by a simplistic electrodynamical analogy (of much the same kind that guided the early theoretical work on quantum gravity, in fact; though, following the likes of W. F. G. Swann, a regular of Babson's "Gravity Days", Brown questioned the veracity of Einstein's theory of relativity and considered the dielectric material paramount in testing gravitational forces). Brown had attempted to establish his own gravity research institute, through a joint grant (for "Project Winterhaven") with the Franklin Foundation via his own "Townsend Brown Foundation", which, in addition to a general study of gravitation and its relation to electromagnetism, proposed to redo the Trouton-Noble experiment to detect the Earth's motion through the ether. The Franklin Institute Laboratories for Research and Development discussion documents for this venture mention quantum gravity, noting that the "smallness of magnitude of the interrelationships, as pertaining to terrestrial experiments, would continue to exist and that any practical bearing which the interrelationships might have upon us would lie in their effects in some large-scale cosmological situation which they control" (http://www.thomastownsendbrown.com/hydro/winterhaven.pdf, A-25). For this reason, the institute was not keen on funding practical work.

> of research concentrating on the field of gravity. He also told me that the foundation was not able to undertake such an expansion.[28]

It seems that DeWitt had suggested to George Rideout something along the lines of the Institute for Advanced Study—a model he was very familiar since both he and Cécile both spent time in several of them.[29] Trimble himself describes the proposed activity as an "modest version of the Institute for Advanced Study".[30] This model better aligned with the Glenn Martin Company's plans. However, their ultimate goal was not pure research for its own sake, but something more applied (with military applications). The letter goes on:

> It occurred to us sometime ago that our industry was vitally concerned with gravity. As time goes on we become more and more concerned because we feel certain that sooner or later man will invade space and we see it as our job to do everything possible to speed this event. At least one category of the things one must study, when he desires to bring space flight to a reality, is the laws of nature surrounding the force of gravity.[31]

One wonders what Babson would have made of the fact that it was Newton's equations that got man into space! Trimble bemoans the fact that most of those working on gravitation are "mad men and quacks"—perhaps he has those connected with Babson's own endeavour in mind here? The relevant work that they had done on space flight, notes Trimble, had been contracted out to German scientists working within Germany. But, the construction of a space vehicle was the ultimate goal of the Glenn Martin gravitational project, and a small research group focusing on the theoretical principles of gravitation was one of their routes.

The Glenn Martin Company established their institute, known as RIAS [Research Institute for Advanced Study, directed by a man named Welcome Bender], with Louis Witten[32] placed in charge of the hiring of gravitational personnel, with

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[28] Letter from G. S. Trimble to Bryce DeWitt, dated June 10, 1955—[CDWA].

[29] Cécile was sent to the Dublin Institute for Advanced Study when she worked in Joliot's lab in the mid-1940s, having earlier been charged, by Joliot, with the task of reviewing the famous Bohr-Wheeler paper on fission (Interview of Bryce DeWitt and Cécile DeWitt-Morette by Kenneth W. Ford, February 28, 1995, Niels Bohr Library & Archives, American Institute of Physics: www.aip.org/history-programs/niels-bohr-library/oral-histories/23199 (accessed January 24, 2019).

[30] Trimble makes an interesting remark concerning the tight relationship between scientific research and society: "we feel morally obligated to push forward in the basic sciences and we believe as a dynamic industry we can provide the motivation for advances that can be obtained in no other way". In other words, for better or for worse, the pursuit of certain areas of basic research demands some kind of motivation beyond the search for deeper knowledge. Practical applications are one way to motivate such study.

[31] Letter from G. S. Trimble to Bryce DeWitt, dated June 10, 1955—[CDWA].

[32] Louis Witten, born in 1921, had previously worked for the Glenn Martin Company on pilotless aircraft, more as an electronics engineer than a theorist, after he left the army in 1946, but before he did his PhD in physics. Despite this late entrance into physics, Witten tenaciously pushed himself through 12 courses per semester to catch up and learn sufficient mathematics to do research. Following his PhD work, Witten worked on Spitzer's stellarator as a postdoc at Princeton, together with Martin Schwarzschild and Martin Kruskal. He shifted back to the Martin Company after just a year, due to financial circumstances (Interview of Louis Witten by Dean Rickles and Donald

Bob Bass joining first. DeWitt was not interested and was simultaneously being approached by another industrialist, Agnew Bahnson, a very wealthy North Carolinian gravity enthusiast, offering more lucrative terms (for both him and, eventually, his wife, Cécile): the promise of a position in a more traditionally academic environment, coupled with the freedom of an externally funded position, would ultimately win out.[33] Though, without the intervention of John Wheeler, DeWitt would have also turned down Bahnson's proposal, viewing him as yet another crackpot.[34]

There was much truth to DeWitt's diagnosis, but, fortunately, Bahnson was no Babson. Bahnson contacted DeWitt via letter, May 30, 1955, and it can clearly be seen that Bahnson is captivated by the spectacular technological possibilities gravity might afford, just as much (if not more) than Babson was. Bahnson liked to get his hands dirty too, with amateur experiments, and had his own quirky theories of gravity and unlimited energy extraction from the gravitational field. Yet, despite this, the research institute Bahnson backed adopted a far more sober approach than the GRF, at least in public life. For example, in the forward of an early draft for the Institute of Field Physics' promotional brochure, entitled "The Glorious Quest", Bahnson wrote:

> In the minds of the public the subject of gravity is often associated with fantastic possibilities. From the standpoint of the institute no specific practical results of the studies can be foreseen at this time.[35]

There is no mention of flying carpets, anti-gravity soles for shoes, or any such whacky Babsonesque ideas. In many ways Babson's GRF was used as a test-case, or foil, highlighting things to copy but (more importantly) things to avoid. A lengthy exchange of letters between Bahnson and several senior physicists (especially John Wheeler, whom Bahnson clearly admired a great deal) set to work on eradicating any aspects that might lead to claims that the institute was for crackpot research. Wheeler did much behind the scenes sculpting of the Institute of Field Physics and was perhaps most responsible for the bringing about of the Institute. For example, writing to the acting president of the University of North Carolina, Harris Purks, November 25, 1955, Wheeler mentions:

Salisbury on March 17, 2011, Niels Bohr Library & Archives, American Institute of Physics: www.aip.org/history-programs/niels-bohr-library/oral-histories/36985).

[33] Trimble and the Glenn Martin Company played a role in helping Bahnson and the DeWitts' institute get off the ground. Not only did they purchase a Founder's Membership for the Institute of Field Physics, but for the considerable sum of $5000, they also offered their support to solicit further funding.

[34] See interview of Bryce DeWitt and Cécile DeWitt-Morette by Kenneth W. Ford, February 28, 1995, Niels Bohr Library & Archives, American Institute of Physics: www.aip.org/history-programs/niels-bohr-library/oral-histories/23199 (accessed January 24, 2019). In this interview, Bryce notes how he had already encountered Wheeler before this when Wheeler came to visit him in Danville (halfway between Livermore and Berkeley, where Cécile was teaching at the time) to discuss quantum gravity.

[35] Document dated November 17, 1955: CDWA.

> the absolute necessity to avoid identification with so-called "anti-gravity research" that may be today's version of the last century's search for a perpetual motion machine. [...]
>
> Unfortunately, there are sensationalists only too willing to confuse in the public mind the distinction between so-called "anti-gravity research" ... and responsible, well informed attempts to understand field physics and gravitational theory at the level where it really is mysterious, on the scale of the universe and in the elementary particle domain.[36]

He goes on to applaud the step (in fact suggested by Wheeler himself, earlier) of attaching to every piece of Institute of Field Physics publicity a "disclaimer" to the effect that the Institute of Field Physics is in no way connected to such anti-gravity research. This, Wheeler says, is necessary to avoid discouraging both sponsors and scientists. The message that anti-gravity connotations must be avoided at all costs runs through much of the correspondence and foundation documents like a mantra. It clearly played a vital role (in the minds of physicists) in establishing the legitimacy of the enterprise. A "Protection Clause" would therefore be attached to each Institute of Field Physics statement stating that "the work in field physics and gravitation theory carried on at the University of North Carolina at Chapel Hill, and financed by the Institute of Field Physics, as fund raising agency, has no connection with so-called 'anti-gravity research' of whatever kind and for whatever purposes".[37]

The plan was to house an institute within an academic institution, so as to avoid the conflict that physicists felt would be involved in working in an industrial setting. The chosen location was the physics department at the University of North Carolina, Chapel Hill. In order to lend prestige to the Institute of Field Physics, Bahnson secured letters of comment from several of the most prominent physicists of the day, including Oppenheimer, Dyson, Teller, Feynman and Wheeler. The various letters of support (dating from between October 1955 and January 1956), for which the preceding letter from Wheeler to Purks provides a cover letter, highlight the recognition that general relativity and gravitational research had been unfairly neglected and the need for a renewal of interest. Oppenheimer writes that he "shares with most physicists the impression that this field has been rather neglected by us". Dyson seconds this (as does Nordheim), but adds some conditions for success, more or less reiterating what Wheeler had already said: that immediate results should not be expected and that ("to avoid becoming isolated and sterile") the institute should be settled as firmly as possible in "the framework of normal university life". Edward Teller remarks that "a comprehensive examination of general relativity and high-energy physics, together with an investigation of the interaction between these two fields may very well lead to the essential advance for which we are all looking".

Feynman too voiced the opinion that "the problem of the relation of gravitation to the rest of physics is one of the outstanding theoretical problems of our age". However, he was less positive about the chances of the proposed institute in its original form. Feynman was not convinced that an industrially funded institute, detached from a university, could possibly deliver the requisite flexibility to develop

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[36] Letter from Wheeler to Purks, dated November 25, 1955: CDWA.

[37] Letter from Wheeler to Purks, dated November 25, 1955: CDWA.

new fundamental knowledge since that required absolute freedom to bounce around between topics, as one chose.[38] On learning that the institute was to be housed in a university, Feynman was unreservedly positive about the proposal.[39]

John Toll, head of physics at the University of Maryland, writes, directly discussing the other letters:

> Most of my colleagues have pointed out in their comments that the field of general relativity has not received the attention which it deserves and that it is particularly important to attempt to obtain some synthesis of the methods and concepts used in general relativity with the ideas now employed to discuss elementary particles. One reason for the neglect of general relativity has been the great difficulty of work in this field which challenges even the best theoretical physicists; solution of the major problems involved will probably require a determined program which may extend over many years. *A second and related reason has been the difficulty of obtaining adequate support for this field; the problems are not of the type which are supported by federal agencies which finance so much of the research in physics in the United States by short term contracts, mostly in fields which appear to have more immediate applicability to defence problems*.[40]

This was all written towards the end of 1955. The Institute of Field Physics had just received its certificate of incorporation on September 7, 1955, becoming at a stroke one of the most important research centres for gravitation. This was enough of an event to attract the front page (and a considerable chunk of real estate elsewhere) in North Carolina's preeminent newspaper the *Salem Sunday Journal and Sentinel*, calling it "one of the most significant developments to have occurred in North Carolina in recent years" (June 24, 1956). Toll's concerns, in his second reason, would quickly become nullified soon after, with support generated from military as well as other private and industrial sources.

Bahnson maintained his own exuberant interests in gravitation and was sufficiently well connected to have these (often naive, sometimes crackpot) ideas examined by the likes of Edward Teller, and also Bryce DeWitt, with the assistance of the head of physics at UNC, Everett Palmatier.[41] Bahnson's interest, as with Babson's, was with electrically induced anti-gravity (or, to use Brown's terminology, "electrogravitics"). But while Babson wanted to shield it for its evils, Bahnson wanted to master it to create spaceships. In 1957 he hired T. T. Brown to assist him with a series of experiments, ultimately to build an anti-gravity powered flying saucer.[42] In other words, while the public face of Bahnson's institute involves an

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[38] These sound remarkably similar to DeWitt's own concerns on being approached by Glenn Martin. It seems likely that there was an on-going debate about the best environment for generating research following Vannevar Bush's 1954 report on "The independent research institution" (*Physics Today*, **7**: 19).

[39] Feynman letter to Wheeler, dated December 2, 1955: CDWA.

[40] Letter from J. S. Toll to John Wheeler, dated December 28, 1955: CDWA—emphasis mine.

[41] Neither DeWitt nor Palmatier took Bahnson's ideas seriously and would frequently joke around in their analyses of Bahnson's experiments, marking them "top secret!" There was one stage, however, where DeWitt seriously wondered whether Bahnson would drop him as a result of his repeated rejection of the experiments—letter from DeWitt to Palmatier, August 1958: CDWA.

[42] See Yost (1991) for a discussion of the notebooks from this collaboration.

explicit blanket dismissal of anti-gravity research, “The Bahnson Lab” was busily experimenting in precisely this area; indeed, one might call it “the world-centre of anti-gravity research”.[43] Louis Witten recalls visiting this lab when he was at RIAS, assessing Bahnson’s claims that he had “an anti-gravity thing”.[44] Witten was faced with a Dr. Strangelove-esque scene, replete with an operator of a high-voltage machine with hair standing on end and sparks flying around the room. He diagnosed any lift effects as a result of simple ionization. While this sounds like nothing but an amusing anecdote, the influence of anti-gravity on the development of legitimate areas of gravitational physics (including, especially, quantum gravity) should not be underestimated. The bulk of funding largely responsible for the significant infrastructure shift that had to occur before the fruits of the renaissance could be produced was grounded in the (mistaken) belief that anti-gravity (or at the very least some practical applications) were forthcoming. In other words, the lack of knowledge of gravitation in the low-water mark period was both a curse and a blessing.

5 The Military-Industrial-Academic Complex

The year 1957 was notable for another reason beyond the Chapel Hill conference (on the Chapel Hill conference see DeWitt and Rickles 2011): Sputnik was launched, much to the surprise of most Americans. This had a marked effect in the funding of scientific projects, including those involving gravitation, since gravitation was linked to aviation and spaceflight. The Cold War element cannot be underestimated, and key gravitational players were behind the Iron Curtain. Vladimir Fock was in the USSR, Leopold Infeld was in Poland, Achilles Papapetrou was in East Berlin, and Léon Rosenfeld and Jules Géhéniau were considered a security risk. There was some speculation that Fock had managed to develop a new “graviplane” flight technology (based on a “lift anomaly”). This was nonsense, and presumably propaganda.[45]

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[43] Old 8mm footage of Bahnson and Brown and an assistant, J. Frank King Jr. (Bahnson’s brother-in-law), has been publicly released: https://www.youtube.com/watch?v=vWuUJt7iSAo (accessed February 10, 2019)—a youthful, and slightly bemused-looking, Bryce DeWitt makes an appearance in the film at around 14 minutes (this visit occurred on the December 19, 1957); it must have been somewhat disconcerting for DeWitt to see Bahnson dabbling in such ventures, especially knowing full well that the “anti-gravity” phenomena were simple “ionization” effects.

[44] Interview of Louis Witten by Dean Rickles and Donald Salisbury on March 17, 2011, Niels Bohr Library & Archives, American Institute of Physics: www.aip.org/history-programs/niels-bohr-library/oral-histories/36985.

[45] Bahnson, Memorandum No. 1, Feb 3, 1958, CDWA. The source of the story was *American Aviation* magazine, which appears to have been the mouthpiece of Gravity Rand Ltd.—it is difficult to probe the origins of this curious venture (I suspect that Bahnson and T. T. Brown were involved in some way, especially since Brown established a company called “Rand International Limited”)—see footnote 56.

But serious government and military interest in gravitation can be traced back earlier, at least to 1955. Crucially, this is when the Aeronautical Research Laboratories[46] (at the Wright-Patterson Air Force Base outside Dayton, Ohio) decided to expand their support to projects involving general relativity. Dr. Max Sherberg, who was in charge of these decisions (as "Chief of Applied Mathematics"), provided the first ever military funding for gravity to Vaclav Hlavatý, a Czech researcher then based at Indiana University. Sherberg himself was trained as an applied mathematician and had written his doctoral thesis, in 1931, on the degree of convergence of a series of Bessel functions, with Dunham Jackson at the University of Minnesota. However, he had worked on flight modelling (especially modelling flight spins) and produced a report[47] for NACA [the National Advisory Committee for Aeronautics, before it became NASA] before his doctoral work. But it is clear that he didn't have knowledge of general relativity, and it is most likely that he approached Hlavatý, hardly the best general relativity scholar at the time, because of a bit of a media frenzy that had occurred in 1953 on account of Hlavatý solving the equations of Einstein's unified field theory. In a popular TV programme "Johns Hopkins science review", Hlavatý was presented as a great man of science.[48] More importantly, Hlavatý was also mentioned in a revealing story, "Conquest of Gravity Aim of Top Scientists in U.S.", featured in the *New York Herald Tribune* (Sunday, November 20, 1955: pp. 1 and 36—the author was Ansel E. Talbert, the military and aviation editor for the paper). Talbert writes that Hlavatý "believes that gravity simply is one aspect of electromagnetism – the basis of all cosmic forces – and eventually may be controlled like light and radio waves".[49] This idea (and the

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[46] The name was changed to the Aerospace Research Laboratory soon after adding general relativity to its portfolio, and that should give some information about the expectations of the Air Force, in terms of applications, when they added relativity. However, there is also a case to made that the direct Air Force support, with a knowledgeable researcher guiding funding decisions, saved the Air Force the trouble of sifting through many proposals that were scientifically ungrounded (see Kennefick 2007, 116–7, for more on this "protective function" of ARL's gravity funding).

[47] "Mass distribution and performance of free flight models" (NACA Technical Note 268, Oct 01, 1927): https://ntrs.nasa.gov/archive/nasa/casi.ntrs.nasa.gov/19930081026.pdf (accessed March 3, 2019).

[48] One can watch this programme by visiting Johns Hopkins University: https://catalyst.library.jhu.edu/catalog/bib_2405701 (accessed March 3, 2019). Hlavatý's solution of Einstein's unified field equations was a *tour de force* (involving 64 unknowns), and it is clear that the fact that Einstein himself deemed their solution impossible played a role in the media frenzy—physicists were not so taken, simply because they distrusted Einstein's unified field theory approach.

[49] We note that Arnowitt and Deser's GRF essay is also picked up by this article, which quotes the following passage: "One of the most hopeful aspects of the problem is that until recently gravitation could be observed but not experimented on in any controlled fashion, while now with the advent in the past two years of the new high-energy accelerators (the Cosmotron and the even more recent Berkeley Bevatron) the new particles which have been linked with the gravitational field can be examined and worked with at will"—we shall return to this in a moment. The article is full of "gee-whiz" speculation and false claims (such as the claim that "[t]here is no scientific knowledge or generally accepted theory about the speed with which it [gravity] travels across interplanetary space").

story in which it was expressed) was undoubtedly a driving force behind many anti-gravity speculations and funding opportunities: if gravity is just an aspect of electromagnetism, and electromagnetism can be controlled and shielded, then it surely stands to reason that gravity can likewise be controlled and shielded!

Sherberg was asked to find someone to oversee Air Force funding in gravitation. He eventually settled on Joshua Goldberg, a student of Peter Bergmann's, who was then at the Armor Research Foundation.[50] Goldberg joined the staff at the Aeronautical Research Laboratories (as part of the General Physics Laboratory) in September, 1956, just as the Chapel Hill conference was being organized. In addition to overseeing funding, it was also a genuine research group and would later include Roy Kerr. It ran until 1972, when the "Mansfield amendment" put an end to the funding of basic research by the Defence Department.

One of the many elements of serendipity was the fact that the DeWitts had just started the Institute of Field Physics venture around the same time that funding for gravitational projects started at the ARL, transforming its inaugural conference into what would have been a more local affair into a truly international event (not least through the military air transport [MATS] flights offered for non-US participants). As mentioned, Wheeler was consulted by Goldberg and mentioned the Institute of Field Physics as a potential recipient of support. This new funding was also known to Bahnson right away, because of his connections. Indeed, the Institute of Field Physics was the very first recipient of ARL support (for the Chapel Hill conference) and would lead to the earliest subsequent recipients since Goldberg was able to interact with the leading relativists at the conference. Goldberg was also offered a rare opportunity to travel by the ARL, in the US and overseas, in order to investigate potential recipients of grants. Thus, this position came with a built-in method for linking the various researchers together. He interacted, in a short period of time, with Géhéniau and Debever in Belgium, Lichnerowicz in Paris and Jordan in Hamburg. Goldberg was a hub in an emerging network.[51]

There was naturally a condition that funding could not be given to support fundamental research: there had to be some discernible military purpose, however remote. Anti-gravity was the answer. As Goldberg puts it:[52]

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[50] Goldberg has given an account of the history of this episode in Goldberg (1993). He notes that Sherberg first asked several others, including Bergmann himself, but also Pascual Jordan, Géhéniau (presumably then unaware of Géhéniau's communist sympathies), Hlavatý and Rosen. Bergmann had suggested Goldberg to Sherberg. Goldberg himself went on to consult with John Wheeler and Peter Bergmann soon after he was hired in order to determine where support was needed.

[51] In fact, political factors constrained Goldberg in various ways. Géhéniau, for example, could not receive funding despite Goldberg's desire to make it happen since the Air Force forbade it on account of his political leanings—this despite, as Goldberg notes (Goldberg 1993, 95), the fact that they were happy to offer support to Jordan who had been associated with the Nazis (of course, "Operation Paperclip" would overlook many such cases, recruiting thousands of Nazi scientists).

[52] Interview of Joshua Goldberg by Donald Salisbury and Dean Rickles on March 21, 2011, Niels Bohr Library & Archives, American Institute of Physics: www.aip.org/history-programs/niels-bohr-library/oral-histories/34461.

> There were people, and I don't know who — this is one of those hearsay things that nobody can verify, so I will say it, but it's totally unverified — that some officer in the Air Force, thinking about the next big thing that the Air Force needed, was an anti-gravity device. And so they needed somebody to work on general relativity…

There are some very revealing reports from the US Air Force that tell us much about what was in their minds when they decided to fund gravitational projects. The primary target seems to have been "electrogravitics". When we look at these reports, we quickly realize why Hlavatý was the first person to be supported by the Air Force, for it was a naive understanding of Einstein's unified field theory that motivated the decision.

An important example of such a report is entitled "Electrogravitic Systems: An Examination of Electrostatic Motion, Dynamic Counterbary and Barycentric Control" [TL 565 A9: AF Wright Aeronautical Laboratories Technical Library Wright-Patterson Air Force Base, Ohio].[53] This report views gravitation as aviation's enemy, arguing that fundamental research "to discover the nature of gravity from cosmic or quantum theory…would [if successful] change the concept of sustentation, and confer upon a vehicle qualities that would now be regarded as the ultimate in aviation". The report then summarizes past and current work in the field, including, primarily, "electrogravitics". However, also central is Hlavatý, who is heralded as "the most authoritative voice in micro-physics" and is presented as having discovered a "concept of gravity as an electromagnetic force that may be controlled like a light wave". Later on, mention is made of finding and solving the

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[53] The report claims to have been prepared (and finalized on February 25, 1956) by the "Gravity Research Group, Aviation Studies (International) Limited", which gives as its address "London 29–31 Cheval Place. Knightsbridge, London SW7, England (with internal report number: GRG 013/56 February 1956)". Curiously, this address corresponds to residential flats and seems to have been such for some time. It is likely that the "group" in question was a single individual, namely, the director, Mr. R. G. Worcester, a fairly well-known expert on air policy and aviation). So far as I can tell, no other reports from this "group" exist. A declassified Douglas Aircraft Company Inc. report on "Unconventional Propulsion Systems" (from W. B. Klemperer to E. P. Wheaton, dated March 1, 1955: MTM-622, Part 2) indicates correspondence with the research group who are described as "Management Consultants … who prepare and distribute the Aviation Reports discussing technical, commercial and political developments in the world of aviation". The report, however, is something of a favourite amongst UFO conspiracy theorists on the World Wide Web. While their interpretations are somewhat left-field, the report is genuine (it appears in the Library of Congress in an issue of *Aviation Studies*, e.g.) and appears to have been taken seriously. Of course, it is perfectly true that the US Air Force were interested in all manner of propulsion methods, and this included "spaceships" and methods of space travel—even Bryce DeWitt wrote an early report on "The Scientific Uses of Large Spaceships" (*General Atomic Report GAMD* **965**, 1958) and Lyman Spitzer, of the Matterhorn Project, wrote on "Interplanetary Travel" as early as 1954—Robert Serber had already written on the possibility of using nuclear power for rockets as early as 1946 ["The Use of Atomic Power For Rockets": Research Memorandum No. 1. Project RAND, July 5, 1946 (reprinted as Appendix IV to Second Quarterly Report on Project RAND, September 1, 1946, RA-15004)]. But this is simply a natural part of the development of spaceflight, leading, amongst other things, to the creation of NASA and the JPL. Any secrecy is due to the fact that the military wanted to keep its funded research under wraps from its enemies.

equations of the unified field theory that "Einstein hopes to find a way of doing … before he dies" (pp. 1–2).

Also presented is a list of industrial parties interested in "counterbary" (1950s aviation-speak for anti-gravity), including Douglas, Hiller, Glenn Martin (quoted as saying that "gravity control could be achieved in six years, but they add that it would entail a Manhattan District type of effort to bring it about") and Sikorsky (quoted as saying that "gravity is tangible and formidable, but there must be a physical carrier for this immense trans-spatial force"). General Electric is claimed to be working on a way "to make adjustments to gravity", Bell Labs is claimed to have hardware that is able "to cancel out gravity", and Lawrence Bell himself is said to be "convinced that practical hardware will emerge from current programs". There are also companies more connected with navigating the challenges of gravity, such as Lear, Inc., Convair and Sperry (who went on to develop traffic control systems and other digital aviation technologies). In each case, the idea is that if "a physical manifestation exists, a physical device can be developed for creating a similar force moving in the opposite direction to cancel it". This lack of understanding of the modern conception of gravitation thus motivated a surge of investment in gravitational research.

The article goes on to mention research institutes with gravity foci, including the GRF, RIAS (at which the new gravitational wing is mentioned) and the Institute for Field Physics (incorrectly written as the Institute for *Pure* Physics), which also is described as under proposal at this stage.

There is clearly an electrogravitics bias in the article, with T. T. Brown hailed as "the equivalent of Frank Whittle in gas turbines". The author also expresses bemusement as to how the Germans could have possibly overlooked electrogravitics, given how close they were to the Americans with respect to the nuclear programme. Once again, however, it seems that Hlavatý's conception of the unified field theory is playing a major role:

> If Dr. Vaclav Hlavatý thinks gravity is potentially controllable that surely should be justification enough, and indeed inspiration, for physicists to apply their minds and for management to take a risk. Hlavatý is the only man who thinks he can see a way of doing the mathematics to demonstrate Einstein's unified field theory—something that Einstein himself said was beyond him. Relativity and the unified field theory go to the root of electrogravitics and the shifts in thinking, the hopes and fears, and a measure of progress is to be obtained only in the last resort from men of this stature. Major theoretical breakthroughs to discover the sources of gravity will be made by the most advanced intellects using the most advanced research tools.[54]

However, the report sounds the cautious note of the scientific establishment that "nothing can be reasonably expected from the science for yet awhile" but ignores this in favour of the engineering positions, pointing out that "NACA [National Advisory Committee for Aeronautics–DR] is active, and nearly all of the Universities are doing work that borders close to what is involved here, and something fruitful

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[54] "Electrogravitic Systems: An Examination of Electrostatic Motion, Dynamic Counterbary and Barycentric Control" [TL 565 A9: AF Wright Aeronautical Laboratories Technical Library Wright-Patterson Air Force Base, Ohio], p. 12.

is likely to turn up before very long". There is certainly truth in the view that the various aviation institutions believed they were close to stumbling on something big. But there was also explicit recognition that the levels of uncertainty were very large indeed. For example, in a NACA Aviation Report, it is reported that:

> Glenn Martin now feels ready to say in public that they are examining the unified field theory to see what can be done. It would probably be truer to say that Martin and other companies are now looking for men who can make some kind of sense out of Einstein's equations. There's nobody in the air industry at present with the faintest idea of what it is all about. Also, just as necessary, companies have somehow to find administrators who know enough of the mathematics to be able to guess what kind of industrial investment is likely to be necessary for the company to secure the most rewarding prime contracts in the new science. This again is not so easy since much of the mathematics just cannot be translated into words. You either understand the figures, or you cannot ever have it explained to you. This is rather new because even things like indeterminacy in quantum mechanics can be more or less put into words.[55]

That so many ideas that we would consider foolish today were taken seriously by the military and their industrial partners, we should simply see as an indication of the poor state of knowledge in gravitation at the time. When the understanding of gravitational physics is unfavourably compared with the understanding of indeterminacy in quantum mechanics, then that has to be trouble. This explains the rather disconcerting fact that Arnowitt and Deser's spoof GRF entry, relating gravity and nuclear particles, was taken up so strongly in this anti-gravity frenzy. A gravity group (again: members unknown) known as "Gravity Rand Ltd." was formed[56] to consider what the author(s) view as potentially plausible anti-gravity schemes. To this end, a report entitled "The Gravitics Situation" was published in December, 1956. The report states:

> [T]he time is fast approaching when for the first time, it will be within the capability of engineers with tevatrons to work directly with particles that – it is increasingly accepted – contribute to the source of gravitation. And while that in itself may not lead [to] an absorber of gravity, it will at least throw more light on the sources of the power. Another task is [the] solution … of outstanding equations to convert gravitational phenomena to nuclear energy. The problem – still not solved – may support the Bondi-Hoyle theory that expansion of the Universe represents energy continually annihilated instead of being carried to the boundaries of the Universe. This energy loss manifests itself in the behavior of the hyperon and K-particles which would – or might – form the link between the microcosm and Macrocosm. Indeed, Deser and Arnowitt propose that the new particles are a direct link between gravitationally-produced energy and nuclear energy. If this were so, it would be the place to begin in the search for practical methods of gravity manipulation. It would be realistic to assume that the K-particles … are such a link. Then a possible approach might

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[55] NACA Aviation Report (October 19, 1954). Reprinted in (Valone 2004, 41–42).

[56] This group, like the Gravity Research Group, was based in London (based at address: 66 Sloane Street, London, SW1—now housing an exclusive estate agents): it seems obvious that they are one and the same company: Aviation Studies (International) Ltd. (It might be no coincidence that T. T. Brown formed a company known as "Rand International, Ltd.", this just after a spell at a Bahnson-owned company, part of Bahnson Labs, known as "Whitehall-Rand Project"—it seems plausible that Bahnson was connected to Gravity Rand, though I have been unable to verify this).

> be to disregard objections which cannot be explained at this juncture until further Unified Field links are established. As in the case of the spin and orbital theories which were naive in the beginning, the technique might have to accept the apparent forces and make theory fit observation until more is known. Some people feel that the chances of finding such a Unified Field Theory to link gravity and electrodynamics are high. Yet still [they] think that the finding of a gravity shield is slight because of the size of the energy source and because the chances of seeing unnoticed [an] effect seem slender. Others feel the opposite and believe that a link between nuclear energy and gravitational energy may precede the link between the Einstein General Relativistic and Quantum Theory disciplines. Some hope that both discoveries may come together while a few believe that a partial explanation of both may come about the same time. Which will afford sufficient knowledge of gravitational fields to perfect an interim type of absorber using field links that are available. The latter seems the more likely since it is already beginning to happen. There is not likely to be any sudden full explanation of the microcosm and Macrocosm but one strand after another joining them will be fashioned as progress is made towards quantizing the Einstein theory.[57]

Again, we see how two strands reassert themselves: on the one hand, there is the Arnowitt-Deser idea, now explicitly mentioned, and on the other hand, there is Hlavatý's idea. Both do indeed suggest the possibility of anti-gravity since they suggest a deeper unity between gravity and other forces that are controllable. It is clear that these ideas led to gravitational project funding from a range of sources with the expectation of technological advances.

The report includes the following interesting aside on funding in this period and on the role of "the nuclear experience":

> It is a common thought in industry to look upon the nuclear experience as a precedent for gravity and to argue that gravitics will similarly depend on the use of giant tools beyond the capabilities of the air industry. And that companies will edge into the gravitational age on the coat tails of the Government as industry has done – or is doing – in nuclear physics. But this overlooks the point that the 2 sciences are likely to be different to their investment. It will not need a place like Hanford or Savannah River to produce a gravity shield or insulator once the know-how has been established. As a piece of conceptual engineering, the project is probably likely to be much more like a repetition of the turbine engine. It will be simple in its essence. But the detailed componentry will become progressively more complex to interpret in the form of a stable flying platform. And even more intricate when it comes to applying the underlying principles to a flexibility of operating altitude ranging from low present flight speeds at one extreme to flight in a vacuum at the other. This latter will be the extreme of its powers.

The author speaks as if the production of a gravity shield or insulator (of the kind Babson envisaged and was so humiliatingly vilified by Gardner for proposing) is a foregone conclusion given enough effort and support. As mentioned above, however, we shouldn't be too hard on the folks who bought this line of thinking

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[57] As cited in (Valone 2004, 59–60) (the entire report is reproduced in this collection). The report cites the works of Bryce DeWitt, including his PhD thesis. This, it would seem, must be someone who was aware of DeWitt himself. My sense is that it was likely written by Bahnson himself, or else by T. T. Brown with whom he was working at the time—or perhaps the pair of them together.

given that the anti-gravity suggestions were coming from reputable sources (or so they believed).[58]

There's serendipity at several levels in this story, but ultimately the lesson is that poor understanding of classical and quantum gravity led to the funding opportunities that transformed it. As this transformation continued, improving knowledge, the unusual funding opportunities were pruned. Beyond serendipity, however, it was vital that people with good sense and training (such as Wheeler and Goldberg) were in control of major decisions at key moments.

Finally, one aspect that we have not delved into here, and is often ignored in historical studies of gravitation, is the *commercial* involvement in gravity through its impact on geology and geological features such as oil deposits: oil is often to be found in areas of anomalous density which are associated with variations in the strength of gravity on the Earth's surface above them (itself a result of the oblate structure of the Earth). One can, in this way, map subsurface geology. High-precision gravitational measurements were used to prospect for oil (the first being conducted in Czechoslovakia in 1915) and were rather successful at a time when few other technologies were available. This work in itself led to advances in technologies used to measure gravity (such as better pendulums, torsion balances and the "gravimeter").[59] Taken together with the other industrial and military work on gravity, it reveals that despite the lack of knowledge of gravitation in the mid-1950s and earlier, it was nonetheless entangled with a great variety of endeavours, practical and theoretical, each of which contributed to the renaissance.

6 Conclusion

At around the same time in the mid-1950s, gravitational physics was suddenly supported in a variety of quite distinct, though inter-connected, ways and through significant sources located in America. These can be viewed as "experiments" in institutional design: some (such as the GRF) were more a "virtual" presence than a physical institution, offering little more than background support for gravitational projects. Others (such as RIAS) were linked to future practical applications. Still others (such as the Institute for Field Physics) were sculpted along more traditional lines, utilizing the benefits of a university. Ultimately, the most successful was the most conservative: the Institute for Field Physics. However, all played a role and

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[58] We should note that there was at least *some* scope for a discussion of gravity shields even in the context of general relativity. For example, in 1956, Peter Bergmann (on an Air Force research grant, but discussed in print in Bergmann 1957) considered the possibility of gravity shields via negative mass particles (which, though not likely to exist, were not ruled out entirely). Bergmann showed that only in the case where the negative mass particle possessed an electric charge would such a particle be polarized gravitationally, by applying an electric field. But even in this case, the shielding effect would be so small as to offer no benefit.

[59] See Eckhardt (1940) for a historical review of this work.

were vital in re-establishing gravitational physics by generating wider interest and managing to attract funding from often surprising sources, as well as, eventually, more standard funding. What is so ironic about this story is that although antigravity speculations clearly fuelled much of the earliest funding of gravitation, those that benefitted most (the physicists!) actively and explicitly opposed the idea.

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The Fock-Infeld Dispute: An Illustration of the Renaissance of General Relativity in the Soviet Union

Jean-Philippe Martinez

1 Introduction

Physicists discussed the problem of the motion of bodies in the theory of general relativity extensively throughout the twentieth century. In his 1989 paper on "The Early History of the 'Problem of Motion' in General Relativity," Peter Havas summarized its birth as follows:

> [. . .] in the case of the absence of any nongravitational fields, Einstein postulated (in the initial 1913 form of the theory and its final 1916 form) that the motion of a mass point should follow a geodesic in four-dimensional space-time. [. . .]
>
> The geodesic law was taken to be exact for a test particle, i.e., a particle that is subject to the effects of a gravitational field, but whose own effect on the field can be neglected, so that the "force" acting on the particle was determined by solving Einstein's field equation in the absence of the particle. Whether the geodesic law also holds for a particle whose effects on the gravitational field cannot be neglected was not discussed initially. It was clear, however, that for the case of several bodies of comparable masses Einstein's (nonlinear) field equations could not be expected to permit an exact solution and thus no universal force law analogous to Newton's law of gravitation could be hoped for, but only an approximate one, which in the lowest order should agree with Newton's (Havas 1989, 235).

As a direct consequence of the need for an approximation, the problem of motion remained discussed during the period defined by Jean Eisenstaedt (1986, 1989) as the "low water mark" of general relativity.[1] The first major achievement in this

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[1] See also Chap. 1 in this volume for a historiographical discussion of the "low water mark" of general relativity.

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direction is generally attributed to Einstein and Grommer (1927).[2] They contributed to the recognition that in general relativity the laws of motion cannot be specified independently of the field equations. The subsequent derivation of approximate equations of motion for several particles of comparable masses is commonly ascribed to Einstein, Infeld, and Hoffmann (1938) in their paper "Gravitational Equations and the Problem of Motion."[3] A year later, in 1939, the exact same problem was also solved independently by the Soviet physicist Vladimir Fock, who published his results in Russian and French (Fock 1939a, b).[4] However, his approach proved to be different from that of the trio. It laid the foundations for an interpretation of general relativity which turned out to be different from Einstein's, and, for this reason, Fock came into direct confrontation with Leopold Infeld in the course of his career.

Vladimir Fock (1898–1974) is nowadays a familiar name to many physicists, especially those busy with quantum theory. The Hartree-Fock method of approximation for the determination of the wave function in many-body problems (Fock 1930) and the Fock space used to describe quantum states in situations where the number of particles is not fixed (Fock 1932) are recognized as fundamental contributions to quantum physics. From the 1930s, Fock was regarded as a theoretical authority in the Soviet Union on domains as various as quantum mechanics, the physics of continuous media, and also general relativity following his 1939 article.[5] While Fock spent his entire career in Leningrad (St. Petersburg), Leopold Infeld (1898–1968) was a Polish physicist with a somewhat unusual trajectory.[6] Jewish, he left Poland for England in 1933, before leaving for the United States in 1936. At Princeton, he became Einstein's assistant. The collaboration between the two men—that notably lasted more than a decade on the problem of motion—fostered friendship between the two as well as the sharing of ideas.[7] Indeed, in the field of general relativity, Infeld became not only a figure of authority but also one of the main defenders of Einstein's thought. Appointed Professor at Toronto University in

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[2] See Havas (1989, 237–254), who detailed some contributions made to the problem of motion before Einstein and Grommer's article.

[3] Havas also discussed how various results of the 1938 article had been previously established by other contributors (Havas 1989, 254–265).

[4] The Russian version was received by *Zhurnal Eksperimental'noy i Teoreticheskoy Fiziki* on February 13, while the French one was received by *Journal of Physics—USSR* on February 26. *Journal of Physics—USSR* was a Soviet journal edited in Moscow that published articles in languages others than Russian. For convenience, we will refer here to the French version. Note also that in this chapter, all translations from non-English sources are ours unless otherwise stated.

[5] About Fock's career, one can consult his unique biography published in Russian by Larissa Vladimirova (2012). See also the PhD dissertation of the present author (Martinez 2017).

[6] Infeld wrote two autobiographies. The first edition of *Quest: An Autobiography* appeared in 1941 (Infeld 1980), while *Why I Left Canada* was published postmortem in 1978 (Infeld 1978). Together, they give a complete overview of Infeld's life.

[7] On the relations between Einstein and Infeld, one can consult *Quest: An Autobiography* (Infeld 1980). See also Stachel's comments on their correspondence (Stachel 1999).

1938, he returned to his native country in 1950 and became Professor at Warsaw University.[8]

Our chapter is not intended to give a complete report of the differences of approach to the problem of motion by the various actors here studied. Only a few hints, in particular regarding Fock's work, will be necessary. Indeed, as part of the investigations proposed by the present volume, we wish to focus on sociocultural aspects. Through the dispute between Infeld and Fock that arose from the problem of motion, we will study the role played by the latter in the process of what historians describe as the "renaissance" of general relativity and put forward some of the peculiar specificities of the Soviet Union context.[9] In particular, the wide diffusion of dialectical materialism, the official ideology in the USSR, throughout all strata of society pushed Soviet scientists to involve themselves more with interpretations of modern physical theories.[10] Fock was well known for his endorsement of dialectical materialism, to which he had connected his understanding of general relativity and quantum theory. In addition, he sometimes raised his voice against philosophical attacks aimed at modern physics in the Soviet Union. This general debate involved not only scientists from different backgrounds but also Marxist philosophers. The unique context of the Soviet Union in the history of general relativity theory is then illustrated by the place in which it was debated during its "low water mark": the philosophical journals. It is also important to underline that from the mid-1930s to the mid-1950s, just before the renaissance of general relativity, the USSR had been progressively closed off, an isolation deeply felt by its scientists.[11] Infeld, returning to Poland in 1950 soon after it had come under the Soviet sphere of influence, would have appeared as an exceptional interlocutor for Fock. His international stature and his knowledge of general relativity were undeniable, giving weight to the Fock-Infeld dispute at the moment of transition from the "low-water mark" and "renaissance" of general relativity. Already, from 1955, the debate had spread to the international community and was widely discussed by various physicists.

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[8] See Infeld (1978, 39–54). The return of Infeld to Poland was partially motivated by the anticommunist atmosphere in North America at the beginning of the Cold War. Unfairly perceived as a potential traitor to Canada and suspected of being capable of delivering nuclear secrets to the communists, Infeld made the choice to return to his native country in order to contribute to the development of science.

[9] In the spirit of the present volume, we refer to Blum et al. (2015) when we mention the process of the "renaissance" of general relativity.

[10] This process is clearly perceptible in several works of reference on the Soviet history of science, such as Joravsky (1961), Graham (1987), or Josephson (1991).

[11] The progressive isolation of Soviet physicists in the 1930s has been discussed by Josephson (1988). The reopening of the USSR then followed Stalin's death, in the context of the Cold War. Notably, in view of the growing nuclear threat, Nikita Khrushchev plied the foreign policy of "peaceful coexistence" between two opposing economic and political systems during the 20th Congress of the Communist Party in 1956. See Ivanov (2002), Richmond (2004), Mastny (2010), or Hollings (2016).

To present this trajectory, this chapter proceeds as follows: Sect. 2 gives the main features of Fock's approach to the problem of motion and of his unorthodox views on the interpretation of general relativity. Section 3 shows how the dispute between Fock and Infeld took shape at the beginning of the 1950s and received special exposure in philosophical journals. Finally, Sect. 4 is devoted to the process that gave the debate an international resonance during the Bern Conference in 1955 and discusses what made Fock a key figure in the process of the renaissance of general relativity in the Soviet Union.

2 Fock's Unorthodox Views on General Relativity

In 1939, Fock learned about Einstein, Infeld, and Hoffmann's article on the problem of motion while his own was ready for publication. Immediately, he wrote an addendum to underline that the two approaches were different (Fock 1939b, 115–116). Later, in a monograph dedicated to the theory of relativity, Fock even placed this contrast at the origins of an opposition between two "schools":

> The development of the ideas laid down in the paper by Einstein and Grommer in 1927 began in the years 1938–1939 quite independently in two directions. The work of Einstein, Infeld and their collaborators is concerned with the one development, the work of Fock and his collaborators with the other; the papers of Papapetrou, which appeared after 1950, can also be counted as going in the second direction (Fock 1964, 397).

This observation was accompanied by comments that shed light on the differences between the two approaches. First, Fock had made use of a harmonic coordinate system that he considered a privileged system of reference. Second, the authors were positioned differently on the question of the mass tensor: Einstein and his colleagues considered masses as singularities while Fock attributed finite dimensions to them. These elements became pillars of Fock's interpretation of general relativity.

The choice to work with harmonic coordinates and their defense by Fock throughout his career are among the topics concerning his contributions to general relativity that have been most discussed not only in the scientific literature but also by historians of science.[12] In 1939, they were introduced by Fock as follows: in general relativity, a system of coordinates is considered harmonic if it possesses the property

$$\Box x_\upsilon = 0,$$

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[12] See Graham (1982, 1987), Havas (1989), and Gorelik (1993). A full exposition of Fock's ideas on general relativity requires a more detailed account than that given in this chapter. The reader can refer in particular to Graham (1987), Gorelik (1993), and the PhD dissertation of the present author (Martinez 2017).

where $\Box$ is the d'Alembert operator, the generalization of the Laplacian from three-dimensional Euclidean space to four-dimensional Minkowski space. Fock was actually following a perspective explored by Théophile de Donder and Cornelius Lanczos in the early 1920s, two physicists that he directly mentioned in his article (Fock 1939b, 85).[13] But the Soviet physicist was particularly enthusiastic about this procedure and underlined the "very great advantage of the harmonic coordinate system over all other systems" (Fock 1939b, 87). It was the case not only from a mathematical point of view, because it allowed for simplifications to the expression of the Ricci tensor which defines the curvature of space-time, but also, according to Fock, from a physical point of view:

> In a theory where almost arbitrary coordinate transformations are permitted [...], it sometimes becomes difficult to give an intuitive interpretation of the parameters we use for coordinates. These difficulties are considerably reduced if we use the harmonic coordinate system. These coordinates are the ones whose properties are as close as possible to Cartesian properties and to the ordinary time of Minkowski space-time. This is why the formulas of general relativity, expressed by means of these coordinates, excel by the ease of their physical interpretation (Fock 1939b, 88).

For the specific problem studied in 1939, Fock even advanced the hypothesis that the harmonic coordinate system could be assimilated to a Galilean (inertial) system valid for general relativity. Indeed, it seemed to him highly probable that this "coordinate system is uniquely determined (to a Lorentz transformation, the same in all space) if we take into account the conditions that require Euclidean space to infinity, as well as the Harmonicity conditions" (Fock 1939b, 114).[14] Clearly, Fock's discourse emphasized the privileged character of harmonic coordinates over all other coordinate systems.

By remaining at the stage of hypothesis, it must be noted that Fock's developments in 1939 proved to be unsatisfactory on some aspects. Nevertheless, the resolution of the problem of motion was accompanied by a clear desire to rethink the physical meaning of the theory of general relativity. It resonated as a call to open a new research program.[15] Fock's wish was to give physical content to a theory he sometimes described as "abstract" or "formal" (Fock 1939b, 83 and 115). There, he echoed the widespread perception in the community of physicists that general relativity, if not considered a true physical theory, would have become from the 1920s a true "mathematical fiefdom."[16] It can explain why, as early as 1939, Fock was motivated to treat masses differently from Einstein, Infeld, and Hoffmann, who worked with singularities. The Leningrad physicist felt their choice

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[13] Fock referred to de Donder (1921) and Lanczos (1922). Nowadays, physicists mainly use the denomination "harmonic gauge," even if they occasionally use "de Donder gauge" or "Fock gauge."

[14] This condition was decisive in the case studied. As we will discuss it, for Fock, each privileged system of coordinates was dependent on the type of space studied.

[15] See Gorelik (1993, 315) that shares this idea.

[16] The expression, used by Jean Eisenstaedt, was borrowed from Marie-Antoinette Tonnelat (Eisenstaedt 1986, 165).

was influenced by "Einstein's former point of view that the ultimate goal of the theory [was] to explain elementary particles as singular points of the field" (Fock 1939b, 115). Indeed, Einstein considered general relativity as an intermediate step toward a unified theory of gravitational and electromagnetic interactions which was the real object of his research program.[17] For his part, Fock did not share this point of view at all: in order to give more physical content to the theory, he approached general relativity as a pure "theory of gravitation," strictly limited to gravitational phenomena playing a preponderant role first and foremost on the astronomical scale.[18] As a result, when the Soviet physicist stated the basic physical assumptions of the problem of motion, he considered "celestial bodies in empty space" (Fock 1939b, 89)—in other words, massive bodies with finite dimensions. This inevitably led Fock to introduce the mass tensor into his equations, while Einstein, Infeld, and Hoffmann worked with singularities and used the vacuum Einstein equations.

This article became a cornerstone of Fock's interpretation of general relativity, which can be summarized as follows: "physical relativity cannot be general and general relativity cannot be physical" (Fock 1966, 212). In this direction, Fock was actually guided by a very specific conception of the principle of relativity which materialized what he regarded as a misinterpretation of general relativity. Originally discovered by Galileo for mechanical phenomena, the principle of relativity was extended with special relativity to electromagnetic phenomena. To simplify, it affirmed that physical laws express themselves identically in all inertial reference systems. In general relativity, Einstein's idea of generalized covariance of equations is traditionally associated with a generalization of the relativity principle to include gravitation. The laws of physics would express themselves identically in all reference systems, whether inertial or not. Thus, it is from this idea that the theory developed by Einstein in 1915 took its name "general relativity." However, for Fock, the principle of relativity was "extremely closely related to the concept of uniformity" (Fock 1964, 6) and, therefore, unsuited to the "generalized" theory of Einstein which dealt with a non-uniform spacetime.[19] To argue in this direction and

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[17] See Eisenstaedt (1986, 165–178), Goenner (2004, 2014), Sauer (2007), van Dongen (2010), or Bergmann (1982) for more general accounts of the role played by general relativity in the quest for a unified theory.

[18] One could object that Fock contributed directly to the research toward a unified theory at the end of the 1920s. In 1926, he proposed a generalization of the Klein-Gordon equation in a curved space-time (Fock 1926). In 1929, he also contributed to the generalization of the Dirac equation and the development of spinors (Fock and Ivanenko 1929a, b; Fock 1929a, b). However, at that time, Fock was primarily motivated by a pragmatic approach to solving mathematical problems in physics. A decade later, the physicist was much more involved in problems of interpretation and he approached theories as an epistemological antireductionist. As a consequence, general relativity could no longer be considered in a unifying perspective because it was unable to explain phenomena at the atomic scale. See Martinez (2017).

[19] A uniform—Galilean or homogeneous—space was defined by Fock as follows: "(a) All points in space and instants in time are equivalent. (b) All directions are equivalent, and (c) All inertial systems, moving uniformly and in a straight line relative to one another, are equivalent (Galilean principle of relativity)" (Fock 1964, 1).

fulfill his desire to give more physical content to the theory, the Soviet physicist used to rely on an extended principle of relativity that he sometimes called the "principle of physical relativity". In order to achieve relativity, he found it necessary that not only physical laws express themselves identically in all inertial reference systems, but also the associated "physical conditions" (Fock 1966, 207).

Mathematically, this implies taking "into account all the equations that determine a physical phenomenon and not just the differential equations of the field" (Fock 1966, 208). In general relativity, this prerequisite is not fulfilled by general covariance; under a change of coordinates, the covariance of the equations concerns only the functional relations, not the initial and boundary conditions that are modified. Fock even made the observation that almost any type of equation can be expressed in a covariant form as long as the addition of auxiliary functions is allowed. This is the case for Lagrange equations in classical mechanics and precisely what happens in general relativity with the introduction of the geometric tensor.[20] Therefore, the Soviet physicist regarded covariance as a mathematical artifice having the heuristic advantage of limiting the variety of possible forms of equations and thus making their choice easier. So, physically speaking, "[. . .] covariance of equations in itself [was] in no way the expression of any kind of physical law" (Fock 1964, 5). In other words, Fock considered that covariance had no physical value and was insignificant in order to interpret the theory.

Thus, to preserve relativity, the Leningrad physicist emphasized the need to consider "space as a whole" and to pay particular attention to initial and boundary conditions.[21] Indeed, for Fock it would be necessary to introduce a physical change in the phenomenon to restore its shape after a coordinate transformation, "for example, if it is a simple rotation of the space axes and if the phenomenon considered is reducible to the rectilinear motion of a material point, the movement along the original X axis must be replaced by a movement along the new X axis" (Fock 1966, 208). This process, which restores the mathematical expression of a physical phenomenon under a coordinate transformation, was called the "principle of physical adaptation" by Fock. It was connected to the concept of invariance: the principle is satisfied only when all the functions describing a phenomenon behaved like invariants after a change of coordinates. According to the Soviet physicist, "physical relativity" was obtained only if "physical adaptation" was verified. The initial covariance requirement of the equations was thus extended. In the case of Lorentz transformations in special relativity, physical adaptation is always assumed to be possible for all the fields studied. Such possibility explains why physical relativity was for Fock a property linked to the uniformity of the space. In the case of arbitrary transformations of the coordinates, physical adaptation is not always possible. Despite the covariance of the equations, physical relativity no longer takes place: "the 'principle of general relativity' (if given the only non-contradictory

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[20] This argument is part of Fock's introduction to his monograph on the theory of relativity (Fock 1964, 4–5).

[21] (Fock 1964, 2–3).

interpretation possible of general covariance) is therefore purely formal and not physical" (Fock 1966, 209).

Fock believed that he had shown that the physical meaning usually given to the theory of general relativity was not justified. To him, in interpreting "general relativity" as "covariance of differential equations for an arbitrary change of coordinates," Einstein lacked physical rigor. He thus called for abandoning the name traditionally attributed to the 1915 theory, which resulted from "the incorrect interpretation of the theory by its author" (Fock 1966, 206). According to his opinion, "theory of gravitation" or "chrono-geometric theory of gravitation" was more appropriate to reflect the true content of the theory.[22] The name of "relativity" remained for its part legitimate for the theory of the special relativity, whose qualifier became by this occasion useless.

In the methodological approach of general relativity, Fock's positions—in particular, the necessity of considering space as a whole—had several consequences. Notably, they explain his constant criticism of the principle of equivalence to which he preferred the law of the equality of inertial and gravitational mass, which he called "Galileo's law." Indeed, this second is not confined to local considerations but it also "avoids dealing with frames of reference in accelerated motion—the precise definition of which is not easy to make" (Fock 1964, 3). If Fock readily admitted the heuristic character of the principle of equivalence in the elaboration of general relativity, he also considered that by giving it too much importance, Einstein had drawn wrong conclusions about the theory.[23] As already discussed, to avoid them, the Soviet physicist rigorously defined physical problems, as well as their initial and boundary conditions. In *The Theory of Space, Time, and Gravitation*, a monograph on the theory of relativity written by Fock, one type of space configuration, to which the 1939 article on the problem of motion applied, was principally examined, "masses and their gravitational field [of local character] embedded in unlimited Galilean space."[24] But the Leningrad theorist also briefly discussed a second configuration, known as "Friedmann-Lobachevski space" (Fock 1964, 3). Initially considered by Alexander Friedmann,[25] it obeys the geometry of Lobachevski and posits a uniform distribution of masses throughout space—the existence of a well-defined gravitational field is permitted when the average density of the ponderable matter is different from zero. As a consequence, it allows

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[22] The idea of "chrono-geometry" was borrowed by Fock from the Dutch physicist A. D. Fokker (1955).

[23] In his words: "As for the principle of equivalence, it probably indicated to Einstein the way leading to the solution of the problem of gravitation" (Fock 1966, 213). For the role of the equivalence principle in the development of general relativity, see Norton (1989).

[24] The first edition was published in Russian in 1955 (Fock 1955). However, for convenience, we refer in our developments to the second English edition, widely distributed around the world (Fock 1964).

[25] Well known for his pioneering theory that the universe was expanding, Friedmann was in Leningrad a teacher of Fock at the beginning of the 1920s. See his biography by Tropp et al. (1993).

the description of a realistic universe of a different scale to the case considered as Galilean to infinity. Then, like with harmonic coordinates in this latter configuration, Fock did not exclude in his monograph privileged systems of reference for the Friedmann-Lobachevski space.[26]

The role of such systems was actually to facilitate the physical adaptation of the system and therefore to maintain physical relativity. For example, with harmonic coordinates under the Lorentz transformations, "the metric tensor returns to its original form after an adaptation of the gravitational field by a suitable choice of the distribution and the motion of the masses" (Fock 1966, 209). Thus, if Fock denied the idea that Einstein's theory represents a generalization of the principle of physical relativity, he did not exclude that for certain classes of non-Galilean spaces, it can be preserved using a judicious choice of coordinates. Indeed, the latter could allow an effective implementation of the principle of physical adaptation in addition to the covariance of the equations. The defense of this idea sparked a dispute between Fock and Infeld in the 1950s.

3 A Scientific Debate Incubated in Philosophical Circles

Fock and Infeld met for the first time in 1952 at a conference in Spała, Poland. Although the event was national in scope, a delegation of Soviet colleagues was invited by the Polish Ministry of Science and Higher Education.[27] Chosen by the Academy of Sciences of the USSR, the delegation consisted of N. V. Belov, R. A. Chentsov, and V. A. Fock.[28] In Spała, Fock gave two talks based on works he had already published in the Soviet Union in 1950 and 1951 (Fock 1950, 1951). The second talk dealt with quantum mechanics and was a critique of Bohr's interpretation of the theory: "Kritika vzglyadov Bora na kvantovuyu mekhaniku" (Criticism of Bohr's views on quantum mechanics). The first talk concentrated on general relativity: "O probleme dvizheniya mass v teorii tyagoteniya Eynshteyna" (On the problem of motion of masses in the Einsteinian theory of gravitation). This article was not only a presentation of Fock's results on the problem of motion but also a full account of his views on the theory, as we have just summarized. We can imagine, then, that Infeld, protector of those ideas of Einstein under attack, understandably began to argue with the Soviet physicist after his presentation in Poland, a dispute that would last a number of years. Later, the general atmosphere of

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[26] "It is probable that in Friedmann-Lobachevsky [sic.] space there also exist some preferred systems of coordinates" (Fock 1964, 4). However, it has to be noted that it remained an open question as, during his career, Fock was not able to confirm this hypothesis formally.

[27] See *Why I Left Canada*, second of Infeld's autobiographies (Infeld 1978, 77).

[28] A report detailing their activities during their stay in Poland is available in the St. Petersburg section of the archives of the Russian Academy of Sciences (ARAN SPb)—ARAN SPb, 1034-2-170.

the exchanges between Fock and Infeld was summarized by the latter who reported not only the courtesy of the two men but also their obstinacy:

> He [Fock] was dogmatic and very much believed in his own convictions. I may say that, during this time, I became friendly with Fock. Our relations were better than correct. I argued with him, tried to convince him that he was wrong, but of course without result (Infeld 1978, 77).

Initially verbal, the debate between Fock and Infeld was quickly put into writings, especially after the former published in *Priroda* an article titled "Sovremennaya teoriya prostranstva i vremeni" (Modern theory of space and time) (Fock 1953b). Indeed, it was translated in Polish by the journal *Myśl Filozoficzna* (Fock 1953c) and, according to Infeld's autobiography *Why I Left Canada*, provoked a somewhat indignant reaction:

> It [Fock's article] contained his own interpretation of relativity theory. From this work it would appear that he had invented the principles of motion in relativity theory and that we had committed plagiarism in publishing them a year later. Actually not a year, but nine years earlier we were supposed to have committed a double plagiarism! This was a bit too much. I wrote a short letter to the editor of *Philosophical Thought* [Myśl Filozoficzna], putting the matter straight. I heard later that the discussion in the editorial board lasted all night as to whether to include the letter, but in the end common sense prevailed and it was published. Then I wrote an article in defense of Einstein and it, too, after some discussion, was printed in the same periodical (Infeld 1978, 80).[29]

In the passage of the Russian edition of Fock's article directly incriminated by Infeld, one can indeed observe the propensity of the Soviet physicist to put forward his own contributions (Fock 1953b, 24). Moreover, Fock had not been precise on dates and omitted to include that Einstein, Infeld, and Hoffmann published an article in 1938 on the problem of motion prior to his own (Einstein et al. 1938; Fock 1939a, b).

Infeld's account, however, does not provide us with a clear account of the course of events either. Part of the confusion came from the publication in 1949 of two articles, respectively, by N. M. Petrova (1949) and by Einstein and Infeld (1949). While Fock's article of 1939 was limited to the first approximation (Newtonian), he announced at that time that the second approximation would be treated in a book by Petrova, one of his doctoral students.[30] However, if she defended her thesis in 1940, she was not able to publish her results until 1949. Meanwhile, Einstein and Infeld had already delivered results in both approximations as soon as 1938 with Hoffmann and their publication of 1949 aimed at giving some clarifications to this work. In 1953, Fock made a direct reference only to this last work that he commented as follows: "However, these authors [Einstein and Infeld] give no explication to [. . .] the curious fact that their associated equations coincide with

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[29] Infeld referred to his article "Kilka uwag o teorii względności" (Some remarks on the theory of relativity) published in 1954 (Infeld 1954a).

[30] Fock defined the second approximation as the one which "allows to replace the generalized d'Alembert operator by the ordinary operator" (Fock 1939b, 115).

the equations of Fock-Petrova, written in a harmonic coordinate system" (Fock 1953b, 24). As a consequence, Infeld may have seen in this remark a suspicion of plagiarism from Fock. Nevertheless, a different reading of the Soviet's writings can be made, especially if we take into account that he regarded the "curious" aspect of the question on a strictly methodological level:

> The explication, according to us, has to be searched in the fact that according to the method of arbitrary functions of Einstein and Infeld, we recourse to little variations of the coordinate system, which, in the first approximation, do not concern the movement equations, as they can be obtained from the variational principle (Fock 1953b, 24).

In this sense, Fock was simply opening a scientific debate about the similarity of equations obtained using different methods.

To better understand Infeld's reaction to Fock's 1953 article, and also the manner in which he presented the events, one cannot ignore sociocultural considerations. Infeld's autobiography sought to stress the particularities of his communist context, rather than to engage with the scientific content of his debate with his Soviet colleague.[31] The insistence on an alleged accusation of plagiarism supported the description of the particular atmosphere Infeld lived in Warsaw and his being regularly the object of dogmatic attacks. In reality, he faced a similar situation to Fock and many of his Soviet colleagues.[32] The Leningrad physicist was himself fighting against what he called the "ignorant criticism" of some Marxist philosophers who regularly attacked the modern theories of physics and their main supporters in the USSR.[33] This situation was greatly influenced by the political context of that time, shaped by the beginning of the Cold War and by the *Zhdanovshchina*, a cultural doctrine developed by Central Committee Secretary Andrei Zhdanov in 1946.[34] The politician claimed that the world was divided into two camps, the "imperialistic" headed by the United States and the "democratic" headed by the Soviet Union. His aim was to reestablish after World War II the ideological primacy and the authority of the Communist Party. He was particularly interested in reinforcing the dominant ideological current from the 1930s, dialectical materialism, and began the most intensive ideological campaign in the history of Soviet scholarship.[35]

Some Marxist philosophers took this opportunity to try to impose their views on modern science. However, for a theory like relativity, many attacks were of

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[31] The subtitle of *Why I Left Canada* is after all *Reflections on Science and Politics*. Also, concerning Fock: "Some role in changing my position with officialdom was played by a discussion I had with Professor [Vladimir Alexandrovich] Fock" (Infeld 1978, 75).

[32] On the Soviet context related to the question of science, political aspects are notably discussed by Krementsov (1997). For more detail on physics, more particularly, see Vizgin (1999), Kojevnikov (2004), and Sonin (2017).

[33] The expression comes from the title of an article by Fock discussed below: "Protiv nevezhestvennoy kritiki sovremennykh fizicheskikh teoriy" (Against the ignorant criticism of modern physical theories) (Fock 1953a).

[34] For more detail on the *Zhdanovshchina*, see Ra'anan (1983) or Fitzpatrick (1992).

[35] Interesting developments concerning Soviet science and philosophy in the inter-war period can be found in Joravsky (1961) and Josephson (1991). See also footnote 32.

rather poor intellectual content, originating most often from incompetent people with a shallow if not outright wrong understanding of physics.[36] Relativities of length, time, or simultaneity were sometimes condemned, simply because of their postulated incompatibility with the objective materiality of the world required by Soviet philosophy. At the heart of this conflict, Fock was in permanent opposition to Aleksander Maksimov, a prominent figure of the Soviet philosophy of natural sciences since the 1930s. During this decade, the philosopher had already attacked the physicist for his approach to quantum mechanics, seen as idealistic because of its proximity to Bohr's point of view.[37] It resulted in an article in 1938 by Fock that aimed at proving the compatibility between his views on quantum mechanics and dialectical materialism, to which he claimed his adherence (Fock 1938). There, Maksimov was directly attacked for his incapacity to understand modern physics.

This episode was only the first warning of what happened in the 1950s, which would focus on the theory of relativity. In 1952, Maksimov co-edited a volume titled *Philosophical Problems of Modern Physics*—also known as the "Green book"—published by the Academy of Sciences of the USSR (Maksimov et al. 1952). It generally carried a sentiment against modern physics. Moreover, the philosopher published in the newspaper *Krasnyy Flot* an article titled "Protiv reaktsionnogo eynshteynianstva v fizike" (Against reactionary einsteinism in physics), where he explicitly attacked the theory of relativity and called for its rejection (Maksimov 1952).[38] Fock did not wait long to react.[39] He first managed to obtain from the Academy of Sciences a statement that the opinions expressed in the "Green book" were not mirroring an official position. Next, he published in *Voprosy Filosofii* a virulent article, "Protiv nevezhestvennoy kritiki sovremennykh fizicheskikh teoriy" (Against the ignorant criticism of modern physical theories), that denounced the incapability of Soviet philosophers to deal with scientific subjects (Fock 1953a). Maksimov was personally criticized. As he did for quantum mechanics in 1938, Fock also claimed the compatibility of the theory of relativity with dialectical materialism. There, he relied on his approach of general relativity. Indeed, his

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[36] For more on the reception of Einstein's ideas in the Soviet Union, see Vucinich (2001) and Graham (1987, 354–363). See also Kojevnikov's review of Vucinich's book (Kojevnikov 2002).

[37] In 1936, while translating the articles of the EPR debate in Russian, Fock clearly took a stand for Bohr (Fock 1936). He also criticized his colleague K. Nikol'skiy for developing a statistical interpretation of the theory (Nikol'skiy 1936; Fock 1937). This set led to a sharp and generalized criticism of Fock by the most reactionary fringe of physicists and philosophers. For more on this peculiar atmosphere, see Gorelik (1990), Vizgin (1999, 1264–1265), or Martinez (2017, 304–317).

[38] This newspaper, belonging to the Soviet Navy, was an unusual place for this type of publication. It actually testifies that Maksimov had difficulties to publish his article, which, however, did have a noticeable echo in the community. See Sonin (1991) and Blokh (1997).

[39] For more on this episode, see Blokh (1997) or Vizgin (1999). See also Ilizarov and Pushkareva (1994) who published the most important letters exchanged with the authorities during the events.

desire to give physical content to the theory and his realistic approach to gravitation problems were consistent with the requirements of materialism.[40]

In the process of publication of his article, Fock had to deal directly with the authorities, notably Georgy Malenkov, Chairman of the Council of Ministers from Stalin's death to February 1955. To convince him, he received support from various physicists. Some of them, as Igor Kurchatov, Head of the Soviet nuclear project, and many of his collaborators, happened to be particularly influential.[41] According to Vizgin (1999), they played the role of "nuclear shield" and benefited from their proximity to the authorities and from their strategic importance in the Cold War context to allow the publication of Fock's article. Indeed, as he was not involved in the nuclear project, the Leningrad physicist was considered by his colleagues the best option to assume publicly the role of defender of the theories of modern physics. As a consequence, his activity against the undue criticism of some Marxist philosophers was recognized and he won the honors of the regime which saw in him one of the best proponents of Soviet science—not only was Fock an internationally renowned scientist, but he was also able to defend a coherent dialectical materialistic viewpoint on modern physics.[42] Meanwhile, Maksimov, humiliated, lost his influence.

In his autobiography, Infeld wished to put forward that Polish physicists were identically subject to ideological pressure. Nevertheless, the emphasis on such aspects was reductive because it did not underline that the confrontation between Fock and Infeld was above all a real debate of ideas on the interpretation of general relativity. We consider that it is precisely this dual dimension, contextual and scientific, which makes the dispute an exceptional case study to perceive the specificities of the renaissance of general relativity in the Soviet Union. Notably, in contrast to the poor quality of relativity debates in the USSR, which had not yet started to reopen international scientific contact, Infeld offered Fock a unique opportunity to raise the level of discussion.[43] Indeed, the Soviet physicist was at that time relatively isolated in the USSR in this field of physics and, clearly, the originality of his thought deserved to be put to the test by eminent physicists. Moreover, ideological pressure, by leading to a permanent philosophical rethinking of modern theories, incubated the Fock-Infeld dispute and gave it special resonance.

This point can be illustrated by many of Fock's articles at the time that expressed a clear adherence to dialectical materialism. But more surprising was the ideological dimension of some of Infeld's articles. In a 1954 paper in English published in the *Acta Physica Polonica*, the Polish physicist discussed the problem of motion and used the term "ideologically" in a rather curious way for an unprepared reader:

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[40] For more on the dialectical materialist character of Fock's interpretation of the theory of general relativity, see Gorelik (1993), Graham (1982, 1987, 367–378), or Martinez (2017, 224–245).

[41] For more on the Soviet nuclear project, see Holloway (1994).

[42] See Graham (1982, 129). This dynamic can also be described in terms of cultural diplomacy (Martinez 2019).

[43] See footnote 11.

> But the two methods also show essential differences. The first [method] consists in the fact that whereas we [Einstein, Infeld and Hoffmann] use the field equations for empty space, regarding masses as singularities, Fock uses the gravitational equations with the energy-momentum tensor. This difference I regard as unimportant ideologically for the following reason. It is obvious that no charge or gravitational body can properly be described by a singularity. We can use the singularity picture only if we describe the field outside the particle, where the energy-momentum tensor vanishes. It is obvious from our derivation that it would have been almost unchanged had we used surfaces covering the regions in which the energy-momentum tensor does not vanish (Infeld 1954c).

In this passage, Infeld was defending the choice made in 1938 to proceed with singularities instead of extended bodies. This difference with Fock's approach to the problem of motion was not intended to become crucial in the debate that opposed the two physicists. As we will see in Sect. 4, it was rather the case of the privileged nature of harmonic coordinates, a central point of the Soviet's unorthodox interpretation of general relativity. However, it remains interesting to observe that in the Communist context which established the foundations of the Fock-Infeld dispute, the Polish physicist felt necessary to justify his approach with Einstein and Hoffmann against a potential "ideological" criticism. Indeed, in a materialist perspective, singularities cannot reflect the materiality of the outside world and Fock had proved possible to treat the problem with extended bodies. Thus, Infeld knew that he was exposed to criticism from the watchdogs of the Marxist philosophy and prepared his discourse in consequence.

As mentioned in *Why I Left Canada,* Infeld wrote an article in defense of Einstein after he felt accused of plagiarism by Fock (Infeld 1978, 80). Its content is another example of the ideological pressure of that time. Moreover, its trajectory and consequences clearly illustrate how philosophical circles in the USSR played the role of an incubator in the Fock-Infeld dispute. The article was initially published in Polish by *Myśl Filozoficzna* (Infeld 1954a). Then, *Voprosy Filosofii* took the responsibility to translate it into Russian to make it available to a wider audience (Infeld 1954b). This Soviet philosophical journal was well known for its implication in the debates about the interpretation of modern physics in the Soviet Union. Its publishing committee revealed this explicitly in a footnote about its approach to Infeld's article:

> As this article has an interest for Soviet readers related to the discussion about the philosophical questions on the relativity theory, the redaction considers it is appropriate to publish it in the pages of the journal "Voprosy Filosofii" (Infeld 1954b, 173).

The content of Infeld's article fully justified this choice. Indeed, it followed an approach which clearly aimed at demonstrating the compatibility of Einstein's understanding of the theory with dialectical materialism. Given this objective, like many of his Soviet colleagues, Fock included, Infeld manifested an adaptation to some rhetorical requirements of the Communist context. For example, he quoted Lenin in aphorism or tried to demonstrate that Einstein was also critical of Ernst

Mach's thought.[44] Indeed, this last subject was the object of the main criticism by Lenin in his famous book dealing with the philosophy of physics: *Materialism and Empiriocriticism* (Lenin 1909).

Therefore, if Infeld also used this article to directly criticize Fock and his defense of harmonic coordinates, one is above all stricken by the observation that, despite their differences in approach, the two men manifested at this time a common state of mind toward the defense of the theory of relativity. Infeld's conclusion in his 1954 article was explicit: "It is necessary, however, to remind that no physicist can explain the modern science without the theory of relativity" (Infeld 1954b, 178). In following, Fock began a correspondence with his Polish colleague, and his first remark was along the same lines: "The publication of your article in Polish and Soviet journals suggests that our countries have defined the correct point of view on the theory of relativity."[45] Nevertheless, it also appears that "correct point of view" must be understood with a different meaning than that of Fock's interpretation: in a first time, it was simply the recognition in the Soviet Union and Poland of the theory of general relativity as a scientific object that satisfied Fock.

Indeed, once this observation was made, he immediately engaged a discussion on the status of harmonic coordinates. Three letters were exchanged before Fock and Infeld met again in Moscow in April 1955 and then at conferences abroad. This last point is the object of the next section, but it should first be pointed out that the letters were published in 1955 at the request of the journal *Voprosy Filosofii* (Fock and Infeld 1955). It may seem surprising as they were only dealing with scientific issues concerning harmonic coordinates and did not refer to any ideological questions. But in reality, this can also be interpreted as a mark of renewal in the discussions concerning the theory of relativity in the Soviet Union. In line with the fall of Maksimov, the Marxist philosophers had lost their influence, and the debates concerning natural sciences regained their nobility.

The Fock-Infeld dispute originated from articles of the late 1930s which aimed at solving a classical issue of general relativity, the problem of motion. However, it took form at the beginning of the 1950s, in a particular context with an undeniable ideological dimension. Although writings on general relativity were often charged with malicious intent in the Soviet Union, it was the climate of permanent philosophical debates that not only allowed a dynamic of critical thinking about the theory to stay alive but also situated discussions on general relativity in a unique environment during its "low-water mark" period: the philosophical journals. Thus, as long as the actors also agreed to recognize the ideological dimension of their context, the dispute between Fock and Infeld—one of the first debates in the post-war period to attain a more scientific level—found in the USSR a favorable ground to develop itself. Therefore, its rapid transposition to international scientific circles—which were becoming more dynamic in relation to general relativity—was then all the easier. After 3 years of maturation, in 1955, the dispute found its way to Bern.

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[44] For more on the scientists' adaptation to rhetorical requirements of the Communist context, see the developments of the present author in the case of Fock (Martinez 2018).

[45] Letter from Fock to Infeld, November 13, 1954 (Fock and Infeld 1955, 156–157).

4 The 1955 Bern Conference: International Perspectives

The year 1955 coincided with the 50-year anniversary of the formulation of special relativity by Einstein. To celebrate the event, a conference dedicated to the theory of relativity as a whole was held in Bern, where Einstein was employed by the Patent Office in 1905. André Mercier, Head of the Department of Theoretical Physics at the University of Bern, was the instigator of the project.[46] To this end, he got support from Wolfgang Pauli, Professor at the Federal Institute of Technology in Zurich, who served as the conference chairman. The celebrations were not limited to its commemorative aspect. Indeed, Mercier wished to organize "a truly scientific event in which the status of the fields connected to relativity theories was to be thoroughly discussed and analyzed by invited speakers" (Lalli 2017, 42).[47] As put forward by Roberto Lalli in *Building the General Relativity and Gravitation Community During the Cold War*, the conference quickly took an elitist turn due to some criteria used in the selection of participants: limited number, personal knowledge of the organizers, and scientific authority in a particular field of research related to relativity. Thus, a panel of recognized and influential scientists gave plenary talks. Among them were the French mathematician André Lichnerowicz and physicist Marie-Antoinette Tonnelat from France; Pascual Jordan and the astronomer Walter Baade from Germany; the Swede Oskar Klein; and the Americans Peter Bergmann, Howard Robertson, and Eugene Wigner. For shorter presentations, names such as Born, Møller, Hoyle, Bondi, Papapetrou, or, of course, Infeld completed the picture of a conference which for the first time brought together a large assembly of the leading specialists of relativity. Institutionally, it ended up being one of the first major events of the renaissance of general relativity.

In some cases, the scholars giving at the conference the "secondary" talks were invited by their scientific academies, which were asked to send to the Bern Conference their most eminent scientists in the field of relativity (Lalli 2017, 44–45). Regarding the Soviet Union, the conference organizers did not influence in any way the choice of speakers; they were directly selected in Moscow by the USSR Academy of Sciences.[48] As discussed in Sect. 3, during the preceding years, Fock had asserted himself as one of the leading specialists in the theory of relativity in the Soviet Union. Not only did he publish quality results on the problem of motion but he also claimed to guarantee the correct interpretation of general relativity in a

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[46] For more on the Bern Conference, the history of its organization, and especially the particular role played by its instigator André Mercier, see Lalli (2017). The proceedings of the conference were published by Mercier and Kervaire (1956).

[47] Mercier expressed this ambition to Einstein in a letter dated November 1953 (Lalli 2017, 42).

[48] See the letter from L. M. Brekhovskikh and E. A. Koridalin to Fock, dated October 25, 1954—ARAN SPb 1034-2-111. This situation led to the following amusing situation: In a letter to Møller on March 1, 1955, Pauli explained that he did not understand immediately that "the famous Fock," which he already knew, was part of the Soviet delegation. It testifies that the organizers did not influence at all the choices of the Soviet Academy. Cited by Lalli (2017, 43).

dialectical materialist frame of thought. Moreover, he had established a relationship of trust with the Soviet authorities during his fight against Marxist philosophers. Thus, this combination of elements made Fock the perfect candidate to attend the Bern Conference.[49] It was therefore not surprising that he received in the autumn of 1954 a letter from the Secretariat of the Physico-mathematical Department of the Academy of Sciences asking him if he wished to participate in the celebrations of the fiftieth anniversary of relativity in Switzerland.[50] In doing so, the Academy opened the dynamic of a virtuous circle for the Leningrad physicist. Indeed, it supported the idea that Fock could hold the central position in the field of relativity in the USSR and made his work more influential than it was already.

All this was reinforced by the fact that Fock was asked to suggest who among his Soviet colleagues could accompany him to Bern. Thus, in his reply on November 5, 1954, Fock accepted the invitation and proposed two names: A. D. Aleksandrov and I. M. Gelfand.[51] Aleksandrov was a mathematician in Leningrad. Although working mainly on special relativity, he was all along his career one of the main supporters of Fock in his interpretation of general relativity.[52] Gelfand was a professor at Moscow State University. He was also a mathematician, but he was not at all a specialist in general relativity. Fock had suggested Gelfand's presence because of his work on group theory, notably the Lorentz group theory, which the Leningrad physicist relied particularly on his defense of the harmonic coordinates. Thus, we note here that Fock had above all in mind to surround himself with people able to support his interpretation of general relativity. He clearly wanted to fully seize an opportunity that was unique for two main reasons. First, the Academy was giving him the possibility to cross the symbolic limit between East and West newly materialized by the Iron Curtain. With the exception of Polish trips in 1952 and 1953, as well as a stay in Hungary in 1954, Fock had not traveled abroad since the early 1930s. Second, he had so far published his ideas only in Russian, or through foreign language publications edited by the USSR Academy of Sciences and allied countries.[53] To go to Switzerland, and to enjoy an audience as remarkable as the one brought together by Mercier and Pauli in Bern, was an invaluable occasion for Fock to make his voice heard widely outside the limits of the Soviet zone of influence. As a consequence, the talks he suggested as soon as he responded to the Academy of Sciences went in this direction: the first one dealt with equations of motion of rotational masses while the second dealt with systems of privileged coordinates.

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[49] See also Martinez (2019).

[50] Letter from L. M. Brekhovskikh and E. A. Koridalin to Fock, dated October 25, 1954—ARAN SPb 1034-2-111.

[51] Letter from Fock to L. M. Brekhovskikh, dated November 5, 1954—ARAN SPb 1034-2-111.

[52] For more on Aleksandrov and the theory of relativity, see Graham (1987, 363–367).

[53] There was one exception, with the publication of an article titled "Le système de Ptolémée et le système de Copernic à la lumière de la théorie générale de la relativité" (Fock 1952) in 1952 in France, translation of a Russian article of 1947 (Fock 1947). However, it must be underlined that it was published among other Soviet articles in a new journal, *Questions scientifiques*, whose editors were claiming a "militant Marxism."

The Academy listened to Fock, but only Aleksandrov joined him for the Bern Conference which was held on July 11–16, 1955. The Soviet physicist played the role of chairman on July 12. On July 14 and 15, he gave the two aforementioned presentations in French.[54] These talks led to a lively exposure of the dispute between Fock and Infeld. In his autobiography, John W. Moffat, who attended the conference in 1955, gave us a somewhat picturesque version of the events:

> Infeld and Fock gave talks one after the other at the meeting. Fock, a large, stocky Russian who kept adjusting his hearing aids since he was completely deaf without them, gave the first talk, presenting his reasons for believing in the gravitational wave solutions in Einstein's theory. Infeld stood off to one side near the podium as Fock spoke. He was also a large man, a Polish physicist from the University of Warsaw. He was already beginning to bristle with indignation over Fock's talk. When Fock finished speaking, Infeld went to the podium. When he started speaking, presenting his reasons for not believing in the existence of gravitational waves, Fock ostentatiously removed his hearing aids, raising laughter in the auditorium. Even Hermann Weyl, the chair of the session, smiled with amusement (Moffat 2010, 101–102).

It was on the morning of July 14, and during the afternoon, an excursion was organized on the lake of Brienz, near Interlaken. Moffat added: "As we sailed around the lake, I had the opportunity to stand next to Infeld and Fock, who continued to attack one another on the issue of gravitational waves. This time Fock did not remove his hearing aids" (Moffat 2010, 103). This testimony is globally correct: Fock and Infeld followed one another during a session chaired by Weyl, the Soviet physicist was deaf, and a trip to Lake Brienz was organized.[55] Nevertheless, even if the subjects were in the end connected, Moffat was mistaken on the main theme of the dispute. As the publication of the different talks proves, it was not directly gravitational waves that were discussed in Switzerland, but the recurring question of privileged coordinates in the theory of general relativity.

Fock's talk of July 14 was a technical presentation on the equations of motion of rotational masses that he approached using harmonic coordinates (Fock 1956a). Immediately after, Infeld's presentation was a historical sketch of recent works on the problem of motion in general relativity (Infeld 1956). Infeld valorized the point of view he shared with Einstein and notably took over the "curiosity" emphasized years earlier by Fock (1953b, 24) about the similarity of the equations he obtained with Einstein and Hoffmann, and the ones obtained by the Soviet physicist with Petrova:

> In our 1938 paper we [Einstein, Infeld, Hoffmann] used a different coordinate condition than Fock's, but from Petrova's calculations published eleven years later, it followed that in spite of the different coordinate conditions, the post-Newtonian approximation is exactly the same for [sic.] coordinate conditions! (Infeld 1956, 207).

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[54] They were published in 1956 in a special issue of the journal *Helvetica Physica Acta* (Fock 1956a, b).

[55] Fock's hearing loss was probably caused by otosclerosis during his childhood.

Afterward, Infeld presented recent results he obtained for a simplification of the problem of motion and made the following comment: "The method has something in common with the work of Papapetrou (1951), *although* he, like Fock, used continuous distribution and harmonic coordinate conditions" (Infeld 1956, 207).[56] In both cases, Infeld wished to point out that, whatever the coordinate systems used, the results obtained by the physicists were similar; it was a strategy for him to deny the idea that harmonic coordinates should have any privileged role. In the Fock-Infeld dispute, every mention of the problem of motion was a potential gateway to a more general discussion on the interpretation of the theory of general relativity.

Thus, although Fock turned off his hearing aids during Infeld's speech, all the ingredients were there to rekindle a debate that, in reality, had never been closed. As Moffat showed us, the attitude of the main actors in our history attracted the attention of the physicists present in Bern. To support this point, the publication of Fock's second talk, "Sur les systèmes de coordonées privilégiés dans la théorie de la gravitation d'Einstein" (On the privileged coordinate systems in Einstein's gravitation theory), is of particular interest (Fock 1956b). The publication was accompanied by a transcription, longer than the talk itself, of the discussion which followed. It began as follows:

> For three years my friend Professor Fock and I [Infeld] have discussed this problem, and we cannot convince one another. I doubt if we shall succeed in doing so today. But since Professor Pauli insists, I shall make a few remarks about the difference between Professor Fock and the usual understanding of relativity (Fock 1956b, 240).

Then, Herman Bondi and Valentine Bargmann also took part in the discussions. Pauli had intentionally made it public, and, clearly, it was now in the hands of the entire community of "relativists."

The Bern conference was an institutional pillar of the renaissance of general relativity. In the case of the Fock-Infeld dispute, it proved the necessity of organizing worldwide events to gather the scientific community. Without such a conference, the debate may have remained relatively confidential. We have shown that the Soviet context provided unique conditions for the birth and vitality of the debate. Philosophical journals gave Fock and Infeld the opportunity to expose and mature their arguments, which explains why the opposition between the two men resonated so strongly in 1955. Fock took full advantage of the Bern possibility to find a new audience and escape the confines of his local context. Indeed, the conference had several consequences that not only helped Fock to widely disseminate his interpretation of general relativity but also to improve his status as the main Soviet reference in the field. Some of these consequences, in relation to the scientific community or local authorities, deserve to be mentioned here.

In 1955, Fock was preparing his Russian monograph on the theory of relativity. Entitled *Teoriya prostranstva, vremeni i tyagoteniya* (The theory of space, time, and gravitation), it developed his original point of view and was intended for both students and confirmed scientists. In Bern, Pauli heard about the project and decided

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[56] Emphasis added.

to get in touch with P. Rosbaud, Director of the Pergamon Press publishing house in London. As the latter wrote to Léon Rosenfeld: “Pauli thought that this book would be probably a very useful one and that it might be desirable to have an English translation”.[57] As soon as October 6, 1955, contacts between Rosbaud and Fock were well established.[58] Once the Russian edition was published in December (Fock 1955), Nicholas Kemmer from the University of Edinburgh was put in charge of its translation and began to work in concert with Fock. Later in 1956, the Akademie Verlag decided to work on a German translation.[59] The German edition was published in 1960 (Fock 1960), a year after the English version (Fock 1959).[60] In this monograph, Fock wrote a “Critical Survey” of the developments relative to the problem of motion in general relativity (Fock 1964, 392–399). In this he was much more cautious about the succession of the articles published, thus avoiding any chronological confusions.

With such wider distribution, Fock’s point of view on general relativity gained visibility and attracted attention from foreign colleagues since 1955. In 1957, the Russian went to Copenhagen after Niels Bohr invited him. Even if this episode is well known to historians for the dispute on the interpretation of quantum mechanics it launched between Fock and Bohr, it is important to underline that the invitation of the Soviet was in reality made at the initiative of Christian Møller after the Bern Conference.[61] Fock first came to Copenhagen to give three lectures on general relativity.[62] Later, in 1959, he made his first trip to the United States, from April 14 to May 1, where he spoke on general relativity and quantum mechanics in several prestigious institutions.[63] In June of 1959, Fock also participated in the “Colloque international sur les théories relativistes de la gravitation” (International Colloquium on the relativistic theories of gravitation) in Royaumont, near Paris, organized by André Lichnerowicz and Marie-Antoinette Tonnelat.[64] After Bern in 1955 and Chapel Hill in 1957 in the United States, it was the third international conference on general relativity.

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[57] Letter from Rosbaud to Rosenfeld, dated February 22, 1956—Niels Bohr Archives, Copenhagen: Léon Rosenfeld Papers, box 7: Manchester, folder 6.

[58] See Fock’s letter to Rosbaud on January 30, 1956. He responded to Rosbaud’s letter from October 6, 1955, mentioning already questions relative to the translation of the monography and also his remuneration—ARAN SPb, 1034-2-218.

[59] See Fock’s correspondence with the publishing house Akademie Verlag—ARAN SPb, 1034-2-218.

[60] A Romanian translation was also published in 1962 (Fock 1962a). As mentioned in footnote 24, we refer in our developments to the second English edition (Fock 1964).

[61] See the letter from Fock to Møller on May 8, 1956—Niels Bohr Archives, Copenhagen: Christian Møller papers, box 2, folder 34.

[62] These lectures were published that same year in the *Reviews of Modern Physics* (Fock 1957).

[63] Harvard University, the Massachusetts Institute of Technology, the Institute for Advanced Study at Princeton and Columbia University in the City of New York.

[64] The proceedings of the event were later published by Lichnerowicz and Tonnelat (1962).

Fock—accompanied by at least Aleksey Z. Petrov and Dmitri D. Ivanenko in the Soviet delegation—gave two talks in Royaumont, as in Bern, defending the concept of a privileged system of coordinates.[65] This conference became a new key institutional moment in the renaissance of general relativity. Hermann Bondi suggested the creation of a structure to stabilize the tradition, born with the Bern Conference in 1955, of international exchanges in the field of relativity. Thus, the International Committee on General Relativity and Gravitation was established. Lalli, whose aforementioned study was largely devoted to this new scientific institution, brought to attention that two main criteria were advanced in view to its composition, a desire to represent the different scientific and geographical sensitivities of the community, as well as scientific authority (Lalli 2017, 52–53). In the end, the committee gathered sixteen scientists, including Fock who clearly represented at that time the most eminent Soviet authority in the field.[66] No one can exclude that the initial intention was only to diplomatically include a physicist from the USSR. The message conveyed by this nomination, however, was a confirmation of the scientific interest of the community for Fock's unorthodox interpretation of general relativity. It was an important success for the Leningrad physicist, who confirmed on this occasion that the Soviet authorities were right to have trust in him.

Another Soviet physicist, Dmitri Dmitrievich Ivanenko, also joined the International Committee on General Relativity and Gravitation.[67] His inclusion actually marked a real renewal and growth of the field in the USSR. Indeed, Ivanenko played one of the leading roles in the structuring of the Soviet community of relativists, alongside Aleksey Zinov'yevich Petrov, also present in Royaumont.[68] The former organized a Soviet conference on gravitation in 1961, the first of a long series, and the latter served as the President of the Soviet Commission on Gravitation at the Ministry of Higher Education during the 1960s.[69] Internationally, it was Fock who remained the privileged interlocutor of his colleagues, hence in 1965, at a conference

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[65] Fock's report on his stay in France can be found in his archives in St. Petersburg—ARAN SPb, 1034-2-179. The talks were: "Quelques remarques sur les équations du mouvement et les conditions pour les coordonnées" (Some remarks on the equations of motion and the coordinate conditions) (Fock 1962b) and "Sur les ondes de gravitation émises par un système de masses en movement" (On gravitational waves emitted by a system of moving masses) (Fock 1962c).

[66] As Fock mentioned in his report to the Soviet authorities after the Bern Conference, "a group of participants invited [him] to join the Committee for the organization of similar conferences in the future"—ARAN SPb, 1034-2-179.

[67] For Fock, Ivanenko was admitted to the Committee only "on his own insistence." Letter from Fock to Artsimovich, September 6, 1962—ARAN SPb 1034-2-111. In reality, the two men had a tumultuous relationship. For unclear reasons, Fock tried several times to limit Ivanenko to secondary roles. See Snygg (2012, 174–179) and Martinez (2017, 444–450, 2019).

[68] In contrast to Ivanenko, Petrov became in the 1960s a "protégé" of Fock. Isaak M. Khalatnikov used this qualifier in his autobiography (Khalatnikov 2012, 135). See also Martinez (2017, 446–447, 2019), for Fock's contrasting attitude toward Ivanenko and Petrov.

[69] For more on Ivanenko's activities in developing the theory of gravitation in the Soviet Union, see his biography by Sardanashvili (2014, 142–152).

held in London, he was asked to be in charge of the organization of the next one in the USSR.[70] In this direction, he obtained the support of the Academy of Sciences and the Ministry of Higher Education.[71] This conference was held in September 1968 in Tbilisi, Georgia.[72] On this occasion, Fock became the President of the International Committee on General Relativity and Gravitation. It was the peak of his career in general relativity, a career which had begun 29 years earlier with the problem of motion.

5 Conclusion

What seemed to be a great achievement for Fock and Soviet science marked, in reality, the beginning of the physicist's disgrace. Political events would interfere with the organization of the Tbilisi Conference and mar his reputation. This descent began after the third Arab-Israeli war that took place on June 5–10, 1967, when the USSR broke its diplomatic relation with Israel.[73] As a consequence, Israeli relativists were not invited to Tbilisi, provoking an intense crisis among the International Committee on General Relativity and Gravitation.[74] If the situation was saved at the last minute with an invitation to Asher Peres in Tel Aviv, the conference had lost its luster, as many scientists had already canceled their trips to Georgia. Science and politics met abruptly in Tbilisi, and Fock found himself in a very delicate situation in which he could not avoid reinforced pressure from Soviet authorities and suspicion from some colleagues shocked by the Soviet organization. The crisis even resurfaced 3 years later at the next conference in Copenhagen.[75] It was decided to hold the following meeting in Israel, somehow in compensation for Tbilisi, and the Soviet delegation failed to prevent this decision. An investigation was conducted in the USSR and Fock was judged, if not directly responsible for the situation, at least unfit to cope with the situation. As a result, he was never allowed

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[70] According to Fock, the Committee on General Relativity and Gravitation unofficially decided on July 31, 1962, to stop its correspondence with Ivanenko. Letter from Fock to Artsimovich, September 6, 1962—ARAN SPb 1034-2-111. However, given the conflicting nature of the relationship between the two men (footnote 67), this information should be treated with caution. The best argument in favor of an alleged privileged position of Fock remains in itself the fact that he was asked to organize a conference in the Soviet Union.

[71] Letter from Leshkovtsev to Fock, October 1, 1965; letter from Kotel'nikov to Fock, November 3, 1965—ARAN SPb. 1034-2-198.

[72] For more on the Tbilisi conference, see Lalli (2017, 75–97), and Martinez (2017, 430–444, 2019).

[73] For more on this point, one can consult Govrin (1998).

[74] See footnote 72. Both sources discuss in detail the succession of events.

[75] Lalli detailed the nature of the events of Copenhagen (Lalli 2017, 101–125). See also Martinez (2017, 450–454, 2019).

to travel abroad again.[76] His international experience in the community of relativists ended suddenly and on a bad note.

Yet, the questions raised by Fock in his dispute with Infeld continued to be discussed. As Loren Graham commented in 1987, the views of the Soviet physicist were challenged but continued to "command respect and attention as a defensible and interesting point of view" (Graham 1987, 374). Fock had succeeded in spreading his ideas among the international community. He had raised a voice that deserved, if not to be followed, to be listened to. As described above, this success was a consequence of both his defense in the Soviet Union of the theory of relativity and his permanent wish to expose his original point of view. The Fock-Infeld dispute proved to be an important catalyst, as it helped the Leningrad scientist to become part of the international dynamics of the renaissance of general relativity. This trajectory had consequences that played a role in the recognition and independence of the discipline in the USSR. Indeed, not only had Fock proven that the theory still needed to be discussed, but the individual recognition of his work by foreign colleagues also opened perspectives for his compatriots, for whom he was an example to follow and one of the best promoters of Soviet science.

Blum, Lalli, and Renn invited us in 2015 to consider "that the historical process that has been dubbed the 'renaissance of general relativity' can be understood as resulting from the confluence of several factors closely interacting with each other" (Blum et al. 2015, 618). Our contribution focused on some sociocultural aspects of the renaissance, which showed us, through the example of the Fock-Infeld dispute, how they could be connected to scientific and institutional factors. In particular, we have highlighted some unavoidable specificities of the Soviet context at the beginning of the 1950s. If the theory of relativity could have been considered as a living field in the USSR, it was mostly through its constant questioning coming from Marxist philosophers. As a consequence, Fock and Infeld found in philosophical journals a very specific media to express their disagreements on the problem of motion and the interpretation of general relativity. They were both high-level physicists that accepted, in view of discussing scientifically valuable disagreements, the constraint of giving their publications a necessary ideological dimension. By their editorial politics, the philosophical journals even succeeded in keeping alive and amplifying the debate. As a consequence, when Fock and Infeld met in Bern in 1955, the community of relativists quickly realized something important was going on. The debate became global, and Fock greatly benefited from this situation for the rest of his career.

To summarize, the Fock-Infeld dispute simply demonstrates the importance of context in the trajectory of scientific ideas and their reception by the community. The actors in our case study were profoundly influenced by the ideological and political pressure on scientists in the Soviet Union and allied countries. Fock even built a large part of his career on this situation. The choice he made in the late 1930s

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[76] See Khalatnikov's testimony on the events in Copenhagen and their consequences (Khalatnikov 2012, 134–135).

to play the role of “physicist philosopher”—responsible for defending the modern theories of physics by proving their compatibility with dialectical materialism—resulted in a privileged postwar position in the USSR, from which followed international exposure. Without such a trajectory, Fock’s defense of the privileged nature of harmonic coordinates would probably have remained peripheral. During the renaissance of general relativity, the scientific community would have had to do without this consistent and valuable challenge to general relativity theory, which in the end had the merit of stimulating debate on its foundations for decades.

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Tokyo Wheeler *or* the Epistemic Preconditions of the Renaissance of Relativity

Alexander S. Blum and Dieter Brill

In the mid-1950s, John Archibald Wheeler radically changed his research agenda, from a successful and thoroughly mainstream career in nuclear physics to his trailblazing pursuit of general relativity, establishing in Princeton one of the most important hubs of the renaissance of relativity, and training a highly influential generation of American relativists.[1] Understanding Wheeler's turnaround will shed new light on the historical process that this volume focuses on. Yet, when speaking about his epochal shift, Wheeler tended to emphasize the metaphysical aspect, highlighting the transition from a particle to a field ontology and portraying it as an almost spiritual and definitely rather personal conversion:

> And of course nobody gets religion like a reformed drunkard. As I've often said about this subject, the fanaticism, if you would like to call it that, with which I pursued the opposite approach—that it's a pure field theory explanation of nature that one ought to work at—comes from having worked so hard at a pure particle explanation of what one sees.[2]

In this paper, we wish to somewhat correct this highly attractive narrative of personal epiphany. We wish to show that it was the use of a specific methodology and the

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[1] Wheeler apparently resented the term "relativist," feeling that it implied too narrow a specialization on relativity (Bartusiak 2015, 91). We will still be using this term to denote a physicist with expertise in general relativity, making no value judgments about that physicist's expertise in other areas of physics.

[2] Oral History Interview by Gloria Lubkin and Charles Weiner, conducted on 5 April 1967. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4958. Accessed 11 September 2019.

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pursuit of a specific (albeit ambitious) research goal that ultimately led Wheeler into the (pardon the pun) field of general relativity.[3] Wheeler came to recognize general relativity as the central theoretical tool in pursuing a longstanding research program. We aim to show that Wheeler's turn to general relativity was thus a paradigmatic example of what Roberto Lalli, Jürgen Renn, and one of the authors (AB) have called the recognition of the untapped potential of general relativity. Reconstructing Wheeler's intellectual biography in the years c.1935–1954 thus provides, besides its intrinsic interest, an explication of what this untapped potential, which was identified as the key epistemic factor in the renaissance of general relativity, precisely meant in the case of one of the central historical actors. Our reconstruction is based to a large part on the extensive archive of Wheeler's papers at the American Philosophical Society Library (Mss. B.W564) in Philadelphia, in particular on a set of notebooks that provide detailed insights into the development of Wheeler's thinking in the 1950s.[4]

The research goal that led Wheeler to general relativity was a radically reductionist one, of reducing the ever-growing number of elementary particles to one fundamental constituent, while still being able to explain and reproduce the multitude of observed particle properties and especially the still ill-understood nuclear interactions. Clearly, Wheeler was not alone in this research goal, which has been a central aspect of attempted "final theories" in physics, from Heisenberg's non-linear spinor theory[5] to contemporary string theory. What made Wheeler's program so radically different was that the proposed fundamental constituent was not to be a still unobserved microscopic element but rather a known entity, one whose properties were established and codified in generally accepted physical theories. This methodology is what Wheeler would later call "daring conservatism": taking well-established theories and (daringly) applying (and trusting) them far beyond their traditional domain, e.g., by applying them at microscopic length scales where they could not possibly have been experimentally confirmed. The theory that Wheeler ultimately selected as the key element in his reductionist program was, of course, general relativity.

In Sect. 1, we will reconstruct the origins of Wheeler's program, which initially had nothing to do with gravity, instead relying solely on electrons and their electromagnetic interactions as a substitute for the speculative theories of nuclear

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[3] Wheeler's transition has also been recently studied by Dean Rickles (2018). That analysis focuses more on a before-after comparison, as well as on the geon paper and its aftermath, not on the details of Wheeler's transition, as we do in this paper. It thus makes good complementary reading.

[4] I shall be referring to this collection simply as "Wheeler Papers" throughout the text. Other archives consulted are the Albert Einstein Archives at the Hebrew University of Jerusalem, Israel ("Einstein Papers"), the Gregory Breit Papers (MS 1465) at the Archives at Yale, New Haven, CT, USA ("Breit Papers") and the Richard P. Feynman Papers (FeynmanRP2) at the Caltech Archives, Pasadena, CA, USA ("Feynman Papers").

[5] As analyzed in detail in (Blum 2019). That book and this chapter represent key case studies for work of the research group *Historical Epistemology of the Final Theory Program* at the Max Planck Institute for the History of Science.

interaction of the day. At the time, renormalization methods had not yet been developed, and quantum electrodynamics (QED) was still a highly defective theory, beset by infinities. Wheeler had convinced himself that the extrapolation of electrodynamics to the microscopic (and then the nuclear) realm would necessitate its reformulation in terms of an action-at-a-distance (AAD) theory (Wheeler-Feynman electrodynamics). Wheeler's hopes that electrodynamics could provide the key to the nuclear interactions began to falter in the late 1940s. He did not, however, abandon the idea of replacing field theories with interactions at a distance, which he now began to apply to gravitation (Sect. 2). While Wheeler had originally hoped to construct an AAD theory of gravity from scratch, he became increasingly aware that he would have to take general relativity as a starting point. There followed an intense period of studying general relativity, also during the first course he taught on the subject in the academic year 1952/1953, as we will discuss in Sect. 3.

His study of general relativity led him to appreciate the great potential of general relativity and to include it alongside electromagnetism in the established foundations of physics he would allow himself to draw upon. He initially focused on one feature of general relativity in particular, namely, the way in which it related the source-free field equations to the motion of point-like particles in that field. He consequently adopted a new physical picture, where particles were described by singularities in the fields. Fields were thereby reinstated into his worldview, increasingly becoming the primary entities (Sect. 4). As this research program matured, he began presenting it, along with his methodology of daring conservatism, in public lectures, first in a lecture in Japan in the fall of 1953. This Japanese connection had Wheeler contemplate the name "Tokyo program" for his research agenda, a name he soon dropped, but which we have resurrected for the title of this paper. In early 1954, he finally abandoned the last vestiges of his particle program as he discovered a new way of drawing on the untapped potential of general relativity: to construct particles as regular (non-singular) field configurations. This led to his famous geon paper (Wheeler 1955) and the establishment of general relativity as the central research focus of Wheeler and a new generation of his students. We discuss this final shift to a pure field theory and his explicit reflections on the daring conservatism program in Sect. 5, before we conclude in Sect. 6.

1 The Great White Hope

The origins of John Wheeler's long and winding path to general relativity are hard to trace; all we have to go by are Wheeler's later recollections of the pipe dreams of a physicist in his mid-20s or, as Wheeler liked to put it, his "great white hopes." Of these there were two, which Wheeler recounted in two interviews, conducted by

Charles Weiner and Gloria Lubkin (WL) in 1967 and by Finn Aaserud (FA) in 1988 (Session I),[6] respectively.

The first great white hope was electrons. After the discovery of the positron in 1932, the theory of electrons and positrons based on the Dirac equation, "pair theory," had been worked out by Dirac, Heisenberg, Oppenheimer, and Furry. Wheeler felt that pair theory offered "mechanisms for binding electrons in very small regions of space that never got a thorough discussion" (WL), and that electrons might well be present in the nucleus after all (an assumption that had been dropped after the discovery of the neutron); in fact that electrons and positrons may form its fundamental constituents.

The second great white hope was scattering, which was to be viewed as the fundamental process from which all other characteristics of (primarily nuclear) interactions were to be derived. Both great white hopes were (at least in hindsight) also imbued with snappy and parallel slogans (Wheeler 1989), "Everything as Electrons" and "Everything as Scattering," and even if these precise titles are not actually contemporary, they do show an essential characteristic of Wheeler's thinking: an extreme reductionism, a reduction to simple, catchy thoughts, and a very small number of fundamental building blocks, a radical Ockhamism if you will. In particular, we see here what Wheeler would later call "daring conservatism": taking (an element of) a well-established theory and trying to use it beyond its usual domain of applicability, i.e., electrons in the nucleus or scattering theory to describe stationary states. But this unique approach did not show at the time. Feynman would later remark (perhaps apocryphally) that:

> Some people think Wheeler's gotten crazy in his later years, but he's always been crazy.[7]

While the later Wheeler would happily have publicized catchy slogans for crazy ideas, such as "everything as electrons," there are no outward indications of his grand vision at the time. As he would remark in the interview with Weiner and Lubkin:

> Nobody was as crazy as I was, to think that you could explain everything in terms of electrons. And this I think illustrates a weakness of my approach at that time, to have this secret hope nursed internally and talk about it occasionally with close friends but not feeling particularly at ease about bringing it out on a public platform…

Similarly, a talk (Wheeler 1934) he gave on (alpha particle) scattering at the APS meeting in Washington, DC, gave no indication (at least from the extant abstract) of the central role he was envisioning for scattering in fundamental physics, nor did his central paper on the subject, in which he famously introduced the S-Matrix (Wheeler 1937).

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[6] https://www.aip.org/history-programs/niels-bohr-library/oral-histories/5063-1. Accessed 11 September 2019. For WL see footnote 2.

[7] Cited in (Overbye 2002) and in (Wong 2008). In the former, Kip Thorne is given as the reference for Feynman's supposed remark.

It would take the meeting with a fellow eccentric to tickle Wheeler into presenting hints of his crazy ideas on "everything as electrons."[8] We refer, of course, to his PhD student, Richard Feynman, with whom he worked out what would later be known as Wheeler-Feynman electrodynamics (Wheeler and Feynman 1945). In this theory, the field-mediated electromagnetic interaction of Maxwell's theory is replaced by a direct interaction at a (spatial) distance between charged point-like particles. This interaction is not instantaneous (in time) but is rather the sum of a retarded and an advanced component, corresponding to the two possible solutions of Maxwell's field equations and ensuring compliance with the special theory of relativity. When a given charged point particle (electron) exerts an advanced force on the other electrons in the universe, it will experience a retarded back-reaction, which will in fact be instantaneous. Imposing the "absorber boundary condition" (which in the corresponding field theory would imply that there are enough electrons in the universe to absorb all outgoing radiation, so that there is no radiation "escaping to infinity") then ensures that this instantaneous back-reaction is equal to the radiation reaction of the usual field theory. This then not only implies empirical equivalence with field theory but also eliminates the possible difficulties with causality an advanced interaction might otherwise suggest.

Wheeler and Feynman give conflicting stories concerning the origins of their joint work on action-at-a-distance electrodynamics. Feynman in his 1965 Nobel lecture relates how, as an undergraduate, he had hit upon the idea of replacing the electromagnetic field with action-at-a-distance in order to eliminate the divergences of quantum electrodynamics (QED), how he had then learned as a graduate student that one cannot explain radiation reaction in this way, how he had tried to get radiation reaction into his action-at-a-distance framework as the back-reaction from other electrons, and how he had then presented this idea to Wheeler. Upon which Wheeler "then went on to give a lecture as though he had worked this all out before and was completely prepared, but he had not, he worked it out as he went along," a lecture that ended in the conclusion that one would have to take into account advanced solutions of Maxwell's equations, in order to get an immediate radiation reaction. From this conversation then grew, as Feynman recalls it, their joint work on action-at-a-distance electrodynamics.

Wheeler's version of events is considerably younger, first published a year after Feynman's death (Wheeler 1989). He recalls how Feynman had expressed some interest in Wheeler's idea of everything as electrons, how Wheeler had "animated by the concept of everything as electrons" worked out one Sunday (up to a factor of 2), how one might get radiation reaction as a back-reaction from the absorber even in a theory without fields, and how he had then presented his calculation to Feynman, who was able to sort out the missing factor of 2. But priority issues are

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[8] We shall have no more to say about "Everything as Scattering," which was here mentioned only to illustrate the early traces of daring conservatism in Wheeler's thinking. This notion was very influential in Feynman's later diagrammatic formulation of renormalized QED, which did effectively become a pure scattering theory, see Blum (2017).

not our concern here. In fact, in this case not only is the issue undecidable; the two stories are not even entirely incompatible: If we assume that both Feynman and Wheeler omitted substantial parts of the story, the two accounts might actually be merged together to form a coherent narrative.

For our story, another aspect evidenced by the two different accounts is much more important: It clearly shows that Wheeler and Feynman came to action-at-a-distance electrodynamics with very different motivations. For Feynman it was the divergence difficulties of QED, an issue which had somewhat dropped out of fashion in the late 1930s due to the great interest in meson physics, but an issue that would soon resurface in the late 1940s and ultimately earn Feynman the Nobel Prize. For Wheeler, it was his great white hope, his "everything as electrons."

According to Feynman's recollections, it was he who got to write the first draft of their joint paper:

> [Wheeler] asked me to write the paper – I wrote this thing up in 27 pages, which we could have sent to a journal, but he began to think, "No, it's too great a business, we'll write it good." And that of course made delays, and got interrupted with the war, and he got it so big that it was five parts – the whole reorientation of physics from a different point of view. I never went along with him on that. I mean, you know, with the idea that it's so marvelous, it's a reorientation of physics, you have to write five papers, and all of physics is turned upside down. But I felt that 27 pages were what it deserved. This was written mostly by him. See, it was a rewrite of the 27 pages, so to speak. I wouldn't say a rewrite because he didn't use the 27 pages as a basis, but the same ideas are developed, which I tried to write much more briefly, and which he tried to write in an historical context, about the arguments of Tetrode and Einstein – you see, it's a relatively long thing, and I didn't really write it, you understand.[9]

The manuscript that Feynman is referring to is probably identical with an untitled manuscript from 1941 in the Feynman Papers at Caltech (Box 6, Folder 1).[10] To assess the difference in tone, compare the first sentence of Feynman's draft:

> The attempts to develop a satisfactory scheme of quantum electrodynamics have met with several difficulties, some of which are found not to be a result of the process of quantization, but to be contained in the classical electron theory itself.

with the opening of the final, printed version:

> It was the 19th of March in 1845 when Gauss described the conception of an action-at-a-distance propagated with a finite velocity, the natural generalization to electrodynamics of the view of force so fruitfully applied by Newton and his followers. In the century between then and now what obstacle has discouraged the general use of this conception in the study of nature? (Wheeler and Feynman 1945, 157)

Wheeler's grand vision clearly came out in the historical pathos. But as far as the motivation provided for reformulating classical electron theory in terms of

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[9] Oral History Interview by Charles Weiner, 5 March 1966. https://www.aip.org/history-programs/niels-bohr-library/oral-histories/5020-2. Accessed 14 September 2020.

[10] The number of pages that this manuscript has depends on how exactly one counts, e.g., if one includes handwritten inserts, typed pages containing just one extraneous paragraph, and figure captions. A case can certainly be made for 27.

action-at-a-distance, Wheeler fully adopted Feynman's motivation of solving the difficulties of QED. There is no hint of Wheeler's great white hope, and so we need to ask the question, what did Wheeler mean when decades later he referred to his work as being "animated by the concept of 'everything as electrons'"? Now, one might simply take this to mean that he was looking for a purely particulate description of nature, only electrons, no fields, and that action-at-a-distance was the implementation of this program. There is certainly something to this, and in his later recollections, Wheeler certainly emphasized the "everything as particles" aspect of his early period, contrasting it with his later pure field approach.

But there is another aspect that appears to have been equally important, if not more so. Wheeler's program was specifically focused on electrons and especially on their continuing role in the nucleus. It was thus not simply concerned with the electrons as generic particles but with electrons as a very specific kind of particles, distinct from nucleons or mesons. Now, he was well aware that there were significant indications that electromagnetically interacting electrons were not the primary constituents of the nucleus, or even present there at all: The short range of the nuclear interactions and the huge kinetic energies obtained by electrons thus confined were the indications most typically cited. It seems, now, that Wheeler hoped that the action-at-a-distance formulation might help to put aside these objections to "everything as electrons," as he also later stated in his autobiography (Wheeler and Ford 2000, 164–165):

> I had another motivation as well for pursuing action-at-a-distance, for I clung to my hope that all of the matter in the world could be reduced to electrons and positrons. Yet I knew that if an electron and a positron were to be crowded together in subnuclear dimensions, some way would have to be found to get around the prediction of conventional theory that they would quickly radiate away their energy in the form of electromagnetic fields. Perhaps, I thought, an action-at-a-distance version of electromagnetic theory – one without fields – might explain the suppression of such radiation and permit the particles to live happily in such a confined space.

There are only vague hints at this in the published paper with Feynman, which contains a section on "Advanced effects associated with incomplete absorption," which discusses the physics of electrons in an incompletely absorbing cavity. This may be interpreted as Wheeler thinking about electrons in the nucleus, but this possibility is not mentioned explicitly. Stronger hints can be found in a lecture he gave, several months after the publication of the first Wheeler-Feynman paper, at a symposium of the American Philosophical Society on "Atomic Energy and Its Implications." This symposium, conducted in November 1945, only 3 months after the use of atomic bombs on Hiroshima and Nagasaki, was a rather serious affair "devoted to the atomic bomb" (as per the opening words of Henry DeWolf Smyth), featuring talks by J. Robert Oppenheimer on "Atomic Weapons," Joseph H. Willits on "Social Adjustments to Atomic Energy," and Irving Langmuir on "World Control of Atomic Energy" (Proceedings of the American Philosophical Society, Vol. 90, No. 1, January 1946). Wheeler recalled that he "didn't really want to talk about atomic energy" but rather "about what lay beyond it" (Wheeler and Ford 2000, 168). So, inspired by his collaboration with Feynman and eager to do fundamental physics after the long war work, he chose this unusual venue to provide first hints on his grander schemes in a talk entitled “Problems and Prospects in Elementary Particle Research.”

The hints toward his hope that action-at-a-distance electrodynamics might revolutionize nuclear physics are still very vague, merely by association (Wheeler 1946, 45–46):

> [T]he theory of [nuclear] mesons, may be said to be at present in a state of free experimentation with ideas and great uncertainty as to principle, both because of the incompleteness of our present experimental picture and because of difficulty in tying the proposed hypotheses to already existing theories. [...] The difficulties of inventing a new theory on the basis of incomplete experimental evidence suggest that one possibility acceptable at this time is the conservative one of extending the range of applicability of already existing and well-established theories.
>
> The second theory whose problems we consider is therefore the formalism of electron-positron pairs.

We see here the notion of daring conservatism (without that name yet) appear in print for the first time, but as to how it is to be applied, there is nothing to go by but a lone and easily overlooked “therefore.” Wheeler would focus on cosmic ray physics in the following years, setting aside once again his great white hope (with the exception of some work with his student Gilbert Plass on counting degrees of freedom and thus doing thermodynamics – Planck’s law – in an action-at-a-distance theory). But in June 1946, he submitted an application to the Guggenheim Foundation to pursue a project on an “Analysis of the Problem of Measurement in Electron Theory.” Through this scholarship, he hoped to obtain a leave of absence from Princeton to work on his foundational ideas. And he could think of no one better suited to help him in this endeavor than his old mentor Niels Bohr. As he wrote in his application:[11]

> Only a start could be made in the [...] program by the applicant himself and in collaboration with his student, R.P. Feynman; the war stopped further work. [...] The primary reason for this proposal is the feeling that the stage has now come in the theoretical work where new concepts and points of view are essential. To develop and test such points of view it appears that by far the most effective course is to take up a close association with Niels Bohr. The writer’s association with this scientist has convinced him that if there is hope of making advancement in the fundamental problems outlined above, this hope is best justified by Bohr’s ability to see into the future, his courage and judgment in considering and testing new concepts. [...] [T]he Applicant therefore is proposing to go to work with Professor Bohr in Denmark.

Even in this proposal, Wheeler was still rather guarded about what he wanted to do exactly. The proposed title recalled Bohr and Rosenfeld’s paper on the Problem of Measurement in QED (Bohr and Rosenfeld 1933), which to Wheeler was the model

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[11] We would like to thank André Bernard of the John Simon Guggenheim Memorial Foundation for making this and other documents available to us. The entire dossier relating to Wheeler’s application and Scholarship is on file at the Guggenheim Foundation. It contains Wheeler’s application (where the above quote is to be found in Item 3, pp. 1–2), as well as several letters quoted below.

for an incisive theoretical analysis of current theory (Hartz and Freire 2015) and which he referred to as "a classic paper" in his application. And when listing the tasks for his project, Wheeler remained excessively general:

(1) The consistent formulation of the mathematical formalism of the theory of electrons and positrons;
(2) A definition in terms of idealized experiments of the possibilities of measurement in the theory of the electrons and positrons;
(3) The interpretation of these idealized experiments in terms of the formalism – and of the formalism in terms of the idealized experiments.

Consequently, Eugene Wigner, who acted as one of the references for Wheeler's application, was also rather noncommittal about his views on Wheeler's project (received 24 June 1946):

> As to his proposed study, I must admit that I find it quite impossible to make any predictions. [. . .] I know from personal contacts that Professor Wheeler is most deeply interested in the project which he outlined.

In any case, while Wheeler's application was granted only 5 days after it was received (and 5 days before even receiving Wigner's letter of reference), Wheeler could not use the grant as soon as he had planned. As he wrote to Henry Allan Moe of the Guggenheim Foundation on 2 July 1946:

> I have now learned on my return to Princeton that there is a distinct possibility that the six-month leave of absence granted by the University to Professor Wigner may have to be extended through the second term of 1946–47 to allow him to accomplish most effectively his task to give a new direction to the work of the Oak Ridge Laboratory. I am afraid after examination of the situation with the Department here that it would create embarrassing difficulties for the work now underway to have me gone for the whole of the Spring term. However, it appears that I may be able to count on a period May 15–October 1, 1947. Even this range of time can, however, not now be made entirely definite. [. . .] I am sorry to have to report to you that the situation is in this uncertain state. I should like to have your advice as to how I can best take into account these difficulties in a manner acceptable to the Foundation.

There was no problem. Moe replied on 5 July 1946:

> It is a pleasure to welcome you to the company of Guggenheim Fellows – the distinguished company as I think.
> As to the date of starting your Fellowship, there's no need to settle that now. [. . .] You may count on us to "play ball."

After some back and forth, Wheeler finally received his scholarship for the period from 1 July 1949 to 30 June 1950 (i.e., also for an extended time period). For family reasons (Wheeler and Ford 2000, 183), he also opted to make Paris his home during his stay in Europe, rather than Copenhagen. And even though he did visit Copenhagen on several occasions during his time in Europe, much of his work with Bohr was focused not on electron-positron theory but rather on completing a paper with David Hill on the collective model of the nucleus, which Bohr ended up not co-signing (see Acknowledgments in Hill and Wheeler 1953).

One reason for this appears to have been a major advance that occurred in the years from 1946 to 1949: The problem of the electromagnetic self-energy of the electron had been solved, among others by Feynman, without the need to eliminate the self-interaction of the electron. Wheeler had always presented the elimination of the self-interaction as the prime motivation for action-at-a-distance. Consequently, when writing an updated version of his 1945 talk in 1949, he duly acknowledged the recent advances in quantum electrodynamics and used a much more cautious language when talking about Wheeler-Feynman electrodynamics. In 1945, he had concluded his brief elaborations on Wheeler-Feynman electrodynamics with the words:

> It is too soon to say whether the translation of the revised classical theory into quantum mechanics will remove the outstanding divergences. To test this point is an important problem for the future. (Wheeler 1946, 46)

In 1949 he wrote:

> Naturally the ultimate complete equivalence of this approach to the usual field theoretical treatment makes it clear that nothing new can result so long as this equivalence is strictly maintained. What may come out by changes and reinterpretations of the existing theory of action-at-a-distance is uncertain. (Wheeler 1949, 53)

That he did not yet give up action-at-a-distance entirely at this time (as Feynman did, see Blum 2017) is clearly due to his great white hope of constructing the atomic nucleus from electrons, a conjecture that was largely unaffected by the advances in QED. Indeed, we have one bit of evidence that Wheeler did in fact discuss this idea with Bohr during his stay in Europe, a letter sent to Bohr from Paris on 21 January 1950,[12] announcing his arrival in Copenhagen for the 27th. This letter is primarily concerned with their joint work on the collective nucleus, but then, on page two, Wheeler brings in "everything as electrons":

> The other problem, about which I am very anxious to get your opinion, is the question: is it possible to exclude a picture of elementary particle constitution based entirely on positive and negative electrons?

Wheeler discussed his hope how half-retarded, half-advanced action-at-a-distance might solve the problems usually brought forth:

> (1) *Localizability*. To localize an electron in a distance of $\approx e^2/mc^2$, it is sufficient to have a potential well of radius (e^2/mc^2) and depth $(137)^2mc^2$ [...]
>
> (4) *Source of potential*. Acceleration and velocity of an electron bound in such a potential are so great that electrostatic forces are negligible in comparison with radiative forces. Radiational transfer of energy to the outer world is itself negligible owing to the symmetry of the charge-current distribution. The radiational forces within the system can therefore be considered in a good approximation as half-advanced, half-retarded [...]. [I]t does not seem impossible to suppose that the electronic system generates a self-consistent potential, in which however the correlations between movements of interacting electrons must have an altogether dominating importance, in contrast to the atomic case.

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[12] Letter from Wheeler to Bohr, 21 January 1950. Archives for the History of Quantum Physics, Bohr Scientific Correspondence, Microfilm 34.

All this was very qualitative. And even if Wheeler's hopes concerning the nuclear binding potential could be realized, he was aware of the fact that there was another difficulty, which he had only very vague ideas on how to address: that of spin and statistics. If a neutron, e.g., was really to be considered as consisting of electrons and positrons only, it would have to contain an equal number of electrons and positrons to ensure its neutrality and thus would have to be an integer-spin boson. Wheeler was clearly interested in Bohr's judgment as to whether the project was worth pursuing despite this apparently insurmountable difficulty:

> Though I joke with you about my heresies, I am trying to be just as honest and as open as I can about the elementary particle problem. I know that there is no one who has your insight. So it will be a great privilege to talk with you over these and other problems…

As this letter was discussed during Wheeler's visit to Copenhagen, we have no evidence as to Bohr's take on the matter. Soon after his visit to Copenhagen, Wheeler cut his sabbatical short to work on the hydrogen bomb. When Wheeler returned to foundational research after the hydrogen bomb interlude, his focus was now on a different, but related problem: action-at-a-distance gravity.

2 Action-at-a-Distance Gravity

The idea of action-at-a-distance gravity can be traced back to 1941, when Wheeler and Feynman had worked out the basics of their absorber theory and Feynman first presented their joint work at a seminar in Princeton. Feynman later recalled that Einstein had remarked (Feynman 1985, 80):

> I find […] that it would be very difficult to make a corresponding theory for gravitational interaction…

Wheeler appears to have been immediately intrigued by this challenge. Although soon mainly occupied by war work, he wrote a letter to Einstein on 3 November 1943 (Einstein Papers, Call Number 23–442), requesting a meeting with Einstein to discuss "where the force of gravitation fits into the point of view" of action-at-a-distance theory. The meeting took place on 14 November in Wilmington, Delaware (Letter from Einstein to Wheeler, 6 November 1943, Einstein Papers, Call Number 14–360), and we can surmise some of its content from a letter which Wheeler wrote to Einstein after the meeting, on 2 December. Wheeler had developed a general framework for action-at-a-distance theories, which he referred to as "the theory of world lines." In this framework not only was there no more talk of fields; there was even "no reference to the concept of a space-time continuum." All that was left were the world lines of individual particles a, b, etc., the points on which were identified by some parameter, α, β, etc. The exact parameterization of the world lines was to be considered arbitrary, and all statements on physics were supposed to be independent of it.

The dynamics were now determined by functions that connect points on two world lines. In his letter to Einstein, Wheeler referred to these functions as light cones, which most clearly reveals their physical interpretation. In his later notes, he would mainly use the term liaison. And when he finally wrote a paper on AAD gravity some 50 years later, together with Daniel Wesley, he called them associators (Wesley and Wheeler 2003).[13] We will be referring to them as liaisons throughout this paper, as this is the term that Wheeler used for most of the period under study.[14]

The liaison function $\alpha_+(\beta)$ then returns the point α that is on the forward light cone of β.[15] Similarly, $\beta_+(\alpha)$ returns the point β that is on the forward light cone of α. Wheeler also introduced liaisons α_-, β_- for the backward light cone, which are the inverses of β_+ and α_+, respectively. Wheeler now proposed to Einstein that one could construct physics from these functions by setting up non-trivial, parameterization-independent relations. He pitched the expression

$$\alpha' = \alpha_-(\gamma_+(\beta_+(\alpha))), \tag{1}$$

where α' is the point on world line a that one reaches by moving along the forward light cone from a point α to world line b and then further on the forward light cone to world line c and then on the backward light cone back to world line a. As can easily be seen by imagining all three particles as being at rest in the (not yet constructed) three-dimensional background space, if $\alpha' = \alpha$, the three particles are on one line in three-space. If $\alpha' \neq \alpha$, they are not. One could thus, merely from three one-dimensional world lines and the liaisons between them, distinguish between a line and a triangle through the (indeed parameterization-independent) statement that α and α' are equal or unequal, respectively. Wheeler further conjectured:

> With a number of particles greater than three, one can build up more complex geometrical concepts.

Apparently, Wheeler had hoped that in this manner one would be able to construct a theory that would be fully equivalent to general relativity, just as Wheeler-Feynman absorber theory was equivalent to Maxwell. In their discussion, Wheeler and Einstein appear to have focused on a surprisingly specific difficulty with

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[13] One reason for Wheeler's return to action-at-a-distance gravity after so many years appears to have been that it had by then become abundantly clear that quantum gravity would suffer from similar divergence difficulties as QED, difficulties that in electrodynamics the Wheeler-Feynman absorber theory had aimed to solve. Since these difficulties could not be solved by renormalization in the case of gravity, it appeared attractive to revisit the old AAD formulation. This was pointed out to us by Daniel Wesley, who co-authored the AAD gravity paper with Wheeler as an undergraduate student, in an email to one of the authors (AB), 11 April 2019.

[14] We will also be using a notation that Wheeler introduced only after his letter to Einstein. This is additionally motivated by the fact that reproducing Wheeler's notation in the Einstein letter presents somewhat of a typesetting challenge.

[15] In general, the forward light cone might intersect the other world line more than once. The liaison was supposed to be a single-valued function that singles out one of those points and thus constituted an object somewhat more restricted than a light cone.

such an equivalence: a universe with two particles. In the world line theory, Wheeler asserted, “no physics at all is possible” in such a setup, because the only expression one can study, namely, $\alpha_-(\beta_+(\alpha))$, is trivially the identity, since α_- is *defined* to be the inverse of β_+.[16] In general relativity, on the other hand, there were, as Einstein pointed out, two-body solutions with a rich, non-trivial four-dimensional geometry.[17] There was thus, as Wheeler remarked, “an apparent discrepancy between the general theory of relativity and the general theory of action at a distance.”

In his letter to Einstein, Wheeler began to develop an understanding, which he would further develop over the course of the following years, that his world line theory was in fact right in implying that there should be “no physics” in a two-body universe. Wheeler would present first hints of his search for an AAD formulation of gravity in several talks given in the first postwar years, such as in his programmatic 1945 talk at the American Philosophical Society, already mentioned earlier, where he remarked:

> Just as the proper recognition of [. . .] atomicity requires in the electromagnetic theory a modification in the use of the field concept equivalent to the introduction of the concept of action at a distance, so it would appear that in the gravitational theory we should be able in principle to dispense with the concepts of space and time and take as the basis of our description of nature the elementary concepts of world line and light cone. (Wheeler 1946, 46)

But his stance on the two-body problem was only spelled out in the revised 1949 version of the talk. In defense of world line theory, Wheeler had adopted a relationalist view of space and time in the tradition of Ernst Mach, who had formulated such ideas in the late nineteenth century in opposition to the Newtonian notions of absolute space and time. Mach’s ideas, in the shape of the more or less formalized “Mach’s Principle” of the relativity of inertia, had played an important role in Einstein’s application of general relativity to cosmology (Smeenk 2014). But the existence of two-body solutions, Wheeler now argued, called into question the validity of Mach’s principle in general relativity. In a strictly relationalist theory, an idealized two-body problem would be non-dynamical: There was only one length scale to relate things, to the distance between the two point masses, and hence that distance should not be representable as changing, since there was nothing that its change could be measured against. Any statement that general relativity claimed to

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[16] In general, it is of course possible to first move along the forward light cone from world line a to world line b and then back, along the backward light cone, to world line a and arrive at a point different from the point one started out from, when conjugate points are involved. In the liaison framework, this issue is avoided: As mentioned in footnote 15, the liaison has to be a single-valued function, and thus it was natural on many levels for Wheeler to simply define the backward liaisons as the inverses of the forward ones.

[17] The two-body solutions by Weyl and Levi-Cività that Einstein was referring to are discussed in detail in an editorial footnote of the Collected Papers of Albert Einstein (Kormos-Buchwald et al. 2018, 437).

be able to make about the time evolution of the distance between the two bodies was thus empty.

While the centrality of the two-body problem was not to persist (already in the 1949 lecture, Wheeler voiced his doubts whether the paradox would necessarily be resolvable by action-at-a-distance gravity), the years 1943–1949 see gravitation moving to the center of Wheeler's attention, with new interesting paths of inquiry popping up, such as the unresolved problem of gravitational radiation reaction. While his talks of the period, as mentioned earlier, contained somewhat defeatist language concerning the prospect of Wheeler-Feynman electrodynamics, this is not the case for action-at-a-distance gravity. As his hopes for explaining the nuclear forces through electrodynamics were waning, gravity was increasingly presenting itself as a worthwhile field of study. Wheeler was beginning to realize the untapped potential of general relativity, though it must be admitted that he does not yet appear to have had a definite program for using this potential. The most specific part of his new interest in gravitational theory was the focus on the role of Mach's principle (beyond the particular case of the two-body universe) in establishing the relation between the world line theory and general relativity. This became, as we shall see, a central theme in Wheeler's thinking about gravitation.

Wheeler appears to have worked on AAD gravity quite a bit during his 1949/1950 stay in Europe. In a letter to Gregory Breit of 28 December 1949 (Breit Papers, Box 27, Folder 1101), he wrote:

> I am working quietly, sometimes on the reconciliation of the individual particle model of the nucleus and the liquid drop model [i.e., collective models of the nucleus], sometimes constructing a description of nature which makes no use of the concepts of space and time (analog in gravitation theory of electromagnetic action-at-a-distance).

Similarly in a letter to Feynman of 10 November 1949 (Feynman Papers, Box 3, Folder 10) and also in the letter to Bohr of 21 January 1950, already cited earlier, Wheeler talked about action-at-a-distance gravity, explicitly connecting it with Machian ideas of making "force dependent upon the number of particles in the universe" and mentioning another letter (not extant) to Wilhelm Magnus, mathematician at the Courant Institute, who had provided Wheeler "with some information about one of the group theoretical aspects of the problem." In later recollections, Wheeler even misremembered that he had proposed "doing similar ideas [to Wheeler-Feynman] for gravitation theory" (FA) already in his application to the Guggenheim Foundation in 1946. But at the time, his main focus was still on electromagnetism. With the success of renormalized QED, electrodynamics was on its way out for Wheeler, and Bohr appears to have disabused him of the last vestiges of "everything as electrons." But gravitation, in Wheeler's mind, was up and coming! In April 1951, he returned to Princeton from Los Alamos, and while still chiefly concerned with the work on the hydrogen bomb (Wheeler and Ford 2000, 218), he did find some time to ponder these foundational questions. In a notebook entitled "Action at a distance I," (Wheeler Papers, Section V, Volume 27, referred to as AAD Notebook hereafter), we find an entry dated 10 November 1951, in which Wheeler considers two different pathways to AAD gravity:

> Can thus work toward desired [action] principle from either one of two directions—(1) math. convenience + naturalness; (2) correspondence.

And at this point, the second pathway, establishing the theory through correspondence with the field theory of general relativity, clearly seemed the less favored, especially since the correspondence could not be exact for small numbers of particles, as he had established in his discussions with Einstein several years earlier. A few lines above the remark just quoted, Wheeler had written

> But should satisfy the principle of correspondence to ordinary general relativity in the limit of infinitely many infinitely small masses (continuous mass distribution).

only to then qualify this remark by a "probably" inserted after "should." So how did Wheeler's pursuit of AAD gravity along the lines of mathematical convenience and naturalness look? Still using the liaisons as his central dynamical variables, Wheeler's idea was now to set up an action principle (similar to the Fokker action in Wheeler-Feynman electrodynamics), written as an integral over world line parameters, the integrand being some function of the liaisons. When varied, this action would return differential equations for determining the liaison functions. As to how this action should look, Wheeler thought that it might involve counting closed cycles of liaisons, as this could provide a notion of local world line density without having to invoke an underlying space-time.

While these general ideas appear to have been present already in late 1951, Wheeler did not elaborate on them any further in his AAD notebook for quite a while. The next entry dealing with liaison theory dates from 17 March 1953. It would appear that Wheeler's purely mathematical approach turned out to be inadequate. For in 1952, he switched gears and started to engage general relativity head-on.

3 Teaching Relativity

Embarking on his study of general relativity and the corresponding action-at-a-distance formulation, Wheeler asked to teach a course on relativity at Princeton in the academic year 1952/1953. His request was granted on 6 May 1952, the day on which Wheeler began his first in a long series of notebooks on Relativity.[18] It is somewhat surprising that Wheeler was the first to teach a dedicated relativity course at the Princeton physics department. After all, Wheeler himself had clearly profited from the fact that Princeton was *the* center for relativity in the USA at the time. We have already mentioned his personal discussions with Einstein. And in his first writing on relativity, the 1945 talk at the American Philosophical Society,

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[18] We will be citing frequently from Wheeler's first two relativity notebooks, which we will be abbreviating as WR1 and WR2, respectively. These notebooks are to be found in Wheeler Papers, Section V, Volumes 39 and 40.

every author cited in the section on gravitation, aside from Ernst Mach, worked in Princeton. However, they all worked at the Institute for Advanced Study, resulting in a great divide between the accumulated expertise on relativity at Princeton and the lack of relativity teaching. There was expertise at the university and Wheeler tapped into that as well, as recalled by Churchill Eisenhart, son of Princeton professor Luther Eisenhart:

> As I understand it, after [Luther Eisenhart] retired he and John Wheeler were working together at writing a book called Mathematics Essential for the Theory of Relativity. [...] Dad and Wheeler, as I understand it, were bringing together in their book the mathematics, from here and there in the various branches of mathematics, you need for the general field theory.[19]

Eisenhart retired in 1945, around the time that Wheeler began thinking about AAD gravitation.[20] But Eisenhart (like Valentine Bargmann, another Princeton University expert on differential geometry) was a mathematician and indeed up until Wheeler's initiative general relativity was only taught in the mathematics department at Princeton (Kaiser 1998).[21] So, despite the immense tradition and expertise that Wheeler could draw on in his exploration of general relativity, he was indeed the first one to teach it to Princeton physics graduate students.

Wheeler's notebook opens with his thoughts on his upcoming course (WR1, p. 1):

> 5:55 pm. Learned from [Allen] Shenstone [then head of the Princeton Physics Department] 1/2 hour ago the great news that I can teach relativity next year. I wish to give the best possible course. To make the most of the opportunity, would be good to plan for a book on the subject. Points to be considered:
>
> (1) a short introductory outline of the whole
> (2) Emphasis on the Mach point of view
> (3) Many tie-ups with other fields of physics. Mention these in class; in the book put them in the ends of chapters as examples

The last two remarks are especially noteworthy. In remark (3), we can already see a recurring theme in Wheeler's later work on and in relativity, both intellectually and institutionally,[22] namely, to establish general relativity as a physical theory, rather than a mathematical or philosophico-cosmological one. Here we see the decidedly pedagogical aspect of this theme, as only in this manner could it legitimately be

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[19] Interview on 10 July 1984 with Churchill Eisenhart conducted by William Aspray, available at https://www.princeton.edu/mudd/finding_aids/mathoral/pmc09.htm. Accessed 17 July 2016. In this interview, Churchill Eisenhart also recalls that the manuscript for Wheeler and Eisenhart's book disappeared under mysterious circumstances after Luther Eisenhart's death.

[20] For biographical information on Eisenhart, see Lefschetz (1969).

[21] Kaiser erroneously gives the year of Wheeler's first course as 1954/1955. The 52/53 course, which is well documented by Wheeler's notebook, indeed did not yet show up in the Princeton course catalogue. A course on relativity by Wheeler is listed for 1953/1954. This course catalogue had not been available to Kaiser at the time he wrote his paper.

[22] On Wheeler's role in ensuring that the institutionalization of research in general relativity would take place in the disciplinary context of physics, see Lalli (2017).

taught to and applied by physics students. This emphasis should of course also be viewed in light of the predominantly mathematical tradition in relativity at Princeton University.

Wheeler's personal intellectual perspective on general relativity shows in remark (2), which hints at how strongly Wheeler's interest in General Relativity was at this point tied up with the prospect of an action-at-a-distance formulation, in which space-time disappears as an independent entity. Indeed, action-at-a-distance was a defining element in Wheeler's subsequent course as documented by his notes. The first term, which dealt primarily with special relativity, saw frequent references to the Wheeler-Feynman papers, including a long discussion of Wheeler-Feynman electrodynamics itself, stretching from December 1952 to January 1953.

Wheeler's general relativity class began in February 1953 with a first meeting on 5 February, in which topics for seminar reports were discussed. Here Wheeler had already honed in on some key physical problems, problems that would be defining elements of the upcoming renaissance of relativity: gravitationally collapsing stars, gravitational radiation, and empirical cosmology. These three problems were joined, in a list of topics for seminar reports discussed in the first class meeting (WR1, p. 47), by an "Assessment of Unified Theories." Here, Wheeler was clearly attempting to make contact with the general relativity scene as it presented itself to him at Princeton, as witnessed by the list of references for this report, which included not only the obvious Einstein (specifically his latest paper, which had just appeared in the January issue of *The Physical Review* Einstein 1953a) but also work by Eisenhart, who had now, in retirement, turned to the study of non-symmetric metrics as they appeared in Einstein's Unified Field Theory (Eisenhart 1951). The suggested topics for seminar reports are followed by an unsorted list of further topics Wheeler wanted to cover, which included both the "Mach point of view" and, immediately afterward, "our particulate point of view." The list also contains the entry "Variational principle and connection with quantum theory," a clear reminiscence to the least-action (in modern parlance: path integral) formulation of quantum mechanics that Feynman had developed precisely in the attempt to quantize Wheeler-Feynman electrodynamics. So, also in his list of topics for the second half of the course, we see the two central foci of physical problems (where Wheeler's identification of the central ones was clearly very influential) and of Wheeler-Feynman-Mach action-at-a-distance gravity, now joined by a rising interest in the idea of a unified field theory stimulated by the Princeton milieu.

Of course these questions were interrelated. The question of gravitational radiation, for example, was connected with the construction of an action-at-a-distance theory. The empirical adequacy of Wheeler-Feynman electrodynamics required imposing the so-called absorber condition that any electromagnetic radiation ultimately be absorbed, with nothing ever escaping to infinity. Is the gravitational world of GR "non-absorptive," Wheeler asks on the following page (WR1, p. 49), labeling it a "*very* vital question to look at." Wheeler was thus following an intellectual trajectory typical for the renaissance of GR: In pursuing a speculative extension of GR (action-at-a-distance in this case), he was forced to reflect on fundamental questions of GR proper (gravitational radiation). The question of absorber boundary conditions was really more of a side issue in this study of GR for ulterior purposes, however. As we saw in the last section, the central challenge for Wheeler was to construct a least action formalism for AAD gravity using liaisons. Wheeler pursued this program in parallel to teaching the course, and his lecture notes are consequently interspersed with research notes, initially focussing on the construction of liaison theory. In the following, we will focus almost exclusively on the research notes, leaving the exact reconstruction of the curriculum of Wheeler's course aside.

After his purely mathematical approach to this problem appears to have led nowhere, Wheeler's aim was now to construct liaison theory by studying its correspondence to regular GR. The first challenge here was to establish the locus of the correspondence, i.e., to identify the correct field quantity in GR that was to be reconstructed from the liaison formulation, the actual "gravitational field." Some two weeks into the course, Wheeler began to focus his attention on the Riemann tensor (WR1, p. 57). In an AAD theory, this would ultimately (via the liaisons) have to be reconstructed solely from the matter content (possibly merely in the form of singular world lines), along with some sort of boundary conditions. Wheeler was thus led back to Einstein's original question concerning the realization of Mach's principle in GR: Is this sufficient to uniquely determine the Riemann tensor (up to coordinate transformations)? Wheeler ultimately reached a conclusion similar to that of Einstein (1917), namely, "that there is a one to one correspondence between mass distribution and metric only when space closes up on itself" (WR1, p. 103).

It should be noted that Wheeler was aware that this statement was merely a plausible conjecture: "Any proof of uniqueness of case where metric is made to close up on itself? Very important question of principle" (WR1, p. 105). His main source for this conjecture was *The Meaning of Relativity* (Einstein 1953b). He continued to discuss this matter at Princeton with Weyl (WR1, p. 111), Wigner, and von Neumann (WR1, p. 120), all of whom disagreed with Wheeler's assessment. Wheeler took this aversion to Mach's principle to also be a result of unfortunate formulation of the principle and gave his class the task of coming up with a better "presentation of Mach's principle in 2 pages for an elementary physics student" (WR1, p. 135). Despite these difficulties, Mach's principle remained central to Wheeler's research program as it provided an analog of the Wheeler-Feynman absorber boundary conditions in general relativity. For some time, closure of the universe became an unquestionable fact to Wheeler, as he explained to his students:

> Question raised in class whether mass density enough to permit open or closed universe, in view of expansion rate. Answer: [. . .] closure comes first, density knowledge too poor to permit proof of contradiction; closure so fundamental to whole Mach idea that in present state of knowledge think of density value having to yield precedence to Mach principle. (WR1, p. 104)

With the Riemann tensor identified as "the field" (WR1, p. 96), the possibility was now established to construct the action for liaison theory through correspondence with the usual field (Hilbert) action:

> Set up an experimental procedure to get R_{ijkl} locally by liaisons between a number of particles. In this way tie up R_{ijkl} with liaison picture. Hence express R in terms of local liaisons. Hence get variation principle in terms of local liaisons. (WR1, p. 89)

The "experimental procedure" was supposed to involve some sort of "batting back and forth" of light signals (WR1, p. 90) which would be a physical realization of the connection between two points established by a liaison. But Wheeler's attempts to tie up the Riemann tensor with the liaison picture ended inconclusively. He attempted to find the liaison function between two world lines from general relativity, where the light signals κ travel from one particle world line to another on light-like geodesics, soon focusing on the limiting flat space case, where the two world lines x and $\overline{x}$ are straight (WR1, p. 97), i.e., the system of equations:

$$\begin{aligned}
x^i(s) &= x^i(0) + s\left(\frac{dx^i}{ds}\right)_{s=0} \\
\overline{x}^i(\overline{s}) &= \overline{x}^i(0) + \overline{s}\left(\frac{d\overline{x}^i}{d\overline{s}}\right)_{\overline{s}=0} \\
\kappa^i &= \overline{x}^i - x^i \\
\kappa^\alpha \kappa_\alpha &= 0
\end{aligned}$$

which was supposed to give a relation between the parameters s and $\overline{s}$, i.e., the liaison function giving for any point on one world line the point on the other one that lies on the first point's light cone. But even this simplified, non-gravitational trial calculation (27 March 1953; WR1, p. 99) ended inconclusively. His simple idea of obtaining the liaison action merely by translating the Hilbert action into liaison language faltered. Although now fully immersed in general relativity and tensor calculus, he returned to his original mathematical approach and began to pursue (8 April) a new approach to the liaison action, no longer based on counting cycles but rather on counting the number of (forward) liaisons entering and exiting a given volume element, a setup inspired (as he himself remarked) by the neutron balance in a nuclear chain reaction (WR1, p. 114).

In all this searching, Wheeler was well aware that he was pursuing an entirely new style of doing physics. On 18 March 1953, in the margins of notes on liaison theory (AAD notebook, p. 21), he remarked:

> This mushy thinking may in end be much better, if less attractive, to present than the usual 1,2, 3 type of argument with which one at the end so often presents his special conclusions.

What was driving him down this road of "mushy thinking" appears to have been the feeling of pursuing something grand, the "great white hope" feeling for which we here have the first contemporary archival evidence. Framing to himself his attempt to eliminate space and time, he wrote

> Undoing work of early man, that theoretical physicist who left no records. (26 March 1953; WR1, p. 97)

and also, for the first time in extant writing, coined one of his snappy slogans to describe his project, a "universe of particles" (WR1, p. 108). Indeed, though still bogged down in the attempt at formulating a liaison theory of gravitation alone, Wheeler always had in the back of his mind the further goal of combining this with electromagnetism and thereby achieving Einstein's goal of a unified theory (though without fields) and ultimately push on to include also the intrinsic properties of particles, such as spin:

> Don't feel discouraged about how much will still remain to do after expressing mere gravitation theory in liaison form. Should serve as guide in trying to put combined gravitation-electromagnetic theory in liaison form, and in later trying to put everything in neutrino language… (WR1, p. 113; 8 April 1953)

The term "neutrino" appears here for the first time prominently in Wheeler's relativity notebook. Its significance for Wheeler is somewhat hard to grasp, as it can imply two distinct things: It appears as the barest possible point particle, carrying no charge or mass (only spin, possibly), or it can appear as a spinor field, the elementary carrier of spin and associated with the weak nuclear interaction, a reading that goes back to Wheeler's 1945 American Philosophical Society talk, where he referred to the neutrino as a "field of interaction." This should be kept in mind in the following. What both notions have in common is that the neutrino is associated with the introduction of spin into the theory, which also appears to be the role in which it is invoked here. As the hope of recasting general relativity in liaison form faded, the fleshing out of the world line picture, i.e., the construction of a more sophisticated model of matter that would also include intrinsic properties such as spin, moved to the center of Wheeler's thinking.

4 Particles as Singularities in the Field

At some time in the spring of 1953, a shift began to occur in Wheeler's research agenda. Despite the day-to-day evidence we have from his notebooks, it is hard to date it exactly. It was rather a gradual shift, even though Wheeler's later use of religious metaphors to describe this tradition might rather imply an instantaneous conversion:

> The idea of action at a distance I gave up, not because the action and the distance was complicated, but because the particle was complicated. It was just the wrong basic starting point for the description of physics, to think of a particle. Pair theory made clear, and renormalization theory, that what one thought was an electron was really an infinite number of pairs of positive and negative electrons indeterminate in number and that the whole of space is filled with pairs. […] *And of course nobody gets religion like a reformed drunkard.* As I've often said about this subject, the fanaticism, if you would like to call it that, with which I pursued the opposite approach—that it's a pure field theory explanation of nature that one ought to work at—comes from having worked so hard at a pure particle explanation of what one sees. (LW, emphasis by us)

Interestingly the reasons that Wheeler gives for abandoning the particle approach (in particular the rise of renormalization theory) may well have been essential for his abandoning of the "everything as electrons" program but played no role for his assessment of action-at-a-distance gravity, which, as we have seen, he was pursuing well into the 1950s. And his shift to field theory did not initially involve thinking of the particle as something "complicated." Rather, he merely shifted from thinking of the particle world lines as the primary elements of the theory to thinking of them as secondary, derived objects, as singular lines in the field, whose equations of motion could be derived from the (vacuum) field equations simply by requiring consistent boundary conditions. This program goes back to the 1920s (Einstein and Grommer 1927).[23] Wheeler focused primarily on the approach by Leopold Infeld, which was first formulated in a paper by Einstein, Infeld (then Einstein's assistant), and Banesh Hoffmann (Einstein et al. 1938) and consequently goes by the name of EIH. It remained a major focus of Infeld's research all through the 1940s (see also Chap. 4 in this volume).

Wheeler had been interested in EIH early on, and, in Infeld and Schild (1949), he is in fact credited with pointing out the fact that the EIH program has only a trivial (Minkowski) zero-mass limit, and that consequently a separate proof is needed in order to show that test particles follow geodesics in a non-trivial background field. The first reference to a paper by Infeld in Wheeler's notebook, however, appears only on 14 April 1953 (WR1, p. 125), several days after his last attempt to construct a liaison action (using the divergence of liaison lines in a small volume element) had ended inconclusively. Already in that attempt, he had to assume a pre-existing (though not necessarily metric) space in which to place the volume element. Wheeler was thus setting aside his ambitious goal of reconstructing space and time entirely from the world lines and liaisons, hoping that "that deduction will come later" (WR1, p. 113). Turning to the Einstein-Infeld-Hoffmann approach was a further step in this direction. After an intense study of Lichnerowicz's formulation of general relativity as initial value problem,[24] which he hoped to combine with the EIH approach (the notes carry the header "Geodesics from Field Eqns *or* Initial Conditions on Field Eqns"), he formulated, on the last pages of his first relativity notebook, a new research program on 1 May (WR1, p. 150).

Before we turn to this research program, we should briefly discuss the attraction of the EIH approach. For it is quite striking that only a few years earlier the EIH approach had been adopted as the basis for another attempt at a theory of everything, Peter Bergmann's construction of a theory of quantum gravity.[25] Bergmann's hope had been that by transferring the EIH approach to quantum theory, the equations of the quantum mechanics for point particles might follow from the quantum field theory of general relativity in a similar manner as the classical equations of motion for point particles could be derived from the classical field theory. Even though

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[23] See Havas (1989) and Lehmkuhl (2019).

[24] He had been pointed to these mathematical works by Arthur Wightman; WR1, p.121.

[25] For more details, see Blum and Rickles (2018).

Bergmann and Wheeler were pursuing quite different approaches, their common interest in EIH can be explained rather simply: EIH held the promise that general relativity might have something to contribute to the microphysics of particles. And for Wheeler, who had now been trying unsuccessfully to reconstruct general relativity from microscopic particle trajectories for quite some time, this prospect, which at the same time let him keep the central notion of the world line, was naturally very interesting.

For Einstein, the representation of matter particles as singular world lines in EIH had not been intended as final. It was a place holder for an ultimate (field theoretic) description of matter, no better (but also no worse) than the energy-momentum tensor on the right-hand side of the Einstein equations.[26] For Wheeler, on the other hand, coming from the pure world line approach, singular world lines appeared as a perfectly adequate description of material particles. The different status accorded to the world lines determined their assumed properties beyond mere approval or disapproval: For Einstein the properties of the singularities could only be determined by the field equations. These did not determine the mass or the charge of the singularities, which were consequently free parameters, independently choosable for each individual singularity; much to Einstein's dismay, it should be added, as he hoped that the final theory would be able to explain why only two different masses (electron and proton) occur for the elementary particles (Einstein and Rosen 1935). For Wheeler, in contrast, the world lines were still entities in and of themselves, and the default assumption (at least in Wheeler's 'everything as electrons' tradition) was that they would be identical:

> [T]here is no place for the e/m of a particle to enter, and all particles should have the same e/m. (WR1, p. 150)

This presented challenges of its own, since there was of course more than one type of particle in the world. Wheeler reported that his physicist colleague Hartland Snyder "was inclined to pooh-pooh it all [on] acc't of existence of mesons, etc., in the world" (WR1, p. 150). Still, Wheeler was optimistic and had some ideas on how to produce a larger variety of particles with just one type of world line: Anti-particles were to be explained as world lines with the opposite orientation in time (an idea he had proposed to Feynman already a decade earlier); and he hoped to include spin in the picture by somehow taking into account the duality introduced by the two-sheeted Einstein-Rosen metric:

> Their [Einstein and Rosen's] bridge idea is most intriguing – two sheets of g meeting at each singularity, get neutrino? (WR1, p. 151)

On 13 May 1953, Wheeler then took his new idea of combining a (ideally unified, i.e., gravitational and electromagnetic) field theory with particles explicitly described as singular world lines to Einstein himself, when he visited him in his house on Mercer Street together with his entire relativity class. Ten years after his first discussion on AAD gravity with Wheeler, Einstein's reaction appears to have

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[26] This assessment is based on Lehmkuhl (2019).

been mixed. As opposed to most of the others that Wheeler had spoken to, “Einstein agreed [the] universe had to be closed to make [Mach’s] principle valid” (p. 11 of Wheeler’s Notebook Relativity 2, henceforth referred to as WR2) but believed this to be merely a necessary but not a sufficient condition.[27]

Einstein’s reactions to the specifics of Wheeler’s research plan were even more lukewarm: He declared that he “was not interested in singularities” (WR2, p. 11) and that the idea expressed of “connecting [an Einstein-Rosen bridge] up with spin of electron, neutrino is no good” (WR2, p. 11).[28] Wheeler’s general relativity class ended two weeks later with a final exam on 28 May. His interest in general relativity was unbroken, and his notebook contains notes on cosmology, gravitational radiation, and long passages in French copied from Lichnerowicz’s 1948 lecture notes “Géomètrie différentielle et topologie” before having to return them to the library (WR2, pp. 29–34). But for two months after the visit to Einstein, the notebook contains nothing new on Wheeler’s foundational ideas and the question of how to turn singular world lines in general relativity into full-fledged particles. Wheeler did take Einstein’s negative remarks with a grain of salt, in particular attributing Einstein’s negative attitude toward singularities to the fact that recent work by Infeld had shown that applying the EIH method to Einstein’s unified field theory did not return the correct equations of motion, i.e., the Lorentz force law in curved space-time (Infeld 1950). But it was his preparations to give a talk at the

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[27] According to the recollections of Wheeler’s student Marcel Wellner, Einstein had apparently not thought about Mach’s principle in a long time (Wheeler 1979) when it came up during the visit of Wheeler’s class. But less than a year after that visit, Einstein was asked about the matter again, by Felix Pirani. Einstein expressed his surprise at the renewed interest, opening his letter of 2 February 1954 (Einstein Papers, 17–447) with the words: “There is a lot of talk about Mach’s principle.” By that time, apparently having rethought the matter following the meeting with Wheeler and his students, Einstein had convinced himself that the principle was obsolete, telling Pirani: “In my opinion, one should not speak of Mach’s principle at all any more.”

[28] Arthur Komar offered a more specific account of Einstein’s dismissal of Einstein-Rosen bridges, recalling: “John Wheeler asked him about the Einstein-Rosen bridge. Why had he first introduced it and then dropped it again? Einstein answered that he had initially believed that the bridge connects two almost plane surfaces in a unique manner. When he, however, discovered that they did not have a unique structure, the bridge seemed to him to be too cumbersome, unattractive, and ambiguous” (Wheeler 1979). (*John Wheeler fragte ihn über die Einstein-Rosen-Brücke. Warum habe er sie zunächst eingeführt und dann wieder fallengelassen? Einstein antwortete, dass er zunächst glaubte, die Brücke verbinde zwei fast ebene Flächen in eindeutiger Weise. Als er jedoch entdeckte, dass sie keine eindeutige Struktur war, schien ihm die Brücke zu schwerfällig, unattraktiv und vieldeutig.*) These still rather vague recollections might be of Einstein referring to the fact that he had hoped that multi-bridge solutions to the Einstein equations might be so constrained as to enforce equal masses for the individual bridges, thereby addressing the problem discussed earlier of explaining why only a few different mass values for elementary particles were observed. He ultimately appears to have concluded that no such constraints would arise, as stated in a letter to Richard Tolman of 23 May 1935 (Einstein Papers, 23–32): “One does not see why the ponderable and electric masses cannot be arbitrarily large or different, when several are present.” Many thanks to Dennis Lehmkuhl for discussions on the Einstein-Rosen paper and for making this letter available to us.

International Conference of Theoretical Physics in Japan, to be held in September 1953, that gave Wheeler a new impulse.

5 Daring Conservatism and the Field Program

Wheeler ended up giving three talks in Japan: two rather technical ones on the origin of cosmic rays (Wheeler 1954b) and on collective models for nuclei (Wheeler 1954a), published in the conference proceedings, and one more programmatic talk, which he held on 10 September 1953, before the conference, at the Physical Society of Japan and which was only published in Japanese translation[29] in the Proceedings of the Society. We provide a retranslation into English of this talk (the original manuscript and recording are lost) in the appendix. It is this talk which is of central importance to our story, and it is this talk that one finds Wheeler preparing in his notebook on 18 July 1953 under the heading: "Philosophy of approach to elementary particle problem." From the start, Wheeler was very eager to establish a clear connection to Japan in his talk, noting in the margins: "Each one of us finds himself reflected in the countries he visits" (WR2, 35–37). But he also took the opportunity to reflect on his overall methodology. We have seen several times Wheeler's predilection for taking existing theories and using and extrapolating them outside their established domain of applicability, the paradigmatic example being his attempts to explain the nuclear forces electromagnetically. In the notes for the Tokyo talk, this methodology, which he would later characterize as "daring conservatism," is now, for the first time, made explicit as the "Tokyo Program":

> Proposed Tokyo program: Be as conservative as possible about introducing new elements into description. Make basics as clear & simple as possible. Is only the consequences that are complicated: ice; elem. particles; meteorology; geology. [...] Strengths of this approach. Its weaknesses. Einstein's May '53 remark to JAW: 'The Lord may have made the universe with five fields. I don't think so. But if he did, I am not interested in the universe.' Quote as a Princeton physicist, nameless. An extreme attitude, not fully open minded. Surely much good. (WR, p. 35)

As he outlined his guiding methodology explicitly for the first time, Wheeler was clearly becoming excited, referring to himself in the margins as "Tokyo Wheeler," drawing an admittedly somewhat bizarre analogy between his new ideas on elementary particle physics and the demoralizing propaganda spread to the American troops by "Tokyo Rose" (Iva Toguri), host of the WWII Japanese English-language radio show "The Zero Hour."

At this point Wheeler's notes shift away from a lecture sketch to an inner monologue about the foundations of his research program (WR, p. 35):

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[29] According to the notebook (Wheeler Papers, Box 156) that Wheeler kept during his stay in Japan, the translation was done by Takahiko Yamanouchi; Japan Notebook p. 51.

> Evidently have in mind something more fundamental. Out with it! Desert island philosophy: imagine selves cast up on Wake Island with library of all theory & exp[erimen]t up to now, to solve elem. particle problem – What to use as starting points? – Others not ambitious enough? Go whole hog now!

What follows is a long list of elements (of existing theory) that might be of importance in his attempts at crafting a theory of elementary particles. We explicitly see Wheeler assessing the potential of existing theories, in particular general relativity. The list contains familiar tropes (action-at-a-distance – point 2; Mach's principle – point 7) but also some novel elements, indicating how Wheeler was reordering his vision of how to think of elementary particles. The central new element is an emphasis on fundamental masslessness, the vision of a theory without intrinsic mass parameters that would ideally include"*no natural constants*. Nothing but e, π, etc." (point 1).[30] Where then was mass to come from? In point 8 of his list, Wheeler remarked on the "Electromagnetic origin of mass and the self energy story," jotting down the first-order radiative corrections to the electron mass, as first derived by Weisskopf in 1939. These terms were, in modern theory, simply absorbed in a renormalization of the electron mass, ultimately implying total agnosticism about the origin of mass. But in view of the proposed masslessness of the fundamental point-like particles, Wheeler was highlighting the electromagnetic origin of mass, advocating (point 11) that one "should apply electrodynamics to very small distances."[31] With mass externalized from the point-like singular particles to the surrounding field, Wheeler could consider all particles as composite (point 13), as "structures held together by radiative, electrodynamic and gravitational forces" (point 14). Wheeler's new vision thus really amalgamated all existing theory by proposing particles with a singular point-like core and field-generated structure.

This new focus on masslessness temporarily moved the neutrino to the center of Wheeler's theorizing as he emphasized the "importance of the *neutrino* in the scheme of things" (point 3). We again encounter the ambiguity in the conceptualization of the neutrino: At one point it appears as the fundamental point-like entity, with the electron to be thought of as a "neutrino with a charge loaded on its back." At other times, it clearly appears as a field-like entity, possibly arising through "spinorization" of the metric of general relativity, that is, taking the "square root" of the (vacuum) Einstein equations in a manner analogous to that which generates the Dirac from the Klein-Gordon equation. While Wheeler saw this as a major challenge, he was rather optimistic, remarking: "Spinorize, fit all together, and listen for the harmony."

Wheeler's novel emphasis on neutrinos was apparently also fueled by first results of the efforts by Frederick Reines and Clyde Cowan to directly detect these elusive

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[30] In a manuscript entitled "The Zero Rest Mass Fundamental Field Hypothesis" (WR, p. 101), which we shall discuss later in more detail, Wheeler ascribes this vision of a theory with no free parameters to Einstein. We have not been able to find relevant statements in Einstein's work.

[31] Wheeler here also invoked, for the first time, Bohr as the godfather of daring conservatism, because Bohr had applied "electrostatics to very small distances" in his atomic model.

particles. At this time, in the summer of 1953, Reines and Cowan were performing first background checks with their liquid scintillator detector at the nuclear reactor in Hanford, WA. They had found a source-independent background, which they thought might be due to "natural neutrinos" (what one might call cosmic relic neutrinos in big bang cosmology). Wheeler was aware of these results, referring in his notebook to "Reines-Cowan radiation" when emphasizing the importance of the neutrino. Wheeler was briefly envisioning the neutrino not only as the fundamental constituent of all particles but also as the prime component of the energy density of the universe, and his notes of 6 August 1953 show him studying the Friedmann equations in a neutrino-dominated universe.[32] These calculations were interrupted by a phone call from Reines, informing Wheeler that they had identified their source-independent background as due to nuclear capture of cosmic ray muons.

Still, the neutrino kept an important role, also in the talk that Wheeler eventually held in September 1953 in Tokyo. The talk is set up as a dialogue between Wheeler and two figures from Japanese history, Saigo Takamori and Sugawara no Michizane. Saigo, an important nineteenth-century Samurai, is given the role of the daring modernizer and presents the current state of the art in particle physics, the discovery of new particles at accelerators, and the meson theory of nuclear interaction. Sugawara no Michizane, a ninth-century scholar and poet, is given the role of reflective traditionalist, who presents Wheeler's Tokyo program, though the program is not actually named in the talk. It is merely characterized as "the principle, which is the basis of the scientific method, of not introducing a new hypothesis until it is clearly and undoubtedly necessary."

Sugawara begins by lauding general relativity as a model field theory: On the one hand, there is the point we have already discussed extensively that it allows for the integration and the derivation of the equations of motion of point particles. But more importantly, general relativity, viewed as Einstein's formalization of Mach's principle, was supposed to provide an account how a field theory (or more generally an interaction, which could also be a theory of action-at-a-distance) could generate mass in a massless theory or rather inertial mass in a theory without inertial mass. The argument as presented in the talk (or at least as presented in the Japanese translation) is somewhat elliptic. It is formulated not in terms of general relativity but in terms of an AAD theory. Clearly, such an AAD theory could not be equivalent to GR; we know that Wheeler had been searching for such an AAD formulation of GR for several years but had not been able to construct one. Instead, the AAD theory he used in the Tokyo talk was a slight modification of Newtonian theory, where the usual Coulomb field is supplemented by a second field that falls off only as $1/r$ and thus dominates at long distances. Wheeler gives

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[32] No correspondence between Wheeler and Reines or Cowan from 1953 is extant, but Reines in turn was clearly aware of Wheeler's contemporaneous elevation of the neutrino to central stage. In his Nobel lecture, Reines makes an inside joke, remarking without mentioning Wheeler: "While we were engaged in this background test, some theorists were rumored to be constructing a world made predominantly of neutrinos!"

this field explicitly as $Gm_g a/c^2 r$, where m_g is the particle's *gravitational* mass and a is its acceleration. This expression is analogous to the long-distance Liénard-Wiechert field of an accelerating charge in electrodynamics, and since it was not to be expected that the analogy between electrodynamics and gravity would be that perfect, Wheeler/Sugawara put the expression in scare quotes. With the long-distance interaction established, Wheeler then introduced what he called the "whole idea of gravity theory," namely, that the total gravitational force on a particle is zero: a particle subject only to gravity is not moved by forces but by the curvature of space-time. In the modified Newtonian AAD theory, the same idea is appropriate to express the expectation that inertia is provided by interaction, and an "intrinsic inertia" term ($m_{\text{inert}}a$) is absent from the equation of motion:

$$\frac{Gm_1 m_2}{r^2} - \sum_k \frac{Gm_1 a m_k}{c^2 r_k} = 0 \tag{2}$$

The equation's second term can instead be understood as the reaction on mass m_1 to the force that m_1's acceleration exerts on the masses m_k through the new, long-distance, Liénard-Wiechert-type interaction. One gets the usual (unmodified) Newtonian equation of motion for m_1 in gravitational interaction with m_2 (with $m_{\text{inert}} = m_g$), under the condition that

$$\frac{G}{c^2} \sum_k \frac{m_k}{r_k} = 1 \tag{3}$$

where the sum extends over all of the distant masses m_k which are at distances r_k from the mass m_1.

It is appropriate at this point to point out the intimate relation between Wheeler's argument and a sketch of the origin of inertial mass published by Dennis Sciama (1953) just a few months before Wheeler's talk. Sciama's argument was field-theoretical but also built on the electromagnetic analogy, explicitly employed vector fields obeying the Maxwell equations as gravitational fields and obtained long-distance Liénard-Wiechert potentials that correspond to Wheeler's long-distance force. Sciama also introduced an analogous principle to Wheeler's "whole point," which in his field-theory language reads that "the total gravitational field at the body arising from all other matter in the universe is zero," but Sciama explicitly labels this as a postulate and specifies that it holds in that body's rest frame. In this rest frame, the whole exterior universe is moving with acceleration $-a$, and the total field from the distant matter (the $1/r$ term) should exactly cancel the short-distance gravitational field (the $1/r^2$ term) of the particle with mass m_2. Rewriting this equation of cancellation, Sciama gets the usual Newtonian force law for the gravitational interaction between the masses m_1 and m_2 under the same condition as Wheeler (Equation 3) obtained in field-theoretical terms (Equation 6/7 of Sciama). It is unclear whether Wheeler knew of Sciama's argument and merely rephrased it in AAD terms or whether he had found it independently in his attempts at constructing an AAD version of gravity, building on an AAD formulation of electrodynamics. Both stories seem plausible, and if Wheeler really did not mention Sciama in his talk (and this is not just an omission of the transcription that was then translated into Japanese), the second one seems the more likely. Wheeler did eventually learn of Sciama's paper, as he jotted the reference down on the last page of his second relativity notebook (which covers the period up to April 1954), but since this last page appears to have served as a general place to note miscellaneous references, it is impossible to date. In any case, the Machian argument in the Japan talk was merely to serve as a proof of principle how mass might arise in a theory in which it is not a primary attribute of matter.

A similar proof of principle was given for the electrodynamic generation of mass through the radiative corrections calculated by Weisskopf, which we have already mentioned above. Wheeler's treatment in the Tokyo talk is somewhat problematic. Following Weisskopf, he (or rather Sugawara) gave the radiative correction δ_m to the electron mass as

$$\frac{\delta m}{m} = \frac{3}{2\pi}\frac{e^2}{\hbar c}\ln\frac{\lambda_{\max}}{\lambda_{\min}} \tag{4}$$

Leaving the question of the infrared and ultraviolet cutoffs in the logarithm aside for the moment, the parameter m is here the electron's bare mass, which should be zero according to Wheeler's assumptions. Wheeler, however, takes it to be the electron's physical mass, assumes this to arise entirely from radiative corrections (i.e., from the field), and thus sets the left-hand side of the equation to 1. Today it is well-established that perturbatively a massless fermion cannot gain mass from its electromagnetic interaction, precisely because the radiative corrections are always proportional to the bare mass (due to chiral symmetry). However, chiral symmetry may well be broken through non-perturbative effects, so that the general idea of a purely electromagnetic mass is not implausible. And again, Wheeler appears to merely have been floating some rough ideas for how mass might arise in a fundamentally massless theory and how one might obtain a unique value for the fine structure constant.[33]

All of this was thus an elaboration of the program he had outlined in his preparatory notes. The conservative Tokyo Program was now personified by the measured statesman and poet who was filled with a "love of Japanese beauty and harmony," who wished to work only with well-established entities and theories and to introduce no free parameters, such as masses, into his considerations, though the end of the talk saw Sugawara reconciled with the audacious Samurai Saigo, already heralding the reformulation of Wheeler's program as not merely conservatism but "daring conservatism" several months later. The part of Wheeler's program that

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[33] Here Wheeler was following in the footsteps of a number of famous physicists who had attempted to derive the value of the fine structure constant (which for a long time looked like it might be precisely 1/137) in the preceding decades. See Kragh (2003).

was most in flux, however, as witnessed not only by the Tokyo talk but also by the notebook entries of the time, was the exact role of the neutrino. While the talk clearly focussed on the field-theoretical aspect of the neutrino, it explicitly raised the question whether it was to be thought of as a massless field that joined the electromagnetic and gravitational fields in giving structure to the elementary particles or whether it was only a derivative of the gravitational field, arising upon spinorization.

Through his study of the literature on spin in general relativity (specifically Pauli 1933) and through discussions with the Princeton mathematician Oswald Veblen (30 October 1953), Wheeler reached the conclusion that the last point was true but that this spinorization could only occur upon quantization (WR2, p. 66):

> My conclusion? I know that the neutrino obeys Pauli statistics, therefore cannot come into a classical theory, therefore ought to show up only after quantization, therefore I should look for the classical theory & then quantize it a la Feynman, but with a square root, antisym, spinor character all put in at that time.

The neutrino and the issue of spin, which had temporarily been at the focus of Wheeler's interest and of the Japan talk, were thus temporarily set aside and relegated to the quantum realm. This further strengthened the focus on the classical fields of electrodynamics and gravitation, which, despite the persistence of singular point particles, were doing the work. It was the fields that had the potential to clarify the question of elementary particles, that would generate masses and define equations of motion, classically and in quantum theory. Wheeler's main focus was thus now on Einstein-Maxwell theory, a classical field theory that would, at least after quantization, give a full account of the physics of elementary particles (WR2, p. 67):

> If ν is somehow contained in em+grav., and if we are right saying that only fields of zero mass count (no meson fields, etc.), and if we have the *right* theory of em+grav., and if Feynman procedure [path integral quantization] is legitimate for such fields, then *here's where we start*.

In Einstein-Maxwell theory, the electromagnetic and gravitational fields appear as separate entities and are simply minimally coupled. Wheeler referred to it as the "un-unified field theory." The contrast with the unified field theory program of Einstein and others was clear, and indeed these were to be viewed at the time as legitimate competitors of Einstein-Maxwell theory as classical descriptions of electrodynamics and gravitation. Wheeler thus felt the need to consider their merits, before further pursuing his program.

How now to judge these merits? Einstein's unified field theory (Einstein 1950) was out, because, as we have already mentioned, one could not EIH-derive the Lorentz force from it. But Wheeler's student Arthur Komar (23 October) had pointed him to an alternative unified theory that gave, through the EIH method, the correct equations of motion, i.e., including the Lorentz force. This was the unified field theory of Behram Kursunoglu (1952). There was, however, a different problem with Kursunoglu's approach for Wheeler: It relied on the introduction of a fundamental length, i.e., a dimensionful parameter into the theory, which was of course in strict opposition to Wheeler's program of having no natural constants. Wheeler asserted that "conservative me" (30 October) had to try out what would happen in Kursunoglu's theory when one let the fundamental length go to zero: Would one still have a unified field theory or would one merely obtain general relativity without electrodynamics? The above quote thus continues (WR2, p. 67):

> Only one question *before we start*—what about so-called unified field theory? Einstein's variety no good. Therefore try Kursunoglu's variety—in case where his p [inverse of fundamental length] is set equal to ∞—just to test whether we have any *conservative* alternative to what we are doing.

Wheeler was thus now explicitly using conservatism (in the sense of no natural constants) as a criterion for theory selection. On 1 November, he came to the conclusion that Kursunoglu's theory, in the limit where the fundamental length goes to zero, merely reproduced Einstein-Maxwell theory. His assessment of unified field theory thus ended with a "bronze plaque" in his notebook, reading: "Unified Field Theories died here" and a letter to Kursunoglu, on 3 November, in which Wheeler wrote (WR2, p. 69):

> I am writing to ask if a conservative physicist who wants to deal with gravitation and electromagnetism within the framework of general relativity has nowadays any acceptable choice but to use as action the expression [action of Einstein-Maxwell theory]. By "conservative" I mean unwilling to introduce new ideas, new concepts, and particularly unwilling to introduce any quantity with the character of a fundamental length except as called for by inescapable evidence.
>
> Will not one who adopts the conservative point of view, as just defined, have to abandon unified field theory as it stands at present?

Wheeler had thus firmly convinced himself that the theory he needed to quantize was the conservative, minimal Einstein-Maxwell theory; he had found the new focus of his research in an attempt to quantize gravity, minimally coupled to electrodynamics. While quantum gravity nowadays, with all of the technical and conceptual difficulties it entails all too clear, hardly seems a conservative endeavor, to Wheeler, it certainly seemed as such; it was based merely on a combination of the well-established principles of general relativity, Maxwell electrodynamics, and quantum theory. After 20 years of private speculations, he felt he was now ready to publicly elaborate on his vision for the foundation of physics, a vision that was built on general relativity, a theory that was not only coherent and well-established, but also, through its unique features, such as Mach's principle and the EIH determination of equations of motion, had the potential to resolve the great open questions of microscopic physics. On 4 November 1953, we thus find in Wheeler's notebook "Points for proposed article Elementary particles from Massless Fields – An Assessment."

Around this time, Wheeler suddenly appears to have remembered a central point, which indeed was absent at least from his notebook entries for a while: the point particles. For November 8, we find the following short entry (WR2, p. 78):

> The big question
> Let's forget about electromagnetism for present. In quantum transcription of the pure gravitation theory with the variation principle based on $\psi = \sum e^{\frac{ic^3}{16\pi G\hbar} \int\int\int\int R\sqrt{-g}dx^1dx^2dx^3dx^4}$ (i.e., the path integral for the Hilbert action) how do we take into account the existence of singularities?

The singular world lines had, over the course of the year 1953, been transformed from the central element of the theory into a problematic embarrassment in the promising program of quantizing general relativity. Like Bergmann several years earlier, Wheeler realized that quantization and point singularities in the field did not really mesh. Bergmann had resigned himself to studying pure general relativity, but this was hardly an option for Wheeler who was after all trying to solve the problem of elementary particles. And indeed, the fields in Wheeler's approach were still mainly meant to provide services to the point particles: give them mass and define their equations of motion. When he met with Einstein once more, in the morning of 13 November 1953, Einstein asked (WR2, p. 83): "What about matter term in Lagrangian" to which Wheeler replied that "matter was to originate from singularities." However, when Wheeler then went on to explain Feynman quantization to Einstein, he remarked that in this setup, "the singularities in field get eliminated, never have to be talked about." This seems to be in reference to the assumption that singular field configurations would have measure zero in the path integral, which seems like a problem for describing matter by singularities, but is of course a good thing when talking about pathological singularities.[34]

But an even more severe difficulty with the singular point particle notion lies in its relation to the field concept. EIH determination of the equations of motion, of course, offered the prospect of reconciling the notions of field and point particle; this fact had originally led Wheeler to reintroduce fields into his worldview and endorse a dualistic ontology. As soon, however, as mass generation through the field entered into the picture, the fundamental incompatibility of point particles and local fields, which had haunted fundamental physics ever since Hendrik Lorentz had first tried to think the two together in his electron theory, again became visible. Indeed, already in his Tokyo lecture, Wheeler had been forced to introduce an ultraviolet cutoff (λ_{min} of Equation 4) to make the field-generated mass finite. This essentially meant abandoning the idea of a point particle and introducing a finite size for the electron. It is important here that Wheeler (or Sugawara) had hypothesized that this finite size would be given by the gravitational (Schwarzschild) radius of the electron, and not the Planck length. So the necessary mass scale that one needed to make a length using the gravitational constant G and the speed of light c was provided by the mass m of the electron and not by Planck's constant h. This clearly indicated that the cutoff was to arise not as a quantum effect but due to the presence of the particle.

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[34] Indeed, Einstein appears to have been impressed. While first remarking that he "abhorred" the idea of first constructing the classical field theory and then quantizing it, he then conceded (according to Wheeler's notes) that "it was the first time he had ever heard describe a way that [quantum theory] might get through, found it very attractive."

By introducing the notion of field-generated masses, Wheeler had thus effectively abandoned the notion of a point particle that had been a mainstay of his research program for a long time. This was not a problem for the EIH determination of the equations of motion, as the use of point particles in that derivation could well be viewed as a mere approximation.[35] But it ultimately undermined Wheeler's briefly-kept hopes for a dual theory of point particles and fields and forced him to consider novel conceptions of matter.

While he spent the next weeks thinking about how to spinorize Einstein-Maxwell theory by taking the square root of the Lagrangian in the action (WR2, p. 88), the pressing question of the constitution of matter moved to the center in a working paper entitled "The Zero Rest Mass Fundamental Field Hypothesis" and dated 19 January 1954.[36] Here, Wheeler addressed the central question that any theory of extended (i.e., not point-like) particles would have to answer. While the spatial extension of the particles avoided the issue of divergent field strengths, it brought with it a different issue, which had a long tradition going back to first attempts at a solution by Poincaré: the issue of stability. Given that there would be no more singular point-like cores, all that was left for constructing a particle were the electromagnetic, gravitational, and possibly neutrino fields (the "zero rest mass fundamental fields" of the manuscript's title), and "an elementary particle is held together by the balance of gravitational, neutrino, and electromagnetic forces" (p. 7 of the manuscript). But how to envision such an object? In the manuscript, Wheeler explored the possibility of comparing elementary particles with a (collapsing) star – the analogy being based on both objects (star and particle) being held together by gravitational forces.

But the big breakthrough for how to model elementary particles only occurred about a week later, when Wheeler attended the Fourth Rochester Conference on High Energy Nuclear Physics from 25–27 January 1954 (Noyes 1954). It is the

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[35] While Wheeler (1961) would later conclude that point singularities were not a valid approximation for any reasonable model of matter (which by that time for him meant geons and wormholes), there is no indication that he (or anybody else) harbored such doubts in 1953/1954, given that the concepts and in particular the conception of matter that these conclusions were based on had not been developed yet.

[36] The paper is included in WR2, p. 101, as an insert. This copy is noteworthy also for some remarks in the margins in which Wheeler explicitly connects his conservative heuristic in physics with conservatism in politics, noting: "To defend well established physical ideas as unpopular as defending well established political parties. People like to criticize. Religion the great defender." In this connection it appears pertinent to mention that Wheeler's conservative stance (in physics), as outlined in the Tokyo talk, was explicitly criticized by the Japanese physicist Shoichi Sakata, an outspoken Marxist (Staley 2001). In discussions on September 18 at the conference in Kyoto, a week after Wheeler's lecture, Sakata remarked: "I am convinced the future theory should not be the progressive improvement of the present theory. At the Tokyo meeting, Professor Wheeler pointed out that there are two methods of approaching the truth, that is, Saigo Takamori's method and Sugawara Michizane's method. But in Japan Professor Tomonaga had pointed out that there are two ways, namely, a non-reactionary conservative way and also a revolutionary way. This is our common sense." (Proceedings of the international conference of theoretical physics, Kyoto and Tokyo 1954, 34–35).

last one of Wheeler's breakthroughs that we shall discuss in this paper, as it finally brings us to Wheeler's geon paper (Wheeler 1955) and his embrace of a pure field theory, from which also the singularities representing matter had been removed. Up until now, Wheeler had mainly attempted to use the untapped potential of general relativity as it related to mass points: the ability to derive their equations of motion from the field equation, the possibility of generating mass for them from fields or interactions. In late January 1954, Wheeler seized upon a feature of general relativity which he had hardly engaged with so far: the non-linearity of the field equations, which in principle allowed for solutions describing a localized and (meta)stable concentration of energy, an idea which had been at the back of Einstein's mind for a long while.

On his manuscript of 19 January (which was never published), Wheeler had noted that he was distributing it to a small number of physicists, including Einstein, Bohr, and Wightman. Wightman was also attending the Rochester conference, and Wheeler appears to have discussed his ideas with him there, for on 25 January 1954, we find the notebook entry (WR2, p. 96):

> Ball of light held together by gravitational forces as classical model for an elementary particle = fireball = (Wightman name) Kugelblitz

immediately followed by calculations for a spherically symmetric gravitational potential fulfilling the vacuum Einstein-Maxwell equations (i.e., the Einstein equations with only an electromagnetic energy-momentum tensor as a source), with all of the electromagnetic energy constrained to a sphere of finite radius. Such a field configuration, which could only exist in a non-linear field theory such as Einstein-Maxwell theory and which Wheeler would soon label a "geon" (first found in WR2, p. 104, in an entry dated 19 February 1954), was thus the new model for elementary particles that Wheeler would pursue for the next few years. Everything point-like had been expelled from the model, in favor of a spatially extended pure zero-mass-field configuration.

There were of course many open questions to tackle, some of which Wheeler listed in the entries of the next two days (WR2, pp. 100ff), such as whether such entities really existed, how to incorporate charge,[37] the still unsettled role of the neutrino and the square root of the Einstein-Maxwell Lagrangian, and the role of quantum theory and self-energies,[38] in particular concerning the quantization of general relativity, in which context Wheeler noted (WR2, p. 103):

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[37] Here Wheeler already pondered the possibility of having "outgoing lines of force [...] understood in terms of lines coming in from an 'internal universe,'" an idea that would later mature into his notion of a wormhole.

[38] Here Wheeler encountered some conservative resistance from Wightman, who objected to Wheeler's predilection for path integrals, arguing instead that one should "improve & understand present formalism," i.e., pursue axiomatic quantum field theory. Even Feynman appears to have been doubtful about the "general utility" of the path integral, as he had not yet been able to properly accommodate fermions.

> Try to understand whether Gupta or anyone else really know what he's talking about on the quantization of gravitation theory, esp. the comm'n. rel'ns at small distances.

But while the new geon model of elementary particles brought with it a host of unanswered questions, an entire research program as it were, just days after the Rochester conference (where he had talked on charged meson decay), Wheeler certainly felt confident enough to publicly present his new idea in New York City, where he held the annual Richtmyer Memorial Lecture of the American Association of Physics Teachers (AAPT).[39] This Lecture, entitled "Fields and Particles," is the last text we shall be discussing and is, as we shall see, in many ways the sum of the development in Wheeler's thinking that we have reconstructed in this paper.[40]

The Richtmyer Lecture began with Wheeler's most explicit elaboration of his conservative methodology, which he now labelled "daring conservatism" and couched in religious terms, citing the apostle Paul (p. 1):

> "Whatsoever things are true, whatsoever things are honored, whatsoever things are judged, whatsoever things are pure, whatsoever things are lovely, whatsoever things are of good repute. If there be any virtue and if there be any praise, think on these things."[41] Following these words of Paul, I would like to dedicate this occasion [...] to an appreciation of the great truth of physics in the saying that from them we will receive guidance in this elementary particle problem beyond anything that we now imagine.

Wheeler then went on to highlight the role of general relativity among the "already well established ideas" of physics on which the conservative physicist should build by daringly "following out [its] consequences" to the "utter most extreme."

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[39] The AAPT was conducting its winter meeting in parallel with the American Physical Society, which conducted its annual meeting at Columbia University from 28 to 30 January 1954 (*Physical Review*, Volume 94, pp. 742ff), so that there were also many research physicists in the audience.

[40] The lecture was never published, but there is an extant transcript in the Wheeler Papers, Box 182, in a folder entitled "Fields and Particles." The Richtmyer Lecture Memorial Award had been established in 1941 to honor Floyd Richtmyer, one of the founders of the AAPT (https://www.aapt.org/Programs/awards/richtmyer.cfm). Accessed 14 September 2019. Many of the previous lectures had been published in the AAPT's journal, the American Journal of Physics (e.g., Slater 1951; Vleck 1950; DuBridge 1949). Wheeler had plans to publish his lecture there as well, and the folder contains two revised versions of the original lecture transcripts, which were clearly supposed to lead up to a publication. The folder also contains some correspondence between Wheeler and Thomas Osgood, editor of the American Journal of Physics, such as a letter from Osgood of 28 January 1957, which begins: "Here is my annual letter of inquiry about the manuscript of the paper 'Fields and Particles' that you gave as Richtmyer Memorial Lecture during the meeting of the American Association of Physics Teachers in New York, January 28–30, 1954. It ought to be published without delay." Wheeler in fact cited the paper in the first footnote of the geon paper as "to be published." That long footnote (a specialty of Wheeler, to which this footnote here is a sort of tribute) also contained a reference to Wheeler's Tokyo talk and "the point of view ascribed by the author to Sugawara-no-Michizane," making the entire footnote rather enigmatic for the average American reader of *The Physical Review*.

[41] This passage is from Philippians 4:8, where it reads "honest" instead of "honored", "just" instead of "judged", and "report" instead of "repute." We have given the quote as it appears in the lecture transcript, and it is to be assumed that the transcriber simply misheard these three words. Wheeler corrected all three in the later manuscripts of the Richtmyer Lecture mentioned in footnote 40.

He then went on to outline the great potential ("exciting new possibilities") of general relativity both "in the realm of what might be called astrophysics" and for the "elementary particle problem," introducing his geon[42] idea to the world and presenting it as a new research program (p. 8):

> In my view following out the philosophy of the conservative daring [sic], it's an inescapable obligation of our present-day physics to continue the investigation of these objects and to see what boundary line if any separates them from the elementary particle problem. The full investigation of both electromagnetism and gravitation of course has to take place within the frame work of quantum theory.

Wheeler had thus publicly outlined his new research program in general relativity, which consisted of studying stable, localized solutions of the Einstein-Maxwell equations, their modification through quantum theory and their relation to elementary particles, as well as the inclusion of further elements into this picture, such as charge and the neutrino/spin. Wheeler's transition to a full-blown "relativist" was completed, and the research program outlined in the Richtmyer lecture would occupy him and his graduate students for years to come. So fruitful was this approach that Princeton and the Wheeler School, despite being the youngest of the relativity centers soon to be connected in the Renaissance, became one of the central hubs of that process.

6 Conclusions

In this paper we have reconstructed John Wheeler's turn to general relativity in the years ca. 1941–1954 and how it was driven by what we have called the untapped potential of general relativity, thereby corroborating and filling with meaning the claim of Blum et al. (2015) that this untapped potential was one of the motors of the renaissance of general relativity. Our reconstruction has shown that Wheeler's general methodology, ultimately branded "daring conservatism," precisely consisted in seeking out the potential of existing theories, rather than constructing new ones. It should, however, be added that the general notion of daring conservatism can be read in two ways, both of which Wheeler endorsed. One is to extrapolate existing theory in order to make predictions for new, unexpected phenomena and then trust those predictions, even though they are made outside the domain for which the theory has been experimentally corroborated. This view of daring conservatism is to be found in an example that Wheeler gave in the Richtmyer lecture, where he claimed that he could have predicted nuclear fission two years before its experimental discovery, had he only trusted the extreme predictions of 1930s nuclear modelling.

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[42] Then still referred to as a "Kugelblitz" or, in the words of the person who transcribed the lecture, "cugoflix."

This view also applies to the use of general relativity in making novel predictions for astrophysics.

But as we have seen, it was another reading of daring conservatism that was initially more central to Wheeler's thinking: using existing theory not to predict novel phenomena but to solve existing (theoretical) problems and paradoxes that one might otherwise have been tempted to solve by introducing new theories. The central issue that Wheeler came to believe general relativity had the potential to solve was what he called the "elementary particle problem." A precise definition of this "problem" is hard to come by, but it meant something along the lines of obtaining a consistent description of the internal structure of elementary particles (which originally of course implied finding a consistent theory of point-like particles without structure). The solutions that Wheeler considered to this problem were shaped by several convictions, in particular that (i) the general idea of the solution should be expressible in classical language, (ii) the solution should be monistic, or at least not gratuitously introduce various types of particles, and (iii) the solution should ideally not involve any free parameters. All of these three conditions favored Wheeler's turn to GR, which was (i) a classical theory, (ii) dealing in universal substance (space-time), and (iii) involving no free parameters beside the gravitational constant (which could be set to 1 in what Wheeler would later call Planck units).

We thus see that also the further development of Wheeler's career in relativity closely paralleled the overall development, as questions in relativistic astrophysics (and thus the first reading of daring conservatism) gradually supplanted (or merged with) his original foundationalist aspirations, in what Roberto Lalli, Jürgen Renn and one of the authors (AB) have called the astrophysical turn of the late renaissance (Blum et al. 2018, 540f, see also Chap. 1 in this volume). It turns out then that an important factor in assessing the relevance of the epistemic potential of GR in the renaissance is the question of "potential for what?" This is true not only with regard to what problems to solve but also to what kind of work to generate. For we have clearly seen the strong pedagogical bent in the way in which Wheeler tackled general relativity and the focus on problems to be solved; the general relativity that Wheeler was exploring was swarming with future PhD theses, theses in physics, that is, connecting the heretofore isolated field of general relativity to particle physics, quantum theory, and astrophysics. It was this aspect which turned Princeton from a research center among several to the home of the "Wheeler School" (Christensen 2009; Misner 2010).

This brings us to a final paradox: How to explain the great impact of Wheeler's approach to general relativity, given that the various solutions to the elementary particle problem that we have discussed in this paper were all eventually viewed as misguided. Neither world lines and liaisons nor geons are nowadays regarded as fruitful ways for thinking about the structure of particles, and also the quantization of gravity did not yield to Wheeler's simple path integral vision. Our study at least suggests an answer to this paradox: The important thing was not so much the specific manner(s) in which Wheeler tried to resolve the elementary particle problem but rather his keen sense for which elements of general relativity would turn out to be the most fruitful.

Looking at Wheeler's trajectory thus also provides insight into where exactly the epistemic potential of general relativity lay, namely, in its unique features as a theory: the determination of the equations of motion through the field equations, the non-linearity of the field equations, and that its quantization will lead to non-trivial new physics. Conceptual studies on the role of point particles in GR could thus segue into studies on the so-called problem of motion, studies on geons into studies of exact solutions of the full Einstein equations, studies on path integral quantization would come to be regarded as important puzzle pieces in the ongoing search for a quantum theory of gravity. Here too, we observe Wheeler's trajectory closely mirroring general trends, where isolated research centers originally focusing on GR-based speculative theorizing move, in the course of the Renaissance, to the study of important conceptual questions within general relativity, relevant to the emerging community at large. The question remains to what extent Wheeler's original interests actually shaped the problems considered important in the GR community of the renaissance and beyond. But this question is beyond the scope of our study, which focused on an individual intellectual trajectory and on a conversion from particle to field theory that turned out to be far more gradual than expected. If the reader thus takes home just one fact from our story, it might be this: For a few months there, in late 1953, John Wheeler believed in both particles and fields.

Translation of John Wheeler's Tokyo Lecture

Discussion on the Problems of Elementary Particle Theory

Originally published in Japanese in **Proceedings of the Physical Society of Japan** *9, pp. 36–41 (1954)*[43]

Translated by Yukari Yamauchi (University of Maryland)[44]

Chairman Yamanouchi and members of the Physical Society of Japan!

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[43] Many thanks to the Physical Society of Japan for letting us publish this translation free of charge.

[44] The authors would also like to thank Lisa Onaga and Masato Hasegawa (both Max Planck Institute for the History of Science) for additional input. We have freely edited the translation based on our understanding of the context and the physics involved, so all mistakes should be considered ours.

I was fortunate to have a pleasant experience in Kofu before visiting here in the Tokyo region. I was deeply impressed with the energy and vision of your scientific researchers in studying the fundamental problems of physics and, at the same time, by the love of truth itself that is part of Japanese culture and philosophical tradition.

Speaking to you about the problems of elementary particles today is something quite special to me. Because I have come to a country where one of the world-renowned journals on theoretical physics is published, I think that one of your groups should be speaking. But what I would like to talk about is not to discuss each special achievement but to discuss a broad plan of research that particle physics has not attacked so far. The problem that was posed this morning – of it, we know the basic philosophy, the fundamental theory, the mathematical equations, it only remains to find the final answer. Unlike that problem, what we are facing in the field of elementary particles is to arrive at a basic principle itself. Our problem is not mathematical physics but theoretical physics. In the important mathematical and theoretical research which we heard from Professors Mott and Slater this morning, we start from basic physical ideas that look relatively simple. I think you could see how rich and complex is the development that follows from these ideas. Unfortunately, in the field of elementary particles, we still do not have the proper basic physical ideas. After thinking about how to explore this problem and about what we know from the discussion this morning and while thinking about how to present what is not yet understood and its higher aspects, I suddenly envisaged talking to two wise men, heroes in Japanese history. One of them is Sugawara, a great statesman, a man of culture, who loved truth and beauty, and the strongest defender of liberty, who maintained his principles even risking life and love. The other one is Saigo Takamori, also a great hero, with strong personality as well as great energy and influence. In my flight of imagination, it first seemed strange to be arguing about the extremely new problem of elementary particles with these two people of a long time ago and from a different era. However, soon I realized that these two people are of great interest in connection with this problem, which is attracting the attention of the leading Japanese scientists. Furthermore I discovered that they had completely different opinions on this subject.

Of the two opinions, I am familiar with the one presented by Saigo. Nevertheless, he brought new ideas into the discussion. According to what he said, we already have a fundamental theory. I followed the path of his argument carefully because I am well aware of how remarkable are the advances in electron theory in the past few years, due to new methods developed partly by Tomonaga, and partly by Schwinger, Dyson, and Feynman. In this approach we try not to solve the problem of divergences but to configure equations and ideas in such a way as to avoid talking about this blind alley of an infinite number of electrons. With this approach to the theory, it is now possible to calculate many important effects such as the microfine structure of the Hydrogen atom and radiative corrections for scattering of electrons in the Coulomb force field. I realized that Saigo Takamori thought that the meson could be treated in the same way.

We all appreciate Professor Yukawa's interesting thought of a few years ago that the force connecting the nucleons in the nucleus is related to a new particle of intermediate mass. Thanks to this theory, new particles were actually discovered soon after. And Saigo Takamori, as well as physicists of my country and many other laboratories, thought that the same renormalization theory as that of electrons should be applied to mesons as well. In connection with this, I asked what great advance in this field is expected in the future. He interprets scattering theoretically by the formation of a complex system of one meson and one nucleon, with a fixed angular momentum. This system is virtually created and disappears, so that meson nucleon scattering is understood as a kind of resonance phenomenon. He pointed out interesting research attempting to theoretically interpret this by the idea that a resonance phenomenon occurs in the so-called scattering. Many features of the problem of interaction are explained in this way, scattering of positive and negative pions on neutron and proton and neutral pi mesons on charged pi mesons. The transformation to a phenomenological explanation by this simple idea surprises not only me, but I think that we were all surprised.

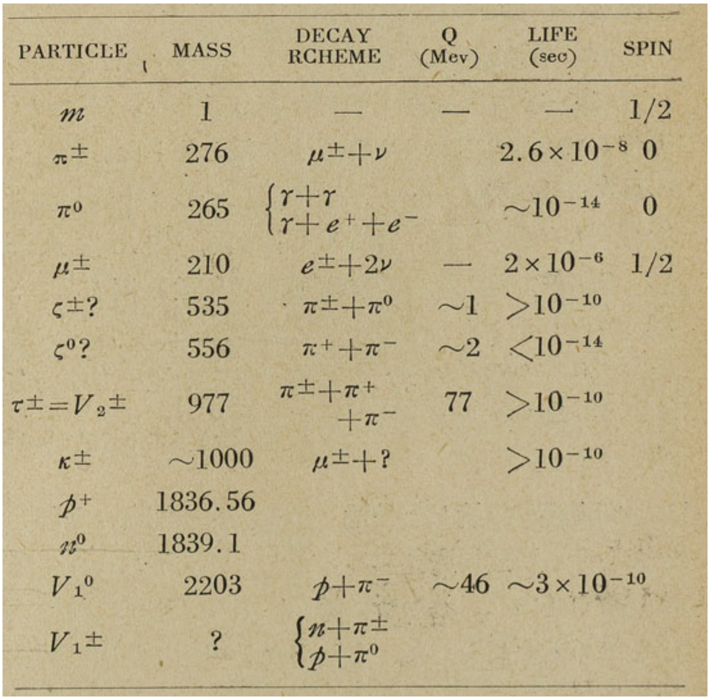
Next I discussed with Saigo Takamori a difficulty that appeared recently with this approach. Although it gives in this way a satisfactory explanation for experiments of scattering and conversion for energies on the order of 100 to 150 million electron volts, a recent experiment with energy reaching 1000 million electron volts at Brookhaven seems to show that there is another resonance in the scattering and transformation cross section that does not fit the simple image of one resonance. Saigo Takamori pointed out that we have not investigated sufficiently what kind of result is to be expected for higher resonances. But I did not enter into this problem in detail, I asked, "What is your broad general plan to get close to your problem?" He laughed and said that a first step to the main problem we'll discuss later, first of all let me show this table (Fig. 1).

This table shows what we know about elementary particles. Certainly there exist more particles that are not yet listed. Even in this table, not everything is generally accepted. Charged and neutral mesons and mu mesons are familiar and well-studied, so you do not need to mention them in particular. Regarding the ξ meson, there are still many questions about the existence of this particle and the details of its decay. The τ meson, however, is a most interesting particle, whose existence was first found at Mott's institute. The decay is such that this particle divides, among what is believed to be three pi mesons, the energy of 77 million electron volts, released after somewhat more than 10^{-10} s.

Let's then turn our attention to K- or κ-mesons. They have masses of around 1000; they decay into mu mesons and other, unknown, radiation. Very recently, these two particles and the τ mesons have come to be considered not different particles but the same particle that decays differently.

I do not need to say anything about protons and neutrons. The first type of V_0 meson that splits up into protons and pi mesons was recently made artificially by the Brookhaven accelerator for the first time. And interesting features of the decay scheme, details of the emitted energy, and mysteries of angular correlation of the emitted particles have been found. Charged V mesons decay in the same way.

But in the discussion with Saigo Takamori, his opinion on the τ meson was the most interesting to me. He pointed out the strength of the interaction between pion



PARTICLE	MASS	DECAY RCHEME	Q (Mev)	LIFE (sec)	SPIN
m	1	—	—	—	1/2
$\pi^{\pm}$	276	$\mu^{\pm}+\nu$		2.6×10^{-8}	0
π^0	265	$\begin{cases}\gamma+\gamma\\ \gamma+e^{+}+e^{-}\end{cases}$		$\sim10^{-14}$	0
$\mu^{\pm}$	210	$e^{\pm}+2\nu$	—	2×10^{-6}	1/2
$\zeta^{\pm}?$	535	$\pi^{\pm}+\pi^0$	~1	$>10^{-10}$	
$\zeta^0?$	556	$\pi^{+}+\pi^{-}$	~2	$<10^{-14}$	
$\tau^{\pm}=V_2^{\pm}$	977	$\pi^{\pm}+\pi^{+}+\pi^{-}$	77	$>10^{-10}$	
$\kappa^{\pm}$	~1000	$\mu^{\pm}+?$		$>10^{-10}$	
p^{+}	1836.56				
n^0	1839.1				
V_1^0	2203	$p+\pi^{-}$	~46	$\sim3\times10^{-10}$	
$V_1^{\pm}$	?	$\begin{cases}n+\pi^{\pm}\\ p+\pi^0\end{cases}$			

Fig. 1 Table of particles discovered by 1953

mesons. As this interaction is strong, when two or three pi mesons are in close proximity, they exert a very strong effect on each other, and, like many of the physicists I have already talked about, he thinks that in this way there is a hope that this τ meson can be analyzed by a normal quantum mechanical method as a structure coupled by force.

It would be foolish to talk about the manifold of studies that Saigo Takamori pointed to about teaching or my amazement at the number of young people returning to computing machines and desks to continue the research. Prosperity brought amazing energy to all.

Then I got an opportunity to speak with Sugawara no Michizane as Saigo got out of the discussion and was gone. Compared to Saigo, he had more lofty aspirations. He was in agreement with the study Saigo had presented, but he had different hopes for the results. He pointed out the similarity between this kind of research and the theory of superconductivity. Suppose superconductivity theory had been formed before quantum mechanics and electron theory were completed sufficiently. Since in the presence of superconductivity, the magnetic field decreases exponentially when going inside from the surface of a metal, a superconducting theory including the inherent length of exponential decay could have been made. Let us suppose that this theory was added to electromagnetic theory and electron quantum theory; that is, three fields, electromagnetic field, superconducting field, and electron field should exist.

Of course it seems absurd to interpret superconductivity like this today. We have a faith that one day we can understand the mechanism of superconductivity from the theoretical principles already established. The fundamental principles and equations are simple, but how difficult it is to keep track of what results from them was best illustrated in the lecture we heard this morning. But in the case of Meson theory, is not Saigo doing exactly the same thing? Would it not be premature to introduce a new auxiliary field, the meson field? Should we not try to understand the situation we are facing without actually introducing new hypotheses and new fields? For a long time, I have been impressed with the principle, which is the basis of the scientific method, of not introducing a new hypothesis until it is clearly and undoubtedly necessary. So this greatly calls for my support and respect. At the same time, however, his view was very unusual, so I naturally asked him for an explanation. He said he would like to call your attention to the nature of the theory we expect in the future. This theory says relativity should be a model. I was quite surprised to hear that Sugawara had brought up relativity, but what he said became clear soon. It is in general relativity that we have a closed theory for the first time. Of course it is incomplete, but as a theory it is self-consistent.

According to the field equations of general relativity, the reduced curvature tensor becomes 0 at each point in space outside a singular point. These equations are sufficient to describe not only the field itself but also the motion of the particle that occurs in the field. This remarkable feature of general relativity is consistent with the form that was desired and required for a long time as the form of a proper field theory. In addition, general relativity has another feature that is required for a basic theory. That is, it does not include natural constants or constants other than pure numbers such as 2 or π.

As I understand it, Sugawara said that it was a simple form of relativity that omits all the discussion on electromagnetic fields, the first simple relativity created to discuss mass point interactions. He pointed out that this theory is closely related to the idea of action-at-a-distance in Newton's theory of gravity, despite being a field theory. He states that the integral representation and the differential representation of the given physical law are not dissimilar from the action-at-a-distance form and the field form of the same basic physical principle, there is no big difference between them, and they are equivalent representations. He said that Mach always pointed out that space and time are the basic guiding principles of physics. According to Sugawara, the equation of general relativity is an abstraction made from this fundamental principle. Einstein made it possible to explain the inertia of matter by the interaction of a given particle with every other particle in the universe, by carrying out the program of Mach. The inertia generating force is only the gravitational analog of the radiation interaction in normal electromagnetic theory.

In this regard, the following similarities shown were the most interesting. The field generated at a distance r from a given charge is e/r^2 at short range and ea/c^2r at long range, where a represents the acceleration of the moving charge. In the same manner, he emphasized that gravity is Gm/r^2 in the short range, but in the long range the field becomes Gma/c^2r, which decreases only by the first power of the distance. In this way, when one particle interacts with a very large number of other particles, one of them being nearby and the others far away, the combined force from the nearby and distant particles results in the kind of formula given below (Equation 5).

The whole idea of gravity theory is that the total resultant force is zero. To be more precise, there is no inertia other than what arises from field theory. Therefore, when the particle 1 is acting on the nearby particle 2 and the other particles i are located in the very far distance of the universe, the equation of motion of particle 1 will be essentially in this form (omitting negligible terms):

$$\frac{Gm_1m_2}{r^2} - \sum_i \frac{Gm_1a_1m_i}{c^2\text{“}r_{i1}\text{''}} = 0 \tag{5}$$

Sugawara smiled at me while writing this equation and noted that the expression written here is just an approximation of the result of field theory in integral form. That is, he put quotes on the “$1/r$” term in the equation so that it clearly reveals that this was simply a rewrite of the true theory. He added that the second term, representing the interaction between the accelerated particle 1 with the other particles, is not exactly what we usually call the Newtonian inertial force but explained that it is the cause of normal Newtonian inertia. Long ago, Mach brought forth the idea that inertia arises from interaction with other things in the universe, and Einstein carried out this program. According to this interpretation that the inertial force is due to the gravitational interaction, the following sum is required to have a magnitude on the order of 1 in the universe we currently know:

$$\frac{G}{c^2}\sum \frac{m_k}{\text{“}r_{1k}\text{''}} \sim 1 \tag{6}$$

This equation seemingly includes natural constants, but Sugawara cautioned that if you use the appropriate units to measure the distances, these constants will all disappear. Thus, general relativity, although incomplete, has nothing appearing as a fundamental constant besides pure numbers such as 2 or π, one property to be required of every normal field theory.

Although this lesson is instructive, I wondered what was Sugawara’s real intention about what everyone would like to hear, how this relates to elementary particle theory. To that he says, to describe the problem of elementary particles, we should only use fields with a bare mass of 0 whose existence is well established. We should study whether all the particles cannot be considered to be made of such fields.

When the discussion had proceeded so far, I became a little worried whether such a fundamental idea should be reported at this kind of meeting. I think you and I have an obligation to consider this problem as clearly and carefully as possible. And if a person like Sugawara had something in mind, I thought that one needed to pay attention to it. In addition, it is very difficult to carry out this program. So I asked how you interpret electrons and other particles. What he said is much longer than I will mention here, but you can get the general idea in the next figure. He noted that the electrons we are now considering are described by a world line, the path of a charge in space-time. In such a diagram, it can be argued that positive electrons and negative electrons disappear at one point and one pair of positive and negative electrons arises at other times. Alternatively, if you wish, you can use the four-dimensional format to say that the path of a charge does not advance in one direction only, in the time direction, but also turns back in time (Fig. 2).

The next graph shows by picture what we know about the nature of electrons today. If I may use the word resolution, then if you look with weak resolving power, electrons will appear as a wide line in the space-time illustration. However, if you look at this trajectory more carefully – speaking this way regardless of the possibility of really seeing the system – when you look at the tracks more carefully, you can see that they go back and forth. In other words, it will be necessary to deal with the creation and destruction of many overlapping electrons (Fig. 3).

In the immediate vicinity of a given electron, we must deal with high-density fluctuations. As you can see, the divergence in electron theory arises from the fact

Fig. 2 Positrons as electrons going backwards in time

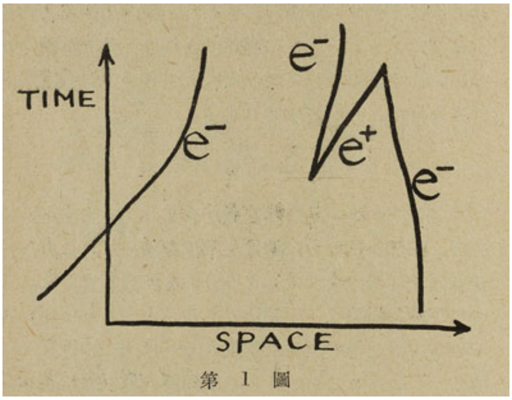
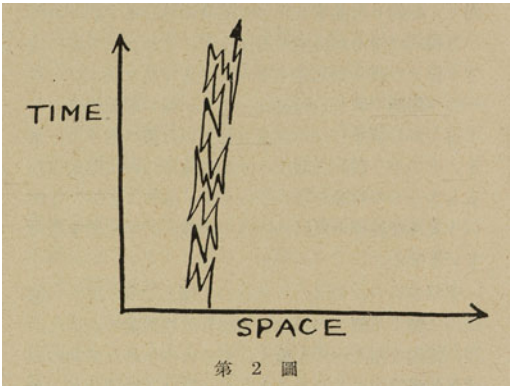


Fig. 3 Electronic *Zitterbewegung*



that the interaction between this large number of virtual electron pairs becomes infinite if we examine it carefully. The more closely you look at this process, the denser the particles of the fluctuation become. The following figure is the energy equation of these interactions calculated to the lowest relevant order in perturbation theory.

$$\left(\frac{\delta m}{m}\right)_{\substack{\text{lowest}\\ \text{order}}} = \frac{3}{2\pi}\frac{e^2}{\hbar c}\ln\frac{\lambda_{\text{max.}}}{\lambda_{\text{min.}}} \tag{7}$$

In this analysis, the ratio of the mass energy of multiple interactions to the normal mass of electrons is 1/137 times the logarithm of the maximum scale divided by the minimum scale. This formula was derived by Weisskopf a while ago, and it is an expression that we do not talk much about today. This is because, according to the new renormalization method, it is possible to argue without entering into the electron behavior in such detail. If we now return to and trust this basic idea, we have to cut off at the minimum distance $\lambda_{\text{min.}}$. Therefore, if you follow Sugawara's idea that you do not think outside of electrical and gravitational interactions, it is best to take this minimum distance as a measure of the gravitational radius of the particle. If, on top of that, all energies, and thus all masses, are due to interactions, then we cannot help concluding that the left side of the expression is equal to 1. On the right side, there is a logarithm whose value is known. Therefore, we get an expression that determines the value of the fine structure constant. Of course, this type of equation gives only the order of magnitude of the fine structure constant correctly, but it takes into account only the lowest order of interactions. If we attempt to pursue this program with a policy of eliminating all the physical constants consistently and reducing this to a problem of pure numbers and initial conditions, we have to figure out the interaction in question more closely and calculate it.

In connection with this question, I asked a question as to what other elementary particles are included. The answer was that we could describe all elementary particles in the same way. Electrons, mesons, and all other elementary particles are properly considered as fluctuations of the basic electromagnetic and gravitational fields. What about the neutrinos? I was concerned about neutrinos as I was keenly interested in recent experimental observations on the absorption of neutrinos by Reines and Cowan and the reasonable values of the cross sections they found experimentally. We are indeed deeply interested in the major role of the neutrino in elementary particle theory. We have reached the conclusion that neutrinos are released during the decay of the pi and mu meson and during beta decay.

Sugawara agrees with this conclusion and he said, in his opinion, he can choose between the following two possibilities. That is, one considers (i) the neutrino as another field like an electromagnetic field and a gravitational field of rest mass 0. In this case we will use all three of these fields to describe the form of all elementary particles or (ii) derive the neutrino from the gravitational field, that is, the result of rewriting the gravitational field in spinor form. He said that it is a subtle matter for him to decide between these two possibilities if he tried to pursue this pure field theory program. I told him that it was an important problem to be studied by all means.

I did not see the young men running here and there. I did not see a large organization with a computing machine. Sugawara talked to young people, thought about such problems, seemed to be walking around in his spare time and did not seem to have a specific program. At the end, I felt that it is appropriate to ask how to explain the differences between elementary particles and weak interaction with electrons and mu mesons and nucleons and how to explain the strong interaction between pi mesons and V-mesons. He says that it is reasonable to think that this difference is related to the type of fluctuation in the field, the microscopic form of the forward and backward movement illustrated in space-time.

I said "Is not your argument a little classical?" He laughed and reminded me of Feynman's work on obtaining quantum theory from any classical theory by using Lagrange's method in conjunction with the sum over paths. This method was also studied in Japan, especially from the viewpoint of its logical foundations.

I came back from the arguments between two great men of the past feeling that there are many things that require research on the many problems that they pointed out. I also felt that they were of the same opinion on a broad principle, only in the following sense that they do not see the problem in mutually contradictory ways. Meson theory, supported by Saigo Takamori, has been recognized by Sugawara as a kind of provisional expression of a more perfect theory. Saigo Takamori, on the other hand, is somewhat pessimistic about Sugawara's direction, but nevertheless the final theory is simple, and they agreed in expecting that it would be much closer to the beautiful form, harmonized perfectly, than any of the ones we have today. At the same time, we think that we must solve many other problems at the same time to solve the problem of elementary particles.

One person has the traditional energy and courage of Japan; the other one has a love of Japanese beauty and harmony. I feel that this country should make a great contribution to the solution of this basic problem. Thank you for your attention.

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Cosmological Constant Λ *vs.* Massive Gravitons: A Case Study in General Relativity Exceptionalism *vs.* Particle Physics Egalitarianism

J. Brian Pitts

1 Introduction

It is often noticed that there is a great gulf between the views of gravitation held by general relativists and those held by particle physicists (Kaiser 1998; Rovelli 2002; Brink 2006; Blum et al. 2015). These communities have different expectations for how gravity, especially Einstein's General Relativity, should fit into physics: general relativists tend to incline toward an exceptionalist position that expects General Relativity to be novel, dramatic, and non-perturbative, whereas particle physicists incline toward an egalitarian view that expects General Relativity to be familiar and perturbative, as just another field (in this case mass 0 and spin 2) to be quantized. As Feynman describes his approach to gravitation in his lectures on gravitation:

> Our pedagogical approach is more suited to meson theorists who have gotten used to the idea of fields, so that it is not hard for them to conceive that the universe is made up of twenty-nine or thirty-one other fields all in one grand equation; the phenomena of gravitation add another such field to the pot, it is a new field which was left out of previous considerations, and it is only one of the thirty or so; explaining gravitation therefore amounts to explaining three percent of the total number of known fields. (Feynman et al. 1995, 2; see also Kaiser 1998)

For present purposes, another relevant feature of the mental training of Feynman's theorist of mesons and nucleons is a habit of contemplating mass terms: the default relativistic wave equation for the particle physicist is the Klein-Gordon equation, not the relativistic wave equation, which is merely the special case when the field/particle in question is massless. It has been known since the 1930s that massless fields/particles are naturally associated with gauge freedom for spins greater than or equal to 1 (vectors, vector-spinors, symmetric tensors,

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etc.) (Fierz 1939). There is also a presumption of a smooth massless limit (Pitts 2011). This presumption, however, can be defeated (Zakharov 1970; van Dam and Veltman 1970, 1972), and this defeat apparently can be overcome non-perturbatively (Vainshtein 1972; Deffayet et al. 2002), a topic of much literature in the 2000s. Mass terms also make a significant difference conceptually even for the scalar case where only a smaller symmetry group, not gauge freedom, is involved (Pitts 2016c). These differing attitudes toward gravity are reflected in the quantization programs pursued in both camps.

General relativists are also far more inclined toward philosophical and historical considerations than are particle physicists. It is therefore not very surprising that historical and philosophical work on twentieth century gravitation theory would usually favor the viewpoint of general relativists, consciously or otherwise. If gravitational physics is best pursued by "amphibious" physicists (Pitts 2017b) (of whom Stanley Deser and the late Bryce DeWitt would be prominent examples), what would the history and philosophy of gravitational physics from an amphibious perspective look like?

There are examples where a particle physics-informed history and philosophy of space-time and gravitation theory would have new insights, diagnosing missed opportunities and poor arguments, offering new alternatives and arguments, and suggesting a different heuristic based on alternate expectations about how General Relativity fits into physics. Ideally the physical insight of both physics communities would be systematically integrated. One would also hope that scientific progress will gradually test each physics community's expectations in some definitive fashion, perhaps with mathematical calculations and/or computer simulations.

The 1950s–1970s witnessed a renaissance of General Relativity (Blum et al. 2015, 2016, 2017, 2018). Prior to that time, the clearly dominant party in theoretical physics was particle physics. General Relativity, when studied at all, tended to appear in departments of mathematics, and Einstein's 1915–1916 equations were not necessarily viewed as the final destination. Among particle physicists, efforts were made to quantize Einstein's equations on the model of other field theories. But little emphasis on novel conceptual lessons about space-time could arise along such lines. One might be tempted to think that such particle physics egalitarianism was an aspect of the decades of stagnation for General Relativity and that the renaissance of General Relativity partly consisted in overcoming such particle physics attitudes. There is, of course, some degree of normative coloration here, one way or the other: while a "renaissance" is supposed to be good, the goodness of an historical tendency to emphasize the distinctiveness of General Relativity depends in part on the normative question of how distinctive General Relativity in fact is, a topic that remains controversial in physics. If one were to compare the largely sound treatment of 4-dimensional symmetry in the Hamiltonian formulation of General Relativity by Rosenfeld and by Anderson and Bergmann (Rosenfeld 1930; Salisbury and Sundermeyer 2017; Anderson and Bergmann 1951) with the problem of missing change (observables as constants of the motion) in the mid-1950s (Bergmann and Goldberg 1955) and the disappearance of 4-dimensional symmetry by the late 1950s (Dirac 1958), one would quickly encounter topics of current research interest in physics (Pitts 2017a) and might less confidently expect revolutionary novelty to be helpful.

This paper briefly notes two areas where the renaissance of General Relativity saw progress that was *positively* related to the particle physicists' egalitarian attitude and then mostly attends to one more. Of the two areas merely noted, one is gravitational waves: whether they exist and whether they carry energy. This topic has been well treated (Kennefick 2007; Cattani and De Maria 1993), albeit without reference to particle physics. It might be useful, with a view to Feynman's contribution regarding the sticky bead argument, to consider how a particle physics egalitarian perspective was helpful in this context. Feynman identified "a perennial prejudice that gravitation is somehow mysterious and different" as why General Relativists "feel that it might be that gravity waves carry no energy at all" (Feynman et al. 1995, 219).

A second area of renaissance progress for General Relativity that could be discussed involves particle physicists' "spin 2" *derivation* of Einstein's equations (Gupta 1954; Kraichnan 1955; Deser 1970; Pitts and Schieve 2001; Pitts 2016a). There is a large difference between a pedestrian re-interpretation of General Relativity in terms of flat space-time and universal distortion forces (which might be viewed as an effort to miss the point of the theory by forcing it into a more familiar mold) and a *derivation* of Einstein's equations assuming flat space-time premises; the difference is especially strong regarding what justifies attending to Einstein's field equations rather than some other field equations. While these derivations admittedly sometimes have been motivated by or invoked in support of egalitarian or conservative/anti-revolutionary sentiments, the fact remains that a demonstration that one can hardly avoid Einstein's equations, given particle physicists' own criteria such as avoiding explosive instabilities (van Nieuwenhuizen 1973) (criteria far less negotiable than those of general relativists such as Einstein's involving rotation Janssen 1999; Pitts 2016b), added considerably to the justification of Einstein's equations. What would have happened if particle physics had been seen as motivating rivals to Einstein's equations? Due to limitations of space, this topic will not be discussed further here.

The last area of renaissance progress, to which this chapter mostly attends, involves substantial clarification of Einstein's 1917 confusion between a graviton mass (or the ancestor thereof) and Einstein's cosmological constant Λ. Removing this confusion both makes conceptual space for massive spin 2 gravitational theories (which encountered new problems in the early 1970s (van Dam and Veltman 1970, 1972; Boulware and Deser 1972) before the 2000s–2010s revival) and clarifies the meaning of Λ. Hence the meaning of Einstein's equations becomes clearer, even if the justification of his equations potentially becomes weaker due to the conceptualization of a serious rival in the form of massive spin 2 gravity (see Pitts 2011). Thus, perhaps ironically given the real or supposed opposition between general relativist and particle physicist viewpoints, particle physics contributed to the renaissance of General Relativity in several ways. The goal of an amphibious approach, of course, is not to replace one kind of partisanship with another kind, nor even to achieve a balance of partisanships, but to overcome the physical divide and do the history and philosophy of gravitation and space-time theory using the whole body of physical knowledge.

Unfortunately this amphibious enterprise has had so little presence historically that a polemical attitude will be inevitable toward some traditional work. While many of the authors singled out for getting the issue right are physicists, initially they are writing to correct the errors of other physicists, whether past (Einstein in the 1910s) or more current (in the 1960s). Some of the authors criticized on the historical side, *viz.* Pais and Jammer, were also physicists. They get classed in history not due to any limitations in physics participation but due to their extensive work and achievements in the history of science and for the purposes of their works considered here. It is inevitable that research physics literature will be somewhat 'ahead' of historical literature. However, the generally commendable historical emphasis on original sources, context, etc. in this case has the effect of perpetuating Einstein's mistake. In this regard, the vol. 6 of the Collected Papers of Albert Einstein (Einstein 1996a, 552) gave Einstein's faulty analogy renewed vigor by providing commentary that did not notice the corrections that had been made over the years and by citing North's flawed treatment (North 1990) (on which more below) for further discussion.

2 Cosmological Constant *vs.* Graviton Mass: A Recurring Confusion

2.1 Historical Background

In introducing his cosmological constant Λ to the world in 1917, Einstein claimed that Λ was analogous to a long-range modification of the Poisson equation that, as a matter of fact, produces a faster (exponential) decay:

> ... the system of equations [that are his field equations] allows a readily suggested extension which is compatible with the relativity postulate, and is perfectly analogous to the extension of Poisson's equation given by equation (2). (Einstein 1996a)

This analogy, unfortunately, is incorrect. Below it will appear that, after criticism in the 1940s that had negligible effect, substantial criticism from a number of noteworthy physicists appeared in the 1960s. It will also appear that historians and philosophers of physics—there seems to be little useful distinction between historians and philosophers in this context—continued to accept Einstein's faulty analogy for decades thereafter, though in the last two decades such confusion has become considerably rarer.

The idea of a graviton mass is due to 1920s work on relativistic wave equations, especially the Klein-Gordon equation, 1930s work on the Yukawa potential, and the 1930s recognition that gravity, even according to Einstein's equations, could be construed as occupying a well-defined place in the taxonomy of relativistic (at least Poincaré-invariant) wave equations (Fierz and Pauli 1939; Fierz 1940). If one expects every field theory to be quantized and to yield "particles" much as quantizing Maxwell's electromagnetism yields massless photons and quantizing de Broglie-Proca massive electromagnetism (with an additional term $-\frac{1}{2}m^2 A^\mu A_\mu$ in the Lagrangian density) yields massive photons, then quantizing Einstein's theory ought to yield massless "gravitons" and quantizing a related theory with a suitable term quadratic in the gravitational potentials ought to yield massive gravitons. Notwithstanding the quantum words and promissory notes about particles, the basic idea is just classical field theory and partial differential equations. A "mass" in effect is an inverse length scale (the conversion being effected using c and $\hbar$).

There is thus a significant pre-history of massive gravitons from the late nineteenth century. That is due especially to Neumann's and Seeliger's modification of Newtonian gravity in the 1890s with an exponentially decaying potential (Pockels 1891; Neumann 1886, 1896, 1903; von Seeliger 1896; Pauli 1921; North 1990; Norton 1999). Neumann paid considerable attention to this differential equation, whereas Seeliger tended to modify the force rather than the potential, at times using a point mass force law of $e^{-\lambda r}/r^2$ (von Seeliger 1895). Seeliger provided, if not a physical meaning (as the graviton mass later would), at least a physical motivation, namely, rendering convergent various integrals that misbehaved for Newtonian gravity with an infinite homogeneous matter distribution. Later Einstein in his popular treatment (Einstein 1996b, 362–363 of the translation) offered some criticisms of Seeliger's modification of the Newtonian force law:

> Of course we purchase our emancipation from the fundamental difficulties mentioned, at the cost of a modification and complication of Newton's law which has neither empirical nor theoretical foundation. We can imagine innumerable laws which would serve the same purpose, without our being able to state a reason why one of them is to be preferred to the others; for any one of these laws would be founded just as little on more general theoretical principles as is the law of Newton.

But the uniqueness problem does not hold in light of Neumann's mathematics (which Einstein reinvented), while the Klein-Gordon equation and Yukawa potential would later give a physical meaning to Neumann's mathematics. The empirical basis, while not empty, is merely analogous: many fields are massive, so why not the graviton field also? In the actual contingent history, Einstein was unaware of Seeliger's work until after the 'final' 1915–1916 field equations were known (Einstein 1996b, 420; 1998, 557; 1919; 2002, 189; c.f. Earman 2001).

As early as 1913, Einstein enunciated a principle to the effect that the field equations for gravity should not depend on the absolute value of the gravitational potential(s) (Norton 2007, 465; Einstein 1913, 544–545). It follows, given the concept of a mass term, that a mass term is not permitted, because mass terms make the field equations contain the potential(s) algebraically. (The cosmological constant provides a subtle way of maintaining gauge freedom with an algebraic term, but does not permit a graviton mass.) It is evident that historians of General Relativity with an eye for particle physics would be more apt to recognize how Einstein's principle generates a problem of unconceived alternatives.

2.2 Physical Background

It isn't initially obvious what connection there might be, if any, between a graviton mass and the cosmological constant Λ. A graviton mass involves a quadratic term in the Lagrangian like $-\frac{1}{2}m^2 A_\mu A^\mu$ in the electromagnetic case, which by differentiation implies a linear term in A_μ in the Euler-Lagrange equations. One might thus envisage something like $-\frac{1}{2}m^2\gamma_{\mu\nu}\gamma^{\mu\nu}$ for gravity. Whether or not one actually quantizes gravity, one can follow particle physics custom and refer to such a term as a graviton mass term. (No other terminology is available. Indices are assumed to be raised or lowered with a background metric $\eta_{\mu\nu}$. The simplest choice is a flat metric.) Massive electromagnetism was first entertained by de Broglie (1922; 1924) and developed as a field theory by Proca (1936). Massive gravity was also encouraged by de Broglie and was pursued in France during the 1940s onward (Tonnelat 1941, 1944a,b; Petiau 1944, 1945; Droz-Vincent 1959) and later also in Sweden (Brulin and Hjalmars 1959); in both countries spinor rather than tensor notation was often used (except by Droz-Vincent). Recognizably modern work appeared in the mid-1960s (Ogievetsky and Polubarinov 1965; Freund et al. 1969); the former paper is still valuable for its radical conceptual innovations (such as inventing nonlinear group realizations) achieved with binomial series expansions to take arbitrary powers of the metric tensor (expanded about the identity matrix using $x^4 = ict$!). Thus they invented many theories of gravity (including some recently reinvented) and showed how to Ockhamize the coupling of gravity to spinors as well (Ogievetskiĭ and Polubarinov 1965; Pitts 2012a) (previously partly anticipated by Bryce DeWitt's series expansions (DeWitt and DeWitt 1952; Seligman 1949) and still not widely known outside the supergravity community). Some of these results on the symmetric square root of the metric and massive gravity theories using it were reinvented in the 2010s.

Complications arise, however, with a symmetric tensor potential. A first complication is that with two indices, $\gamma_{\mu\nu}$ admits a trace $\gamma =_{\text{def}} \gamma^\mu_\mu$, which cannot justly be ignored as a participant in the mass term, so there will be a new coefficient for the new scalar term involving $m^2\gamma^2$. A second complication (partly following from the first) is that, unlike electromagnetism, gravity admits many (indeed infinitely many) relevant distinct but comparably plausible definitions of the gravitational potential. Comparing two such definitions, such as $g_{\mu\nu} - \eta_{\mu\nu}$ and $-\mathfrak{g}^{\mu\nu} + \sqrt{-\eta}\eta^{\mu\nu}$, one differs from another by how much of the trace term is mixed in and what nonlinearities are included. There is no 'correct' answer, although there are a few incorrect ones, for which the relation to the others cannot be inverted. Physical meaning of an expression such as $\gamma_{\mu\nu}$ is achieved by relating $\gamma_{\mu\nu}$ to the effective curved metric $g_{\mu\nu}$ and $\eta_{\mu\nu}$. A third complication is that the trace γ suggests a negative-energy "ghost" degree of freedom, which is likely to lead to explosive instability in quantum field theory. One can tune away this ghost at linear order (Fierz and Pauli 1939). But such a theory seems not to have the expected massless limit of General Relativity (van Dam and Veltman 1970; Zakharov 1970; Iwasaki 1970; van Dam and Veltman 1972), the van Dam-Veltman-Zakharov discontinuity, making pure spin 2 massive gravity apparently refuted by experimental data. A fourth complication is that even if one tunes away the ghost at lowest order, it reappears nonlinearly (Tyutin and Fradkin 1972; Boulware and Deser 1972), the Boulware-Deser ghost. The apparent dilemma of empirical falsification or explosive instability largely stopped research on massive gravity from *ca.* 1972 to 1999.[1] The apparent empirical need for a cosmological constant (Riess et al. 1998; Perlmutter et al. 1999), however, undermined confidence in General Relativity especially on long distance scales, thus cracking open the door for renewed consideration of massive gravity. The van Dam-Veltman-Zakharov discontinuity was plausibly resolved during the 2000s by a non-perturbative treatment called the Vainshtein mechanism, which built on an early suggestion by Arkady Vainshtein (1972) and Deffayet et al. (2002). The Boulware-Deser ghost was resolved in the 2010s (de Rham et al. 2011; Hassan and Rosen 2012) (but see the neglected early work of Maheshwari: Maheshwari 1972; Pitts 2016d; Freund et al. 1969, Note added February 1969) Thus massive gravity, not necessarily in this simple form, is now a lively field of research overcoming the GR-particle physics split, spawning review articles in prestigious places (de Rham 2014; Hinterbichler 2012) and getting some of its (re-)developers good physics jobs. After decades of darkness, massive gravity is very much a part of the current physics scene. These decades of darkness, however, have left their mark on the historical and philosophical work on gravity, in that until recently one would have had to look in unusual places to acquire any knowledge of such matters.

How can one compare an expression like $\Lambda\sqrt{-g}$ to a mass term at all? $\sqrt{-g}$ seems not to be any kind of series and is not expressed in terms of a graviton potential $\gamma_{\mu\nu}$ that vanishes when 'nothing is happening.' Rather, the trivial value for $\sqrt{-g}$ is 1, not 0. (This over-simple claim is coordinate-dependent but heuristically useful.) It also isn't clear how differentiating $\Lambda\sqrt{-g}$ to find the Euler-Lagrange equations leads to something one order lower in the potential, as one might have expected; indeed the expression $\frac{\partial\sqrt{-g}}{\partial g_{\mu\nu}} = \frac{1}{2}\sqrt{-g}g^{\mu\nu}$ is of order 1 (dominated by a zeroth order term in the potential, so to speak), just as $\sqrt{-g}$ is. One can render $\sqrt{-g}$ comparable to a graviton mass expression (quadratic in $\gamma_{\mu\nu}$) using a perturbative expansion of $g_{\mu\nu}$ about $\eta_{\mu\nu}$, defining $\gamma_{\mu\nu}$ by $g_{\mu\nu} = \eta_{\mu\nu} + \sqrt{32\pi G}\gamma_{\mu\nu}$, though suppressing the normalizing $\sqrt{32\pi G}$ is sometimes clarifying and is employed here. (Infinitely many other choices of field variables are possible (Ogievetsky and Polubarinov 1965; Pitts 2012b, 2016d), leading to differences of detail that can be important in some contexts.) The determinant g becomes a quartic polynomial in $\gamma_{\mu\nu}$ (though quadratic terms suffice for present purposes):

---

[1] The obvious exception was the Russian school of A. A. Logunov and collaborators starting in the late 1970s, which was largely ignored by others or occasionally subject to polemics (Zel'dovich and Grishchuk 1988), not without some justification. Logunov being the editor of the Russian original of *Theoretical and Mathematical Physics* and a Soviet and Russian Academician, he was able to maintain a noticeable research group with many publications.

$$g = \eta(1 + \gamma - \frac{1}{2}\gamma_{\mu\nu}\gamma^{\mu\nu} + \frac{1}{2}\gamma^2 + O(\gamma^3) + O(\gamma^4)).$$

One can find $\sqrt{-g}$ using the binomial series expansion (here with $n = \frac{1}{2}$ and using an obvious formal extension of the factorial notation for convenience)

$$(1+x)^n = \sum_{i=0}^{\infty} \frac{n!}{(n-i)!i!} x^i = \sum_{i=0}^{\infty} \frac{n \cdot (n-1) \dots (n-i+1)}{i!} x^i = 1+nx+n(n-1)x^2/2 \dots .$$

The coefficient of γ^2 gets contributions from two different terms. Thus

$$\sqrt{-g} = \sqrt{-\eta}\left(1 + \frac{1}{2}\gamma - \frac{1}{4}\gamma_{\mu\nu}\gamma^{\mu\nu} + \frac{1}{8}\gamma^2 + \dots\right):$$

the cosmological constant term in the Lagrangian density is an infinite series of powers of the graviton potential, starting with zeroth order. Thus its contribution to the field equations is also such an infinite series. While the zeroth order term in the Lagrangian density does not influence the field equations, the first-order term (perforce a constant times γ) is all-important in giving the characteristic Λ phenomenology arising from a constant term in the field equations. There are, of course, other ways of having a linear term in the Lagrangian and hence a constant in the field equations: one could simply install a term of the form $\sqrt{-\eta}(g_{\mu\nu}\eta^{\mu\nu} - 4)$, but there is little motivation for such a term in isolation. The cosmological constant, by contrast, provides a motivation for such a term. The precise tuning of linear, quadratic, and higher terms in the cosmological constant term preserves general covariance (in the sense of admitting arbitrary coordinates with only fields varied in the action and no gauge compensation fields: Pitts 2009, 2006). To have instead a graviton mass term rather than a cosmological constant, one needs to remove the linear term from the Lagrangian and hence the zeroth order term from the field equations, leaving a quadratic term in the Lagrangian and hence a linear term in the field equations. Some old (Ogievetsky and Polubarinov 1965; Freund et al. 1969; Maheshwari 1972) and recent (Hassan and Rosen 2012) works on massive gravity therefore use expressions along the lines of $\sim \sqrt{-g} + \sim g_{\mu\nu}\eta^{\mu\nu}\sqrt{-\eta} + \sim \sqrt{-\eta}$, where the coefficients are chosen to cancel the linear term in the Lagrangian (to avoid Λ phenomenology) and the zeroth order term (for the tidiness of having the Lagrangian density vanish when the graviton potential does). The details of the middle term admit considerable variety such as some constant times $g_{\mu\nu}\eta^{\mu\nu}\sqrt{-\eta}$, or $\sqrt{-g}^{84.6} g^{\mu\nu}\eta_{\mu\nu}\sqrt{-\eta}^{-83.6}$, or non-rational density weights or even non-rational powers of the metrics (Ogievetsky and Polubarinov 1965). Such generality was reinvented recently (de Rham et al. 2011; Hassan and Rosen 2012; Pitts 2016d) in order to (re)discover nonlinearly ghost-free massive gravities (i.e., theories such that there are no negative-energy degrees of freedom even when nonlinear terms are considered).

A key point is that having a mass term requires two metrics, and consequently the gauge freedom (substantive general covariance) of General Relativity is removed, leaving more degrees of freedom. It is, in fact, possible to have both a cosmological constant and a graviton mass if there are linear and quadratic terms in the Lagrangian density, but their coefficients are not related as in $\sqrt{-g}$. Cubic and higher terms can be construed as interactions and are not very important empirically in comparison to the linear and quadratic terms.

How does the cosmological constant Λ differ from a graviton mass term in its effects on the equations of motion of test particles? The geodesic equation of motion for a point test particle, assuming slow motions, weak fields, nearly Cartesian coordinates, and $-+++$ signature, has spatial components $\frac{d^2x^i}{dt^2} - \frac{1}{2}\partial_i g_{00} = 0$. Identifying this approximate result with the Newtonian result $\frac{d^2x^i}{dt^2} = -\partial_i \phi$ for the Newtonian potential ϕ, one obtains $g_{00} \approx -c^2 - 2\phi$. Because of the constant term $-c^2$, the cosmological constant leads at lowest order to a *zeroth* order term in the field equations, not to the antecedently more physically plausible nineteenth century modification with a linear algebraic term

$$\nabla^2\phi - \lambda\phi = 4\pi G\rho.$$

One can assess the relative sizes of the terms such as $-c^2$ and -2ϕ using Newton's constant $G = 6.674 \cdot 10^{-11}\text{N} \cdot \text{m}^2\text{kg}^{-2}$, the masses of the Sun and the Earth, the radius of the Earth's orbit, the radius of the Earth, and the speed of light. Taking $\phi \approx -\frac{GM}{r}$ (which will hold approximately for equations approximating the Poisson equation for Newtonian gravity), one has for the potential from the Sun at the Earth

$$\phi \approx -(1.99 \cdot 10^{30}\text{kg}) \cdot (6.674 \cdot 10^{-11}\text{N} \cdot \text{m}^2\text{kg}^{-2})/(1.50 \cdot 10^{6}\text{km}) \approx -8.85 \cdot 10^{9}\frac{\text{m}^2}{\text{s}^2}.$$

The potential from the Earth at its surface is analogously

$$\phi \approx -(5.97 \cdot 10^{24}\text{kg}) \cdot (6.674 \cdot 10^{-11}\text{N} \cdot \text{m}^2\text{kg}^{-2})/6371\text{km} \approx -6.25 \cdot 10^{7}\frac{\text{m}^2}{\text{s}^2}.$$

By contrast $c^2 \approx 9 \cdot 10^{16}\frac{\text{m}^2}{\text{s}^2}$. Thus typical values of the potential for both terrestrial and solar system effects are vastly smaller (in absolute value) than c^2. The strange (Freund et al. 1969; Schucking 1991) zeroth order term will tend to dominate over the intended linear term in ϕ. The potential grows quadratically and, if $\Lambda > 0$, is repulsive in Einstein's theory, giving an anti-oscillator force (proportional to distance like a spring, but with the 'wrong' sign). By contrast a graviton mass term leaves the gravitational force attractive but merely makes it decay faster than $\frac{1}{r^2}$

at long distances due to exponential decay.[2] Eddington, without comparing Λ to a graviton mass (or Neumann's antecedent) or criticizing Λ as bizarre, described the phenomena fairly adequately in 1923 (Schucking 1991):

> Hence $\frac{d^2r}{ds^2} = \frac{1}{3}\lambda r$..................(70.22).
> Thus a particle at rest will not remain at rest unless it is at the origin; but will be repelled from the origin with an acceleration increasing with distance. (Eddington 1952, 161)

It is easy to imagine that such a description would help to draw attention to the difference between Λ and the idea of a graviton mass, which would become easier and easier to conceive during the 1920s and 1930s. But such a result seems not to have occurred. A subtler mistake that arose during the 1960s held that the cosmological constant was exactly analogous to a Neumann and Seeliger-type long-modification of gravity, if not for *static* fields, at least for gravitational wave propagation.

Since the 1910s an exact solution for Einstein's equations with spherical symmetry and a cosmological constant Λ has been known due to Kottler (1918), Weyl (1919), Perlick (2004), Uzan et al. (2011), and Ohanian and Ruffini (1994, 177, 397). It is now known among physicists (with naming conventions more suited to economy by taking equivalence classes under coordinate transformations than to historical accuracy) as the Schwarzschild-de Sitter solution. The solution is

$$ds^2 = -\left(1 - \frac{2GM}{r} - \frac{\Lambda r^2}{3}\right) dt^2 + \left(1 - \frac{2GM}{r} - \frac{\Lambda r^2}{3}\right)^{-1} dr^2 + r^2(d\theta^2 + \sin^2\theta d\phi^2). \tag{1}$$

From this expression one sees from g_{00} that, since the potential $-\frac{GM}{r}$ gives an attractive force proportional to $-\frac{GM}{r^2}$, the cosmological constant analogously gives an additional potential $-\frac{\Lambda r^2}{6}$ yielding a repulsive force (for $\Lambda > 0$) proportional to $\frac{\Lambda r}{3}$ and independent of M, a force that grows with distance and eventually dominates the attraction from the heavy body of mass M. The comment by Freund, Maheshwari, and Schonberg is worth recalling:

> A "universal harmonic oscillator" is, so to speak, superposed on the Newton law. The origin of this extra "oscillator" term is, to say the least, very hard to understand. (Freund et al. 1969)

That description seems to fit $\Lambda < 0$ especially, whereas a positive Λ's repulsive anti-oscillator potential seems even worse.

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[2] Mathematically with the graviton mass term one has a superposition of exponentially decaying and exponentially growing factors times $\frac{1}{r}$, but one routinely discards the growing solution on grounds of physical reasonableness. With the zeroth order term in the field equations, by contrast, there are no solutions to spare and a (quadratically) growing solution cannot be discarded.

3 Physicists Come to Reject the Λ-Graviton Mass Conflation

3.1 Otto Heckmann (1942)

Einstein's mistake seems to have been noticed first in 1942 by Otto Heckmann in Germany (Heckmann 1942). (For more on Heckmann, see Hubert Goenner's encyclopedia article Goenner 2018.) Previously Heckmann and coauthor Siedentopf had embraced Einstein's analogy (Heckmann and Siedentopf 1930, p. 88), claiming that Einstein's equations with a cosmological constant (their equation (5, 17)) has as an approximation $\Delta V + \lambda^2 V = -4\pi \chi \rho$, their equation (5, 18). Heckmann's critique of Einstein's analogy[3] thus involved a retraction. This passage is freely rendered in English by Harvey and Schucking:

> The equation $\Delta + \lambda\phi = 4\pi G\rho$ [*sic*: ϕ is missing from the first term] which is different from $\Delta + \Lambda_0(t) = 4\pi G\rho$ [*sic*: again ϕ is missing from the first term] is used by Einstein in his paper S.-B. Preuss. Acad. Wiss. *1917*, 142 to explain the introduction of the term $\lambda g_{\mu\nu}$ into his field equations. This suggestion for a change of Newton's law (C. Neumann: "About the Newtonian Principle of Action at a Distance," p. 1 and 2, Leipzig 1896—see also Leipziger Ber. Math.-Phys, Kl. 1874, 149) does *not* result as an approximation of the field equations of relativity theory. Thus, the argument which Heckmann and Siedentopf [footnote suppressed] gave for their Eq. (5.18) is void. (Harvey and Schucking 2000, emphasis in the original).

However, given the wartime focus on the practical, the divide between Allied and Axis countries, and the crude Nazi opposition to Einstein's relativistic physics and so-called 'Jewish' physics more generally, Germany in 1942 was not an opportune time and place for serious criticisms of some specific aspect of General Relativity to draw worldwide notice. Heckmann's book was republished in 1968 but still not widely read (Harvey and Schucking 2000).

3.2 Bryce DeWitt

In his distinctive mathematical style, Bryce DeWitt made clear in his 1963 Les Houches lectures (published both in the proceedings (DeWitt and DeWitt 1964) and as a separate book DeWitt 1965) that a cosmological constant is quite distinct from a graviton mass. A graviton mass requires a background geometry, which the cosmological constant does not involve. He further emphasized the connection

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[3] "In diesem Zusammenhang sei folgendes bemerkt: Die von (21) verschiedene Gleichung $\Delta\Phi + \lambda\Phi = 4\pi G\rho$ wird von EINSTEIN in der S. 2. Anm. 4 zitierten Arbeit zur Erläuterung der Einführung des Gliedes $\lambda g_{\mu\nu}$ in seine Feldgleichungen herangezogen. Dieser bereits von C. NEUMANN gemachte Abänderungsvorschlag des NEWTONschen Gezetzes (vgl. S. 1 Anm. 1) ergibt sich aber *nicht* als Näherung aus den Feldgleichungen der Relativitätstheorie. Damit ist die Begründung, die HECKMANN und SIEDENTOPF [Z. Astrophys. **1**, 67 (1930)] für ihre Gleichung (5, 18) gegeben haben, hinfällig." (Heckmann 1942, 15, emphasis in the original).

between mass terms and smaller symmetry groups. It was recognized in the late 1930s that whereas massive particles/fields naturally lack gauge freedom, massless particles/fields, at least for spin 1 and higher, naturally have gauge freedom and correspondingly fewer degrees of freedom (Pauli and Fierz 1939; Fierz and Pauli 1939). Thus DeWitt points out that the cosmological constant Λ does not shrink the symmetry group, as a real graviton mass term would do, but leaves the general relativistic gauge (coordinate) freedom. Thus this is another way to see that Λ does not give a graviton mass. (It is in fact possible to have both a cosmological constant and a graviton mass, but that is another matter, and subtle questions of definition arise.)

As background for his remarks on gravity (spin 2), one can consider the simpler and uncontentious case of electromagnetism. Instead of gauge freedom (as in Maxwell's "massless photon" electromagnetism), one has as consequences of the field equations, for the case of massive electromagnetism, in effect the Lorenz-Lorentz condition $m^2 \partial_\mu A^\mu$ on the potentials. Thus the time-like potential A_0 is not an independent degree of freedom. For sufficiently high spins (such as 2), there is also the possibility of taking the trace and identifying a scalar field within the tensor field content. If gravity is to be pure spin 2, then this trace must vanish. DeWitt contemplates whether these conditions hold on small disturbances, such as waves.

He wrote:

> On comparing equation (16.10) [a complicated expression for the second functional derivative of the action for General Relativity] with equation (6.69) one is at first sight led to infer that small disturbances in the metric field propagate like those of a tensor field of rest mass $m = (-\lambda)^{1/2}$ ($\hbar = c = 16\pi G = 1$). This inference is incorrect, however, for two reasons. In the first place the concept of rest mass requires for its definition the presence of an asymptotically flat space-time. Indeed space-time is assumed to be everywhere flat in the linearized theory to which equation (6.69) refers, whereas, in virtue of (16.9) [Einstein's equations with the cosmological constant], the background field of equation (16.10) cannot be even asymptotically flat. It is true that homogeneous isotropic cosmological solutions of equations (16.9) exist which can provide a background field with respect to which a decomposition of small disturbances into positive and negative frequency components can be effected just as for flat space-time theories. However, these components necessarily have physical properties which differ to such an extent from the plane wave components of flat space-time theories that the rest mass concept is no longer valid.
>
> In the second place, since the operator (16.10) does not have a form precisely analogous to that of the tensor field of mass m, it does not lead to conditions $g^{\mu\nu}\delta g_{\mu\nu} = 0$, $\delta g_{\mu\nu.}{}^{\nu} = 0$ on the small disturbances, analogous to the conditions [of vanishing trace and 4-divergence] of part (c) of Problem 3. In fact, if one attempts to repeat, in generally covariant form, the arguments which, in the flat space-time theory, lead to such conditions, one finds in virtue of the lack of commutativity of covariant differentiation, that a Ricci tensor always appears in such a way as completely to cancel the cosmological constant via the dynamical equations (16.9). The basic reason for this, of course, is that the coordinate transformation group is still present as an invariance group for the gravitational field, and the operator (16.10), despite appearances, is a singular operator. The time-like components of $\delta g_{\mu\nu}$ are therefore not dynamically suppressed, and $\delta g_{\mu\nu}$ may, in fact, satisfy four arbitrary supplementary conditions at each space-time point. (DeWitt 1965, 131–132, footnotes suppressed)

Whether or not DeWitt had interests in the history of the question, this was a clear, deep, authoritative, and twice-published critique of the confusion between a cosmological constant and a graviton mass. It might have helped that DeWitt straddled the GR-particle physics border.

3.3 Hans-Jürgen Treder, Andrzej Trautman, and Wolfgang Rindler

Critiques of Einstein's analogy and related ideas from Treder, Trautman, and Rindler are all somewhat similar. It is noteworthy that in contrast to DeWitt, these three figures are closer to paradigm general relativists.

In comparison to DeWitt, Treder made a more explicitly polemical attack on the idea that the cosmological constant gives gravitons a rest mass (Treder 1963, 1968). The claim that Λ implies a graviton mass is a higher-tech version of Einstein's analogy between Λ and the modified Poisson equation. While mentioning "authors" he cites the book on unified field theories by Marie-Antoinette Tonnelat (1965). Treder introduces Einstein's equations and compares two wave equations, one with the cosmological constant and one with a graviton mass (Treder 1963). Regarding the equation with the cosmological constant (a zeroth order term in the wave equation), he says:

> [t]he constant λ cannot be set proportional to k^2, where k^{-1} is the Compton wavelength of the graviton. If we form the equivalent of (1.7) [the linearized wave equation without the cosmological constant] to the new equations (1.10) [Einstein's equations with the cosmological constant], we get
>
> $$\frac{1}{2}\Box\gamma_{\mu\nu} + \lambda(\eta_{\mu\nu} + \gamma_{\mu\nu}) = 0. \tag{1.11}$$
>
> So that, however, λ could in essence be k^2, instead of (1.11)
>
> $$\frac{1}{2}\Box\gamma_{\mu\nu} + \lambda\gamma_{\mu\nu} = 0 \tag{1.11a}$$
>
> would have to apply" [references suppressed] (Treder 1963)[4]

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[4] "Die Konstante λ kann nun aber nicht proportional k^2 gesetzt werden, wobei k^{-1} die Comptonwellenlänge des Gravitons wäre. Bilden wir nämlich das Äquivalent von (1.7) [the linearized wave equation, without the cosmological constant] zu den neuen Gleichungen (1.10) [Einstein's equations with the cosmological constant], so erhalten wir

$$\frac{1}{2}\Box\gamma_{\mu\nu} + \lambda(\eta_{\mu\nu} + \gamma_{\mu\nu}) = 0. \tag{1.11}$$

Damit aber λ im wesentlichen k^2 sein könnte, müßte anstelle von (1.11)

$$\frac{1}{2}\Box\gamma_{\mu\nu} + \lambda\gamma_{\mu\nu} = 0 \tag{1.11a}$$

gelten" [references to papers by L. de Broglie and M.-A. Tonnelat].

The contrast between the two wave equations is clear: the cosmological constant introduces a zeroth order term $\lambda\eta_{\mu\nu}$, which is more important for weak fields than is the first order term $\lambda\gamma_{\mu\nu}$ that would signify a graviton mass. He also mentions how general covariance is retained, and hence there are only two wave polarizations even with the cosmological constant.

In the later paper (1968), Treder discusses a tempting mistake with the wave equations that people claiming that a cosmological constant gives a graviton mass sometimes make. (This is a more sophisticated mistake than Einstein's original one.)

> The authors who interpret the cosmological constant λ like the square of the rest-mass of gravitons…, put forward as a general argument for their opinion that the variation of the cosmological equation (1) gives Yukawa-type equations for the perturbations of the gravitational field. (Treder 1968)

But this is wrong, as one sees once one keeps track of the covariant derivatives.

> Therefore, the terms of [the weak field wave equation] with the cosmological constant λ are compensated for by the terms with the Ricci tensor.
> The result is that the same propagation equations (17) for the perturbations δg_{kl} result from the cosmological equations (1) as from the equations
>
> $$R_{kl} = 0.$$
>
> This means that the final form of the propagation equations for the perturbations of the gravitational field is independent of the existence of a cosmological term in the Einstein vacuum equations. Therefore, the gravitons connected with these perturbations have zero rest-mass for a cosmological constant $\lambda \neq 0$ too. The cosmological constant does not have any connection with a graviton rest-mass. (Treder 1968)

Thus Treder's and DeWitt's points are basically the same, apart from Treder's explicit polemical aim.

Andrzej Trautman, a very mathematical general relativist, accurately critiqued the Λ-graviton mass confusion in some book-length lectures at Brandeis in 1964, along with a brief passable reference to the history (Trautman 1965, 228–231). After discussing Olbers' paradox and the problem of the diverging Newtonian potential for an infinite homogeneous matter distribution, Trautman says:

> Neumann and Seeliger (in 1895) proposed the idea of replacing Poisson's equation by
>
> $$\nabla^2\phi - \lambda\phi = 4\pi k\rho.$$
>
> This corresponds to assuming that the gravitational forces have a finite range with $1/\sqrt{\lambda}$ being the characteristic length for the gravitational interactions.…
> …Einstein modified the field equations by adding a cosmological term
>
> $$\lambda g_{ab} + R_{ab} - \frac{1}{2} g_{ab} R = -8\pi k T_{ab} \tag{9.2}$$
>
> However, these equations are not the analog of the Neumann-Seeliger equation in the Newtonian limit but go over into
>
> $$\nabla^2\phi + \lambda c^2 = 4\pi k\rho.$$
>
> (Trautman 1965, 229–230)

While not linking the Neumann-Seeliger nineteenth-century idea to the 1920s–1930s physical meaning of a graviton mass or being explicitly interested in Einstein's own making of the analogy, Trautman nonetheless makes clear the distinction between the two things that Einstein had presented as analogous.

Wolfgang Rindler also gently criticized the analogy between Λ and the exponential decay of Neumann and Seeliger (Rindler 1969). The problem of the infinite potential in a homogeneous matter distribution in absolute space:

> …led Neumann and Seeliger in 1896 to suggest that the Newtonian potential of a point mass be replaced by
>
> $$\phi = -\frac{mG}{r} e^{-r\sqrt{\lambda}}, \qquad (\lambda = constant \approx 0), \qquad (82.1)$$
>
> whose integral would remain finite. (Note that this is identical in form with Yukawa's mesonic potential put forward in 1935.) Poisson's equation [footnote suppressed] $\nabla^2\phi = 4\pi G\rho$ then becomes
>
> $$\nabla^2\phi - \lambda\phi = 4\pi G\rho, \qquad (82.2)$$
>
> which possesses the obvious solution
>
> $$\phi = -\frac{4\pi G\rho}{\lambda} \qquad (82.3)$$
>
> in a homogeneous universe. (This results also on integrating (82.1) throughout space for a continuous distribution of matter.)
> It is interesting to observe the striking formal analogy between Einstein's modification (79.10) of his original field equations (79.7) and Neumann and Seeliger's modification (82.2) of Poisson's equation. However, in first approximation (79.10) does not reduce to (82.2) but rather to another modification of Poisson's equation, namely
>
> $$\nabla^2\phi + c^2\Lambda = 4\pi G\rho, \qquad (82.4)$$
>
> as can be shown by methods similar to those of Section 79. This *also* admits a constant solution in the presence of homogeneous matter, namely $\phi = 0$, *provided* $c^2\Lambda = 4\pi G\rho$—a situation which obtains in Einstein's static universe, for which, indeed, Einstein originally introduced his Λ term. (Rindler 1969, 222–223)

Though the contributions of Neumann and Seeliger are run together, this description in an accessible textbook is both historically and technically serviceable.

3.4 Peter G. O. Freund, Amar Maheshwari, and Edmond Schonberg

One of the clearest distinctions between a cosmological constant and a graviton mass comes from a significant particle physics-flavored paper (in the *Astrophysical Journal*!) putting forward a theory of massive gravitons (Freund et al. 1969).

Einstein’s theory with the cosmological constant is faulted on multiple grounds, of which here is the second.

> B. In the “Newtonian” limit it leads to the potential equation,
>
> $$\Delta V + \Lambda = \kappa\rho. \tag{1}$$
>
> Correspondingly, the gravitational potential of a material point of mass M will be given by
>
> $$V = -\frac{1}{2}\Lambda r^2 - \frac{\kappa M}{r}. \tag{2}$$
>
> A “universal harmonic oscillator” is, so to speak, superposed on the Newton law. The origin of this extra “oscillator” term is, to say the least, very hard to understand. (Freund et al. 1969)

By contrast, their proposed massive graviton theory is far more reasonable by the standards of particle physics, and because it violates general covariance, it is easier to quantize as well.[5]

> In the Newtonian limit, equation (1) is now replaced by the Neumann-Yukawa equation,
>
> $$(\Delta - m^2)V = \kappa\rho, \tag{3}$$
>
> which leads to the quantum-mechanically reasonable Yukawa potential,
>
> $$V(r) = -\frac{\kappa M e^{-mr}}{r}, \tag{4}$$
>
> rather than the peculiar oscillator of equation (2). Difficulty B is thus removed.

Thus during the 1960s, quite a few leading physicists took aim at the confusion between the cosmological constant and the graviton mass, confusion that apparently had gone unchallenged apart from Heckmann’s little-noticed work.

4 Engelbert Schucking’s Decisive Influence

While some authors since the 1960s have criticized Einstein’s conflation of his cosmological constant with a graviton mass, Engelbert Schucking (also spelled “Schücking”) later mounted a sustained assault on that error (Schucking 1991;

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[5] They were presumably not reckoning sufficiently with the ghost problem, though they did discuss it. But their appendix is evidently the first public appearance of a *nonlinearly* ghost-free, that is pure spin 2, massive gravity, singled out from among the OP 2-parameter family of theories. The nonlinear *argument* was published later (Maheshwari 1972) (submitted no later than early April 1971). This (Tyutin-Fradkin-)Boulware-Deser nonlinear ghost problem was pre-solved before it was proposed. But no one noticed and the problem had to be solved again in 2010. Thus the decades of darkness for massive gravity were quite contingent. Maheshwari was unaware of the van Dam-Veltman-Zakharov discontinuity, however.

Harvey and Schucking 2000). Schucking's first work appeared in a *Festschrift* for Peter Bergmann, a work likely to be read only by physicists, though the content is substantially historical. Schucking's work on the Einstein Papers Project also implies that one could as plausibly list his contribution among the historians and philosophers to be treated later as among the physicists treated above, though his work somehow had no influence on the treatment of the analogy in the Einstein Papers Project. Given the difficulty of classifying him and the transformative nature of his interventions, it seems fitting to devote a separate section to his influence.

His first writing is so accurate, brief, and vigorous that it is tempting to quote the whole thing, though one might wish that Seeliger got mentioned along with Neumann. Here is a substantial portion.

> To motivate the introduction of this new constant of nature without a wisp of empirical evidence he wrote that his new extension was "completely analogous to the extension of the Poisson equation to
>
> $$\Delta\phi - \lambda\phi = 4\pi K\rho \qquad \text{''} \tag{3}$$
>
> This remark was the opening line in a bizarre comedy of errors.
>
> Einstein's modified Poisson equation is now familiar to all physicists through the static version of Yukawa's meson theory which has the spherically symmetric vacuum solution
>
> $$\phi = \frac{\text{const}}{r} e^{-r\sqrt{\lambda}}, \qquad \lambda = \left(\frac{mc}{\hbar}\right)^2, \qquad r = (x^2 + y^2 + z^2)^{\frac{1}{2}}. \tag{4}$$
>
> But this equation had a deeper root. The Königsberg theoretician Carl Neumann (Neumann 1896) had proposed the modified Poisson equation to introduce an exponential cut-off for the gravitational potential. He thus anticipated Einstein's worry about the disastrous influence of distant stars on the potential. Einstein, apparently, was not aware of Neumann's work in 1917.
>
> It is true that the Poisson equation modified by a term $-\lambda\phi$ (with a positive λ) on its left hand side leads to an exponential cut-off for the gravitational potential. But Einstein's flat assertion that the λ -term in his field equations had a completely analogous effect was wrong. However generations of physicists have parroted this nonsense. Even Abraham Pais (1982) writes in his magisterial Einstein biography about the analogy between the λ-terms in Poisson's and Einstein's equations "he (Einstein) performs the very same transition in general relativity."
>
> It seemed so deceptively obvious: the potential corresponds in the Newtonian approximation to $(c = 1)$
>
> $$g_{00} = -(1+2\phi). \tag{5}$$
>
> Thus adding a term $-\lambda\phi$ to $\Delta\phi$ might correspond to inserting a term $-\lambda g_{\mu\nu}$ in addition to the Ricci tensor whose 00-component gives essentially the Laplacean in Newtonian approximation.
>
> I still remember when Otto Heckmann told me 35 years ago: "Einstein's [sic] Argument ist naturlich Quatsch (baloney)." And the late Hamburg cosmologist was right. For ϕ is ϕ/c^2 and can be neglected compared to one in first approximation. Thus the Newtonian analog of Einstein's equations with λ-term is not the modified Poisson equation (3) but
>
> $$\Delta\phi + \lambda c^2 = 4\pi K\rho. \tag{6}$$

> With equation (6) Einstein had not introduced an exponential cut-off for the range of gravitation but a new repulsive force ($\lambda > 0$), proportional to mass, that pushed away every particle of mass m with a force
>
> $$\vec{F} = mc^2 \frac{\lambda}{3} \vec{x}, \tag{7}$$
>
> a force derivable from the repulsive oscillator potential $-\lambda c^2 r^2/6$. This was clearly stated by Arthur Eddington (Eddington 1923)....
>
> Instead of getting a shielded gravitation one had now at large distances almost naked repulsion. This was quite different from the expected bargain. (Schucking 1991, 185–186)

This Italian *Festschrift* for Peter Bergmann was probably a bit too obscure and technical to reach the widest relevant audience, however. As perhaps a foretaste of how old errors die hard, Joe Weber in the same volume (Weber 1991) made the same mistake that Schucking corrected! At least Weber was thinking about experimental tests of a graviton mass, calling attention to Zwicky (1961), who was influenced by his Caltech colleague Feynman. One might think that Schucking's work has already said everything that needs saying. But the persistence of unclear and even erroneous ideas in the newer historical literature, after both the 1960s physics corrections and in some cases after Schucking's blasts, shows that the topic requires continuing discussion. Doubtless his own personal connection to Heckmann played a role in his work on this topic. A more recent article by Alex Harvey and Engelbert Schucking (2000), published in a more visible and pedagogical place (*American Journal of Physics*), takes much the same message (with a fair amount of reused text) to a larger audience.

5 History and Philosophy of Science and Einstein's Analogy

Unfortunately, historical treatments of Einstein's cosmological constant Λ and its relevance to the Seeliger-Neumann modification of Newtonian gravity have not always been reliable. That is despite the fact that some of those here classed as historians were trained as and long operated as physicists; their classification as historians here reflects more their high achievements in history than any dearth of participation in physics. This section might be an interesting case study on the need for an at least partly internalist history of science, in that a purely externalist view would not be motivated to inquire further to trace the origins of erroneous claims. It also helps to illustrate how systematic neglect of particle physics by historians and philosophers of General Relativity leads to errors and oversights.

5.1 John D. North

John D. North addressed the issues in question on more than one occasion in his historically rich work. His classic 1965 work starts well in its discussion of Seeliger and Neumann, but later tends to confuse their idea with that of Laplace and with Einstein's cosmological constant.

> In 1895 Seeliger began by protesting that as the volume (V) of a Newtonian distribution of matter of finite density tends to infinity, the gravitational potential at any point can be assigned no definite value; added to which the expression for the gravitational force also becomes indefinite. Carl Neumann, faced by the same difficulties, proposed that Poisson's equation should be adjusted so as to permit a uniform and static distribution of matter throughout space. For the gravitational potential they took expressions of the usual Newtonian form, multiplied by an additional factor $e^{-\alpha r}$, where α is a quantity sufficiently small to make the modification insignificant, except for large distances.…
> …On the other hand, neither were Seeliger and Neumann first with the exponential law: Laplace had taken this very law fifty years before. In all the earlier cases, however, the concern was in only a narrow sense cosmological.
> The effect of the exponential modifying factor is to introduce a cosmical repulsion capable, at large distances, of exceeding the usual gravitational forces. As will be seen in due course, the introduction of what was to be known as the 'cosmological term' into the later gravitational field equations of Einstein is reminiscent of Neumann's modification of Poisson's equation. (North 1990, 17–18, footnotes suppressed)

Here Seeliger's diversity of mathematical expressions has been pared down to match the more unique and optimal expression of Neumann. North's effort to find an antecedent in Laplace (1846, book 16, chapter 4, 481), unfortunately, confused Laplace's multiplication of a $\frac{1}{r^2}$ *force* by an exponentially decaying factor with Neumann's multiplication of the $\frac{1}{r}$ *potential* by such an exponential factor. (The same mistake was made by Erich Robert Paul 1993, 69. Laplace's modified force law is not obviously the solution to any relevant linear differential equation or connected with any clear physical meaning of current interest, so Neumann's exponential decay modifying the potential is more plausible. And North is in mathematical error in holding that the exponential modifying factor introduces "a cosmical repulsion capable, at large distances, of exceeding the usual gravitational forces." The exponential decay merely causes the gravitational attraction to weaken faster than it otherwise would. He seems to be warming up for confusing Neumann's exponentially decaying factor in the potential (or its ancestor in the relevant differential equation, the modified Poisson equation) with Einstein's cosmological constant Λ.

North unfortunately makes a version of Einstein's erroneous conflation in discussing the Milne-McCrea modified Newtonian cosmology that sought to encompass much of relativistic cosmology within a simpler framework. This work permitted a λ term, an introduction by hand of a term like Einstein's cosmological constant Λ into Newtonian equations (McCrea and Milne 1934). According to North:

> (The λ-term in the equation of motion is precisely that which Neumann and Seeliger had introduced, nearly forty years before, in the hope of explaining how an infinite and static universe was possible.) (North 1990, 179)

North's more recent work seemed not to profit fully from the somewhat greater visibility of critiques of Einstein's Λ error that had become available:

> When applied to cosmological problems assuming an infinite Universe, ordinary Newtonian theory, based on the familiar (Euclidean) geometry, seemed to lead to inconsistencies. In fact Carl Neumann and Hugo von Seeliger—to name only two—tried to modify the Newtonian law of gravity to remove these difficulties. In doing so, strangely enough, they introduced what was effectively a cosmical repulsion (one that they supposed worked against the much more powerful gravitational attraction), which has its counterpart in later relativistic cosmology. (North 1994, 515–516)

5.2 *Max Jammer and Abraham Pais*

Max Jammer's generally impressive and erudite work has, unfortunately, also tended to perpetuate Einstein's confusion about Λ (Jammer 1993, 1997). Jammer wrote in *Concepts of Space*:

> Einstein's introduction of the cosmological constant λ, by which he hoped to remove the inconsistency with Mach's Principle, stands in striking similarity to H. Seeliger's modification of the classical Laplace-Poisson equation $\Delta\varphi = 4\pi\rho$ into $\Delta\varphi - \mu^2\rho = 4\pi\rho$, [*sic*—the left side should contain φ rather than ρ] whereby Seeliger attempted to relieve Newtonian cosmology from certain inconsistencies. The positive constant μ should be chosen so small that within the dimensions of the solar system the solution of the original equation (i.e., $\varphi = -m/r$) and that of the supplemented equation ($\varphi = -\frac{m}{r}e^{-\mu r}$) should coincide within the margin of observational error. (Jammer 1993, 194–195, footnotes suppressed).

(There is no change from the 1969 edition.) While criticisms of Einstein are admitted, there seems to be no expectation that they would come from much later decades (1940s–1960s) and be facilitated by particle physics. Notwithstanding the citation of Seeliger's 1895 and 1896 papers, Jammer's Seeliger looks more like Neumann (who does not appear here in Jammer's account) or a composite figure propounding a fusion of Seeliger's physical arguments and Neumann's mathematics in presenting a unique plausible differential equation, rather than a variety of more or less arbitrary modified force laws as Seeliger in fact gave.

More recent historical works show no monotonic improvement. Pais's work is similar to Jammer's on this point; thus Einstein's standard scientific biography did no better in the 1980s, well after the corrections were available in the physics literature, than Jammer had in the 1960s. Pais discusses Einstein's proposed modification of Newtonian gravity well enough, but then endorses Einstein's analogy. If the Newton-Poisson equation

$$\Delta\phi = 4\pi G\rho \tag{15.17}$$

…is replaced by

$$\Delta\phi - \lambda\phi = 4\pi G\rho \tag{15.18}$$

(a proposal which again has nineteenth century origins), then the solution [with uniform matter density ρ and gravitational potential ϕ] is dynamically acceptable.

…Let us return to the transition from Eq. 15.17 to Eq. 15.18. There are three main points in Einstein's paper. First, he performs the very same transition in general relativity…(Pais 1982, 286).

5.3 Pierre Kerszberg

In a generally fascinating and illuminating book written in the late 1980s, Kerszberg seems to have lacked the concept of a graviton mass and consequently had an obsolete view of the plausibility of a Neumann-Seeliger modification of the Newtonian potential, while also falling into Einstein's false analogy (Schucking commented on Kerszberg's book (Schucking 1991)). Kerszberg writes:

> By the end of the nineteenth century, Seeliger and Neumann proposed a modification of the Newtonian law of gravitation that would make no perceptible difference within the solar system but would dispense with the disturbing increase of attraction over larger distances.… Suffice to say here that this modification was designed to restore homogeneity at large, and that it was just as ad hoc a solution as the absorbing matter had been in the case of the optical paradox. (Kerszberg 1989, 50).

The promised further discussion in the next chapter largely embraces Einstein's analogy:

> In fact Einstein introduces the cosmological constant by again appealing to a parallel with the Newtonian theory. As he reminds us at the end of the analysis of Newtonian cosmology with which he opened his memoir, the strategy for overcoming the paradoxes of the island universe involves what looks like a piece of similar contrivance. It was in the third edition of his popular exposition of relativity that Einstein gave Seeliger credit for the modification of Newton's law, according to which "the force of attraction between two masses diminishes more rapidly than would result from the inverse square law" (1918…). In fact, C. Neumann had reached similar conclusions (see Seeliger 1895 and Neumann 1896). Einstein went on to emphasize that neither a theoretical principle nor an observation would ever justify the proposed modification; any other convenient law would do the same job.… Seeliger's modification of the inverse square law was $F = Gmm'e^{-\Lambda r}/r^2$. (Kerszberg 1989, 161–162)

Unfortunately this expression of Seeliger's (one of a number that he used) was not the most plausible option even in the 1890s. Neumann's potential had the virtue of solving a known linear differential equation, unlike most of Seeliger's expressions. The enhanced plausibility of Neumann's mathematics with the rise of the concept of graviton mass overcomes the objections to Seeliger. Kerszberg commendably finds Λ difficult to interpret.

He comments more explicitly on Einstein's analogy between his modified Poisson equation and his cosmological constant Λ:

> Beyond any surface similarity, there is indeed a fundamental contrast, because the Λ-term now fixes the periphery and removes all reference to the centre. Thus, the new constant is parallel to Newton insofar as the *form* of the laws is concerned, but the *interpretation* of it diverges sharply from formal analogy. (Kerszberg 1989, 163)

But in fact scalar (spin 0) and symmetric tensor (spin 2) tensor theories of gravity are too similar to permit this disconnect between formal analogy and interpretation (Freund et al. 1969; Boulware and Deser 1972). In fact there is no formal analogy, either; Kerszberg's sense of an important difference is correct. Once one has the proper alignment of scalar/spin 0 and tensor/spin 2 analogs, the behavior of the corresponding cases (scalar with graviton mass, tensor with graviton mass; scalar with cosmological constant, tensor with cosmological constant) is quite similar, both empirically and conceptually. In both the scalar and tensor cases, the massive graviton case involves a background geometry and a smaller symmetry group (typically the Poincaré group of special relativity), whereas the cosmological constant involves no background geometry and does not shrink the symmetry group.

5.4 John Norton and John Earman: Clarity But No Graviton Mass

Some philosophically flavored history out of Pittsburgh has fared noticeably better. At the end of the 1990s, John Norton very helpfully discussed "The Cosmological Woes of Newtonian Gravitational Theory," providing a useful discussion of Seeliger, Neumann, and Neumann's elusive priority claim. Norton, citing Trautman, is not taken in by Einstein's claimed analogy between the modification of Newtonian gravity and the cosmological constant:

> (The analogy is less than perfect—something Einstein may have found it expedient to overlook in order not to compromise his introduction of the cosmological term. As Trautman (1965: 230) pointed out, Einstein's augmented gravitational field equation reduces in Newtonian limit to a field equation other than [the modified Newtonian equation $\nabla^2\phi - \lambda\phi = 4\pi G\rho$]: $\nabla^2\phi + \lambda c^2 = 4\pi G\rho$.) (Norton 1999, 299)

Norton does not, however, consider how developments in the 1920s–1930s in particle physics provided a physical meaning for Neumann's mathematics and thus made a specific and plausible form of Seeliger's ideas more available. Admittedly, changing gravitation on long ranges becomes far less urgent once one finds that the universe is expanding.

John Earman's paper on Λ (2001), which draws upon Norton's, is also clear and accurate technically—indeed clearer than others that say little about the perturbative business involved—as well as historically aware. He does not, however, connect the Neumann-Seeliger work with the later physical meaning of a graviton mass.

5.5 Helge Kragh: Rewriting History

More recently, in an otherwise admirable book *Matter and Spirit in the Universe: Scientific and Religious Preludes to Modern Cosmology*, Helge Kragh took Einstein's false analogy between Λ and the Seeliger-Neumann sort of modification of gravity so seriously as to rewrite history in light of this analogy, at variance with what Seeliger actually proposed (Kragh 2004, 28). Kragh seems to have used both Einstein's false analogy and the 1918–1919 Kottler-Weyl (a.k.a. "Schwarzschild-de Sitter") *solution* of Einstein's equations with Λ discussed above to infer the mathematical potential comparable to $\frac{1}{r}$ that Seeliger supposedly posited in the 1890s. This error seems not to appear in Kragh's sources (Norton 1999; Harrison 1986; Jaki 1979). Kragh writes:

> Seeliger's suggestion was to replace the [Newtonian gravitational potential $\varphi(r) = -M/r$]…with
>
> $$\varphi(r) = -\frac{M}{r} - \frac{\Lambda r^2}{6}$$
>
> where Λ is a cosmological constant so small that its effects will be unnoticeable except for exceedingly large distances. The body not only experiences an attractive inverse-square force towards the central body, but also a repulsive force given by $\Lambda r/3$. In a somewhat different way the slight but significant adjustment of the inverse square law was suggested also by Carl Neumann in 1896, and it reappeared in a very different context in 1917, now as Einstein's famous cosmological constant. (Kragh 2004, 28)

Seeliger did experiment with various ways to modify the Newtonian potential (von Seeliger 1895, 1896, 1898) and did not seem captivated by Neumann's specific proposal to include just an exponentially decaying factor in the potential (a modification later interpretable as a graviton mass and hence the 'right' way to do it). But adding a divergent quadratic potential was not among the things that Seeliger considered.

5.6 Marco Mamone Capria: Clarity but No Graviton Mass

The faulty analogy has been treated better in the work of Marco Mamone Capria (2005), partly with influence from Schucking. Regarding Einstein's proposed modification of Newtonian gravity, it:

> was probably inspired by the modified gravitational potential ϕ_N built out of the mass-point potential $Ae^{-r\sqrt{\lambda}}/r$ (A is a constant), as proposed by the German theoretical physicist Carl Neumann in 1896. Neumann introduced this form of the potential in order to solve the gravitational paradox in the form of the impossibility to assign, at any point, a finite value to the gravitational potential corresponding to a uniform infinite mass distribution. In fact ϕ_N goes to zero at infinity even with such a mass distribution, and (2) [the modified Poisson equation] is precisely the equation which it satisfies. (Mamone Capria 2005, 131)

Seeliger's motivation seems to be attributed to Neumann, however. Mamone Capria explicitly rejects Einstein's analogy (Mamone Capria 2005, 135), following Harvey and Schucking (2000).

> Even more remarkable [than Einstein's false analogy, the quarter of a century before it was criticized by Heckmann, and the ease with which one refutes the analogy], perhaps, is that a long sequence of eminent authors missed this basic point and blindly endorsed Einstein's stated analogy between (2) and (7) ("generations of physicists have parroted this nonsense" as is said in [Harvey and Schucking 2000, 723]). Clearly most working scientists are just too anxious to publish some 'new' piece of research of theirs to spend a sufficient amount of time reviewing the foundations of their disciplines; so they frequently end up by relying on authority much more than on rational belief, in contrast with the scientific ethos as ordinarily proclaimed. (Mamone Capria 2005, 135)

Mamone Capria does not go on to draw the further conclusion that the actual analog of the modified Poisson equation is thus a potentially interesting and unexplored possibility for relativistic cosmology or recognize that particle physics is the missing ingredient, but still this is progress.

5.7 O'Raifeartaigh, O'Keeffe, Nahm and Mitton: Clarity But No Graviton Mass

A recent centennial review of Einstein's 1917 cosmological paper also makes a clear distinction between the Neumann-Seeliger enhanced long-range decay and Einstein's cosmological constant:

> It is an intriguing but little-known fact that, despite his claim to the contrary, Einstein's modification of the field equations in Section §4 of his memoir was not in fact "perfectly analogous" to his modification of Newtonian gravity in Section §1. As later pointed out by several analysts [footnote suppressed], the modified field equations (E13a) do not reduce in the Newtonian limit to the modified Poisson's equation (E2), but to a different relation given by
>
> $$\nabla^2\phi + c^2\lambda = 4\pi G\rho. \tag{10}$$
>
> This might seem a rather pedantic point, given that the general theory allowed the introduction of the cosmological constant term, irrespective of comparisons with Newtonian cosmology. Indeed, as noted in Section 4.1, Einstein described his modification of Newtonian cosmology merely as a "*foil for what is to follow*". However, the error may be significant with regard to Einstein's interpretation of the term. Where he intended to introduce a term to the field equations representing an attenuation of the gravitational interaction at large distances, he in fact introduced a term representing a very different effect. Indeed, the later interpretation of the cosmological term as representing a tendency for empty space to expand would have been deeply problematic for Einstein in 1917, given his understanding of Mach's Principle at the time. Thus, while there is no question that relativity allowed the introduction of the cosmic constant term, it appears that Einstein's interpretation of the term may have been to some extent founded on a misconception (Harvey and Schucking 2000, 223). (O'Raifeartaigh et al. 2017, 28)

This is a very appropriate assessment, albeit without seeing that the Neumann-Seeliger idea provided the mathematical nucleus of a potentially interesting case of unconceived alternatives in the form of a graviton mass.

6 Conclusion

It seems that there is an irregular but gradual upward trend in recognition by historians and philosophers of physics of the distinction between faster long-range decay and a cosmological constant, notwithstanding Einstein's claimed analogy. Thus it is certainly possible to get the correct answer without much personal awareness of particle physics. However, it is also quite easy to fall for the false analogy. Much depends on what one has read. Works that clarify the issue are not standard reading material for historians or philosophers of physics.

But there are a number of imperfections even in a situation in which historians and philosophers recognize the erroneous quality of Einstein's analogy. First, it is unclear that one can make sense of the difference between a cosmological constant and a graviton mass if one is averse to particle physics; one cannot make sense of a graviton mass without a background geometry and perturbative-looking comparison of two geometries. Second, it appears that the increased recognition of the distinction is based largely on authority, oftentimes that of Schücking, making the recognition highly contingent on reading the right materials. Third, if one does manage to avoid the false analogy, one can easily still fail to recognize that one has encountered a potentially interesting example of the problem of unconceived alternatives. A dose of particle physics egalitarianism and corresponding de-privileging of General Relativity exceptionalism would help on all counts.

This discussion is clearly not intended to imply that anyone should embrace a graviton mass or should not embrace a cosmological constant. Ultimately the world should settle that issue through observational data. At present at least an effective cosmological constant has empirical support, whereas the graviton mass does not clearly have any empirical support (although it is difficult to say what to make of the dark matter problem, which might be soluble in terms of modified gravity in some form). But failing even to conceive of a graviton mass, which is a plausible option from the standpoint of particle physics, while taking with great seriousness a cosmological constant even prior to any data, when it is at least *prima facie* implausible from the standpoint of particle physics (prior to *quantum* field calculations), is hardly a balanced view. One needs to adjust one's prior plausibilities in order to profit more from the data.

The briefly mentioned examples of gravitational radiation and particle physicists' spin 2 derivations of Einstein's equations also illustrate how influence from particle physics can be salutary for understanding and motivating Einstein's equations. All three examples provide support for the claim that systematic integration of general relativist and particle physics ideas holds considerable promise for historical and philosophical reflection on gravitation and space-time.

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Toward a Quantum Theory of Gravity: Syracuse 1949–1962

Donald Salisbury

1 Introduction

Peter G. Bergmann served as a research assistant to Albert Einstein at the Institute for Advanced Study in Princeton from 1936 to 1941, immediately after receiving his doctoral degree at Charles University in Prague. While still working on his dissertation in which he dealt with the quantum description of a harmonic oscillator in a curved space, he wrote to Einstein that "As you can see, my training in any direction is in no way to be understood as complete. It is clear to me that I have much to learn in the field of relativity theory as well as quantum mechanics. If it were possible I would gladly continue to work in the search for a link between these two fields."[1] His work with Einstein was mainly concerned with unified field theory, but he continued to think about this problem. Having secured a tenure-track position at Syracuse University in 1947, following war-related research from 1944 to 1947, he was finally able to focus on the problem that would occupy him for the remainder of his career. His first groundbreaking paper dealt with a generalized approach to non-linear field theories like general relativity, with an

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[1] "Wie Sie ersehen können, ist meine Ausbildung noch keineswegs in irgendeiner Richtung als abgeschlossen zu betrachten. Ich bin mir darüber klar, dass ich besonders auf dem Gebiet der Relativitätstheorie einerseits, der Quantentheorie anderseits, noch sehr viel zu lernen habe. Wenn es mir möglich wäre, würde ich sehr gern in der Richtung weiterarbeiten, die Verbindung zwischen diesen beiden Gebieten zu suchen." The Albert Einstein Archives at the Hebrew University of Jerusalem (AEA), 6–222, Letter from Bergmann to Einstein dated March 14, 1936.

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eye toward quantization (Bergmann 1949). Einstein at the time did not seem to endorse this enterprise. In response to a note from Bergmann[2] expressing a hope that they might get together to discuss these ideas, he wrote "You are seeking an independent new path toward a solution of the fundamental problems. No one can help in this effort, and least of all someone who to some extent has his own fixed ideas. You know, for example, that on the basis of certain considerations I firmly believe that the probability concept can not play a primary role in the description of reality. You seem to believe that one should first set up a field theory and then subsequently 'quantize' it ... Your attempt to carry out an abstract field theory without knowing in advance the formal nature of the field quantities seems to me unfortunate, being formally deficient and indeterminate."[3] Nevertheless, Einstein did in 1954 recommend approval of Bergmann's application for National Science Foundation funding of his project entitled Quantum Theory of Gravitation with the words "All physicists are convinced of the high truth value of the probabilistic quantum theory and of the general relativity theory ... There are presently only few theoretical physicists who have penetrated deeply enough into both theories to be able to undertake such an attempt at all. Dr. Bergmann is one of the few who are completely at home in both theories."[4] Einstein was, however, still not willing to participate in this effort according to Joshua Goldberg. Following Goldberg's brief meeting with Einstein in 1954, Bergmann contacted Einstein to inquire whether the three of them might collaborate. Einstein declined. See Salisbury (2020a).

I will focus in this essay on the work of Bergmann and his collaborators from 1949 to 1962, where appropriate comparing and contrasting with contemporary work in the "opposing" camps. As far as possible, I will try to structure this essay in the chronological order in which problems were addressed by the Syracuse associates. The plan was to eventually develop a Hamiltonian formalism and then to undertake a canonical quantization. The fundamental challenge was to properly take into account the symmetry of Einstein's theory under general coordinate transformations. The profound consequence from Bergmann's perspective was that spacetime coordinates were themselves devoid of physical content. He firmly believed that the ultimate quantum theory must deal exclusively with operators that were invariant under the action of this symmetry group. This view is in sharp contrast to the geometrodynamic principles that were proposed by Wheeler

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[2] AEA, 6–282, Letter from Bergmann to Einstein dated January 24, 1949.

[3] "Sie suchen einen selbständigen und neuen Weg zur Lösung der prizipiellen Schwierigkeiten. Bei diesem Bestreben kann einem Niemand helfen, am wenigsten Einer, der einigermassen fixierte Ideen hat. Sie wissen z. B., dass ich auf Grund gewisser Ueberlegungen fest glaube, dass der Wahrscheinlichkeitsbegriff nicht primär in die Realitätsbeschreibung eingehen darf, während Sie daran zu glauben scheinen, dass man zuerst eine Feldtheorie aufzustellen und diese dann nachträglich zu 'quantisieren' hat ... Ihr Versuch, eine Feldtheorie abstrakt durchzuführen, ohne von vornherein über die formale Natur der Feldgrössen zu verfügen, erscheint mir nicht glücklich, weil formal zu arm und unbestimmt." AEA, 6–282, Letter from Einstein to Bergmann dated January 26, 1949.

[4] AEA, 6313, Recommendation dated April 18, 1954.

at roughly the time at which my narrative terminates. Bergmann may well have contributed to the dominance of the 3+1 formalism with his interpretation of Dirac's 1958 variables, published in his 1962 overview of general relativity.

In Sect. 2, after addressing the first foray by Bergmann (B49) (Bergmann 1949) and Bergmann Brunings (BB49) (Bergmann and Brunings 1949) that constituted the theoretical basis of much that was to follow, I turn in 1950 to Bergmann and his students Penfield, Schiller, and Zatzkis (BPSZ50) (Bergmann et al. 1950) who constructed a Hamiltonian for Einstein-Maxwell theory in a formalism in which the spacetime coordinates evolved in an arbitrary parameter time. After the discovery by Penfield (P50) (Penfield 1950) that the parameterized formalism could be abandoned and that four of the Einstein equations arose from the demand that primary constraints be preserved in time, Anderson and Bergmann (AB51) in 1951 (Anderson and Bergmann 1951) derived secondary constraints in a generic diffeomorphism covariant theory and wrote down the phase space generator of general coordinate transformations. These studies were undertaken before the Syracuse group became aware of the earlier pioneering work of Léon Rosenfeld (R30) (Rosenfeld 1930), and they became aware of Dirac's new approach to constrained dynamics only when BPSZ50 was already a preprint. Pirani and Schild had in the meantime effectively simultaneously derived their own Hamiltonian employing Dirac's method, also in the parameterized version. I will show in Schiller's thesis (S52) (Schiller 1952) that Rosenfeld had strongly influenced him.

In Sect. 3, after a brief look at Anderson's 1952 thesis speculation regarding a Schwinger-type Lagrangian action operator, I consider the full-scale effort in 1953 by Bergmann and Schiller (BS53) (Bergmann and Schiller 1953) to formulate a Lagrangian quantum action principle, attempting to extend the Schwinger action principle to Einstein's generally covariant theory. This same paper also looks closely at not only the Poisson bracket generators of canonical transformations that correspond to infinitesimal coordinate transformations (invariant transformations) but also transformations between physically distinct solutions of Einstein's equations, an analysis I take up in Sect. 4, where I also consider the related 1955 work by Bergmann and I. Goldberg (BG55) (Bergmann and Goldberg 1955b). They undertook a closer examination of the classical factor group of canonical transformations modulo invariant transformations, leading to the notion of a reduced phase space whose elements are constants of the motion. The algebra of these observables was realized by what I call "Bergmann-Goldberg brackets" – modestly termed "extended Dirac brackets" by the authors. With the recognition that the determination of these brackets required the construction of diffeomorphism invariants (constants both temporally and spatially), I consider in Sect. 5 efforts to construct diffeomorphism invariants. Bergmann's student Newman proposed an iterative construction. Alternatively, Janis investigated the possibility of obtaining the desired algebra of invariants through the imposition of coordinate conditions, simplified through the addition of a divergence to the Einstein Lagrangian that trivialized the primary constraints. Dirac somewhat later in 1959 (D59) (Dirac 1959) proposed his own general relativistic coordinate conditions, with Bergmann's enthusiastic support. Roughly simultaneously Komar proposed the use of intrinsic coordinates to construct the elements of this physical phase space (K58) (Komar 1958).

In Sect. 6 I discuss Dirac's 1958 gravitational Hamiltonian (D58) (Dirac 1958b) and Bergmann's 1962 (B62) (Bergmann 1962) interpretation. This reformulation of general relativistic Hamiltonian dynamics strongly influenced Bergmann's own treatment of general covariance. Most significantly, he showed that Dirac's new variables implied that infinitesimal diffeomorphisms must be understood as incorporating a compulsory metric field dependence – that which is now taken as a conventional decomposition in the direction normal to the spacelike temporal foliations of spacetime. This decomposition later became a foundational principle in Wheeler's geometrodynamics program – but with a conscious abandonment of the full four-dimensional symmetry that had been pursued by the Syracuse group. I will argue that although the Wheeler program has until recently dominated the efforts at quantization, the Bergmann program has itself undergone a continuous development since the supposed pre-Renaissance era and is in my opinion now poised to reassert what Pitts in this volume has called its strong general relativity exceptionalism (see Chap. 6 in this volume). Significantly, the lapse and shift were abandoned as phase space variables. The link to the underlying four-dimensional diffeomorphism symmetry was thus obscured and perhaps even temporally lost. Such was the case in the equivalent parallel work of Arnowitt, Deser, and Misner, culminating in their 1962 review (Arnowitt et al. 1962a). Bergmann carefully laid out the basis of this apparent loss in his 1962 interpretation of Dirac's classical general relativistic Hamiltonian analysis (Bergmann 1962).

2 Initial Consequences of a Generally Covariant Lagrangian Action

Given that a Lagrangian density transforms under an arbitrary infinitesimal spacetime coordinate transformation $x'^{\mu} = x^{\mu} + \xi^{\mu}(x)$ as a density of weight one plus a possible total divergence, Bergmann showed in 1949 (Bergmann 1949) that the Legendre matrix must be singular and that consequently there exist as many (primary) constraining relations among the fields and conjugate momenta as there are independent null vectors of this matrix. Bergmann and Brunings (1949) then applied this logic to a generic reparameterization covariant theory with field variables $y_A(x)$ and with spacetime coordinates x^{μ} themselves functions of four parameters $u^{\alpha} := (t, u^s)$. They assumed a Lagrangian density $\mathfrak{L}$ that depended only on field variables y_A and first derivatives with respect to the spacetime coordinates, $y_{A,\mu} := \frac{\partial y_A}{\partial x^{\mu}}$. BPSZ50 (Bergmann et al. 1950) later assumed that the Lagrangian was quadratic in the $y_{A,\mu}$, and of the form

$$\mathfrak{L} = \Lambda^{A\rho B\sigma}(y) y_{A,\mu} y_{B,\nu}, \tag{1}$$

then the transformed Lagrangian with variables $x^{\mu}(u)$ is no longer quadratic in time derivatives $y_{A,0}$ since $y_{A,\mu} = \frac{\partial u^{\alpha}}{\partial x^{\mu}} \frac{\partial y_A}{\partial u^{\alpha}} = J^{-1} M^{\alpha}{}_{\mu} \frac{\partial y_A}{\partial u^{\alpha}}$, where the $M^{\alpha}{}_{\mu}$ are minors of the matrix $\frac{\partial x^{\nu}}{\partial u^{\beta}}$[5] and J is the determinant of this matrix. Both J and $M^{\alpha}{}_{\mu}$ are functions of $\dot{x}^{\mu} := \frac{\partial x^{\mu}}{\partial t}$. It does turn out, however, that the resulting Lagrangian is homogeneous of degree one in the velocities $\dot{y}_A$ and $\dot{x}^{\mu}$. They simplified the notation by representing the variables $y_a(t, u^s)$ and $x^{\mu}(t, u^s)$ collectively as y_a, with corresponding conjugate momenta π^a. Then the Hamiltonian density defined in the usual manner as $\mathfrak{H} := \pi^a \dot{y}_a - \mathfrak{L} \equiv 0$ vanished identically when viewed as a functional of the fields and velocities. Seven of the primary constraints could easily be found. The challenge was to find the eighth which they showed needed to be employed as the Hamiltonian density (with the option of adding arbitrary linear combinations of the remaining seven). They were able to demonstrate that the original Lagrangian dynamics followed from the resulting Hamiltonian field equations.

The expressed rationale for the introduction of the parameter formalism was that in this way if would be possible to incorporate singular particle sources in the theory. The parameterization procedure was apparently inspired by the work of Paul Weiss who had employed it in work cited by Bergmann and Brunings (Weiss 1936, 1938a,b).[6] It is possible that at least initially Bergmann thought it conceivable that just as the classical field theory automatically determined particle motions via the Einstein-Infeld-Hoffmann (EIH) procedure, the quantized field might automatically induce particle quantum wave mechanics.[7] He did write that “it is possible, in the general theory of relativity, to treat the motion of field singularities (which are used to represent particles) without having to deal with infinite interaction terms of one kind or another. It is possible that this accomplishment will lead to a more satisfactory quantized theory …”[8]

The task of constructing the explicit Hamiltonian was taken up in BPSZ50 (Bergmann et al. 1950). Since the Legendre matrix $G^{ab} := \frac{\partial^2 \mathfrak{L}}{\partial \dot{y}_a \partial \dot{y}_b}$ is singular, it is not possible to solve the defining equation $\pi^a = G^{ab} \dot{y}_b + f^a[y]$ for the velocities in terms of the fields y_a and momenta π^b. BPSZ50 developed a scheme for solving for the $\dot{y}_b$ which involved the use of a matrix E_{ab} that they called a “quasi-inverse” of G^{ab} which satisfied the relations $G^{ab} E_{bc} G^{cd} = G^{ad}$ and

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[5] Explicitly, $M^{\alpha}{}_{\mu} = \frac{1}{3!} \epsilon^{\alpha\beta\gamma\delta} \epsilon_{\mu\nu\rho\sigma} \frac{\partial x^{\nu}}{\partial u^{\beta}} \frac{\partial x^{\rho}}{\partial u^{\gamma}} \frac{\partial x^{\sigma}}{\partial u^{\delta}}$.

[6] Goldberg has expressed to me the opinion that the parameterization idea was inspired by Weiss. Weiss had introduced a parameterized version of electromagnetism in flat spacetime, but he did not conceive of the $x^{\mu}(u)$ as dynamical variables. See Rickles and Blum (2015) for more background and also Salisbury (2020b) for Weiss’ influence on Hamilton-Jacobi techniques in general relativity.

[7] See Blum and Salisbury (2018). See also Salisbury (2019a, 2020a) for a discussion of the Syracuse school’s work on classical equations of motion.

[8] Bergmann (1949, 680).

$E_{ab}G^{bc}E_{cd} = E_{ad}$.[9] They did not cite a reference for this procedure.[10] The bulk of this paper dealt with the determination of E_{ab}. Since the task was greatly simplified when the parameterized formalism was abandoned, I will wait to describe this simplified procedure. Most relevant for the moment is the fact that the resulting vanishing Hamiltonian density was quadratic in the momenta, taking the form $\mathfrak{H} = \frac{1}{4}\pi^a E_{ab}\pi^b = 0$. Taking as the gravitational Lagrangian Einstein's first-order form

$$\mathfrak{L}_E := \sqrt{-g}g^{\mu\nu}\left(\Gamma^{\sigma}_{\rho\sigma}\Gamma^{\rho}_{\mu\nu} - \Gamma^{\sigma}_{\rho\mu}\Gamma^{\rho}_{\sigma\nu}\right), \tag{2}$$

they found an explicit expression of the Hamiltonian that generated the correct first-order equations of motion for all of the phase space variables. As expected, it contained eight arbitrary spacetime functions, four of them corresponding to the original diffeomorphism freedom and four to the reparameterization freedom. The first four were functions that appeared in the solution for E_{ab}. The fifth amounted to an arbitrary choice for function $t(x)$, and the three remaining functions could be chosen to multiply the constraints that generated reparameterizations in u^s. Still in this work the authors stated their intention that “the new formalism will be used to give a new derivation of the equations of motion.”[11]

At this juncture we make a quick detour to the Hamiltonian derivation of Pirani and Schild (Pirani and Schild 1950). They had listened to Dirac's Vancouver lectures on constrained Hamiltonian dynamics in August of 1949. A letter from Bergmann to Schild, dated February 12, 1949, indicates that Schild had already read Bergmann's 1949 paper.[12] It was natural that it would then have occurred to him that Dirac's procedure could be applied to general relativity, and he suggested this as a thesis topic to Pirani. Even though they continued to work with the Bergmann Brunings parameterized model, the technique they employed turned out to be substantially less involved than BPSZ50. Dirac's procedure involved the independent variation

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[9] Equations (3.1) in BPSZ50 have incorrect right-hand sides. The typos were corrected in Anderson and Bergmann (1951), equation (4.3).

[10] The matrix E_{ab} is known as the Moore-Penrose pseudoinverse (Rao and Mitra 1971), first described by Moore in 1920 (Moore 1920) and then rediscovered by Penrose in 1955 (Penrose 1955). Penrose has communicated to me that he was unaware of its use in BPSZ50.

[11] Bergmann et al. (1950, 88). Curiously, even though Joshua Goldberg had just begun his own thesis work (Goldberg 1952) under Bergmann's direction at this time, and it was devoted both to the covariance foundations of the EIH approach and the Hamiltonian form of general relativity, he has communicated to me that he never shared this view.

[12] “I am sorry that we did not get together in New York … In the meantime, I have received the reprints you were kind enough to send me. I shall reciprocate in kind and send you one I have here at the moment and regularly send you my output … We are having a project at Syracuse in which we are attempting to investigate the general properties of covariant field theories (whether or not they be impressed on a Riemannian geometry), with the special purpose of learning how to quantize covariant theories. If you should have any papers relating to this general subject, I should certainly appreciate knowing about them.” Alfred Schild Papers at the University of Texas at Austin (ASP), Box 86-27/2.

of y_A, π^B, and $\dot{y}_A$, leading him to distinguish a "strong" equality which remained valid under these variations, assuming one begins on the constraint hypersurface in phase space, and a "weak" equality which is not preserved under these variations. He could prove that with constraints $\phi_a = 0$, the Hamiltonian could be written as a strong equality, $H \equiv \beta_a \phi_a$, where the β_a are arbitrary functions of the three sets of variables. Some ingenuity was still required to construct the Hamiltonian constraint which was assumed to be of the form $H = \dot{x}^\gamma \left(\Phi_\gamma + c_{\gamma\sigma}\phi^\sigma\right)$ where the ϕ^σ were already known constraints. The problem was to find the constraints Φ_γ. This feat was accomplished through the adroit addition of a strongly vanishing product of known constraints. Goldberg insists that the news in Syracuse of the Pirani Schild result was a shock and the Dirac analysis was still unknown when the preprint arrived.[13] The resulting exchange was nevertheless civil. Bergmann acknowledged receipt of the preprint in a letter to Schild dated January 15, 1950:[14] "I got your letter, plus preprints, in Syracuse last week, in the midst of reading term papers. As a result, I have not been able to compare your formula with ours to see whether they agree. This is, however, trivial, because disagreement would mean an arithmetical error on our part or yours, and that is not very likely. The formulas will look quite different, and that is why comparison will be somewhat slow." He continued "From the external appearance of your constraint, I suspect that you get yours by methods quite different from ours, and thus, I think we ought to publish separately, calling attention to the other fellow's paper. In particular, we are distributing a preprint of our paper as an ONR Technical Report (as soon as it is off the press, you will get your copy). I would, therefore, suggest that you people in Pittsburgh go ahead with your plans for publication, and we do with ours…From the point of view of further development, I consider it extremely important that we should exchange complete information on the technique employed, because the obvious next stage is to construct improved covariant theories, and to do so requires use of the tricks learned. Likewise, we should know what we are planning, so that we shall not duplicate our work unnecessarily. So I am looking forward to spending a day with you in New York. Give me a ring as soon as you hit town." The reference is to the APS meeting in New York, February 2–4, 1950, in which either Schild or Pirani reported on their work.

Bergmann had already by this time had some misgivings about the BPSZ50 results, as reported by Scheidegger to Schild on January 20, 1950.[15] Scheidegger writes "I had a chance to talk to Bergmann; he is still worried about the quantization of the gravitational field; he thinks that his previous results were wrong since there are some 8 more constraints which had been overlooked heretofore." Indeed, as I have noted previously (Salisbury 2012), Bergmann and Brunings had stated without supporting evidence that there were no constraints beyond the eight primary ones. A letter from Bergmann to Schild dated November 16, 1950, confirms his realization

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[13] Private communication.

[14] Syracuse University Bergmann Archive, Syracuse, NY, USA (SUBA), Correspondence folder.

[15] ASP, Box 86-27/2. See also Blum and Salisbury (2018).

that more constraints are present: “In the meantime we have continued our work and found that additional constraints must be satisfied if the equations of the Hamiltonian formalism are to be equivalent to those of the Lagrangian formalism. At the present we are working on a proof that the number of these additional constraints equals the number of primary constraints. A serious quantization procedure must take all the constraints into account ab initio. As soon as we know, we shall write up and send you a copy of the MS. What are you doing these days?”[16] Schild responds on February 5, 1951: “Felix and I are very interested in your results on additional constraints. At the moment I do not quite understand how they come in. In Dirac’s language, these constraints mean that the identities obtained earlier by you (and presumably the same as those obtained by us) are second-class ϕ – equations. We had an argument that seemed convincing at the time, that the Poisson brackets between identities for the gravitational Lagrangian ought to vanish. Your results seem to contradict this (Do you get new constraints in the specific case of the gravitational field without singularities?) We would be very interested to see your manuscript when you have the mimeographed form. We are looking into the matter too and will let you know as soon as we can confirm your results.”[17] According to a note from Schild to Bergmann dated January 23, 1952, Schild’s student R. Skinner began working on this problem in June, 1951, as a component of his dissertation.[18] The results were published jointly with Pirani and Schild (Pirani et al. 1952). By this time they had abandoned the parameterized formalism, as had AB51 in 1951 (Anderson and Bergmann 1951).

2.1 *The Rosenfeld, Anderson, Bergmann, Penfield, Schiller Nexus*

Here the story becomes confused. Goldberg maintains that Bergmann had always known that there were additional constraints. Yet he attributes the first derivation of secondary constraints in general relativity to Penfield, following Penfield’s discovery that the parameterized formalism could be abandoned.[19] Penfield showed that the four vacuum Einstein equations $G_{\mu\nu} = 0$ followed as a consequence of the preservation in time of the primary constraints (Penfield 1950). Goldberg went on to show in his thesis that these secondary constraints were preserved as a consequence of the Bianchi identities (Goldberg 1952). Also, Penfield was listed as a co-author in the abstract of the APS talk that announced the results of the AB51 paper that has until recently been cited as the first work in which the secondary constraints of Einstein’s theory were systematically derived. The tale is

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[16] ASP, Box 86-27/2.

[17] SUBA, Correspondence folder.

[18] SUBA, Correspondence folder.

[19] Private communication, and cf. also Salisbury (2020a).

substantially further complicated through the discovery by the Syracuse group in 1950 of Rosenfeld's pioneering systematic analysis of the constrained Hamiltonian dynamics of Einstein's theory (Rosenfeld 1930, 2017). Anderson informed me in 2007 that it was he who had made this discovery and brought it to Bergmann's attention. In the same conversation, Schiller told me that this Rosenfeld paper inspired his own doctoral thesis (Schiller 1952), and he cited Rosenfeld both in the original thesis and in this article which was received by *The Physical Review* in May, 1951. They write that their work "is in some respects similar to results obtained by Rosenfeld." This is of particular relevance since the same methods that Rosenfeld had developed for a generally covariant Lagrangian theory were directly applicable to the quadratic model with the Einstein Lagrangian (2) in its unparameterized form, as analyzed by Anderson and Bergmann (1951). Furthermore, Rosenfeld had already in this groundbreaking article not only developed an algorithm for determining all higher-order constraints but also showed how to construct the phase space generator of general infinitesimal coordinate transformations.[20] Penfield's thesis, which he defended in July, 1950, contains no reference to Rosenfeld. The published version (Penfield 1951) is almost identical, except for the inclusion of this reference, and this – in addition to Penfield's full-time teaching duties at Harpur College – may account for the 1 year delay in its submission to *The Physical Review*. Bergmann's first public recognition of Rosenfeld's work apparently occurred at the August 1950 International Congress of Mathematicians (Bergmann 1952).

The basis of all of the cited works is the fundamental identity that follows from the assumption that the variation under the symmetry group of the Lagrangian density is a total divergence. Although all of the authors considered more general symmetry groups, I will confine my attention here to general covariance under infinitesimal coordinate transformations $x'^\mu = x^\mu + \xi^\mu(x)$, in which case all assumed that the fields y_A transformed as

$$\bar{\delta} y_A = F_{A\mu}{}^{B\nu} y_B \xi^\mu_{,\nu} - y_{A,\mu} \xi^\mu, \tag{3}$$

where the $F_{A\mu}{}^{\nu\sigma}$ are constants. Furthermore, it was assumed that the term $S^\mu_{,\mu}$ that was added to the Lagrangian was linear in the second derivatives, so that $S^\mu = f^{A\mu\rho}(y) y_{A,\rho}$. In this case the sum of the Lagrangian density and a total divergence must transform as a scalar density of weight one, namely,[21]

$$\bar{\delta}\mathfrak{L} + \left(\bar{\delta} S^\mu\right)_{,\mu} \equiv \left(\left(\mathfrak{L} + S^\nu_{,\nu}\right) \xi^\mu\right)_{,\mu} . \tag{4}$$

This is true in the passage from $\sqrt{-g}R$ to the Einstein Lagrangian, as Bergmann had spelled out in detail in his textbook (Bergmann 1942). Although Rosenfeld did not employ the Einstein Lagrangian, it was quadratic in the first derivatives of the

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[20] See Salisbury and Sundermeyer (2017) for a detailed analysis of this paper.

[21] This is a direct expression of the transformation property of a scalar density $\mathfrak{S}\left(y(x)\right)$ of weight one under a coordinate transformation $x'^\mu(x)$, $\mathfrak{S}'\left(y'(x')\right) = \mathfrak{S}\left(y(x)\right)\left|\frac{\partial x}{\partial x'}\right|$.

field variables. The difference was that his purely gravitational contribution was constructed using tetrad fields, and as such it transformed as a scalar density under general coordinate transformations, whereas the Einstein Lagrangian did not. On the other hand, under local Lorentz transformations, its variation involved a total divergence, and Rosenfeld showed how to construct the correct Hamiltonian with this additional local gauge symmetry.

Since (4) is an identity, a wealth of information can be derived from it. It must be true that the coefficients of each derivative of ξ^μ must vanish. The origins of this procedure can be traced all the way back to Felix Klein (Klein 1918).[22] This is the method utilized by Rosenfeld. Bergmann, on the other hand, undertook an integration over all space, taking the $\xi^\mu \to 0$ on the boundary. In this manner he derived what he called "the generalized contracted Bianchi identities,"

$$F_{A\mu}{}^{B\rho}\left(L^A y_B\right)_{,\rho} + L^A y_{A,\mu} \equiv 0, \tag{5}$$

where $L^A := \frac{\partial \mathfrak{L}}{\partial y_A} - \left(\frac{\partial \mathfrak{L}}{\partial y_{A,\mu}}\right)_{,\mu}$. It follows that the coefficient of $y_{A,\mu\nu\rho}$ must vanish identically, leading to the conclusion that $F_{A\mu}{}^{B0} y_B \Lambda^{AC} \equiv 0$. Using Bergmann's notation I let (1) represent the Einstein Lagrangian (2). Then it follows from (5) that $u_{\mu A} := F_{A\mu}{}^{B0} y_B$ is a null vector of $\Lambda^{AC} := \Lambda^{A0B0}$,[23]

$$u_{\mu A}\Lambda^{AC} \equiv 0. \tag{6}$$

Then since

$$\pi^A = 2\Lambda^{A0B0}\dot{y}_B + 2\Lambda^{A0Ba} y_{B,a} \tag{7}$$

it follows that (7) does not possess unique solutions for $\dot{y}_A$. Rosenfeld's procedure for obtaining the general solution was to first find those linear combinations of $\dot{y}_A$ which could be set equal to zero, consistently with (7). He let $\dot{y}^0_A$ represent this solution set, including the non-vanishing $\dot{y}_A$. For his Lagrangian choices, there was a simple procedure for finding this special solution. Letting N represent the number of fields, he merely needed as a first step to isolate $N-4$ independent rows of the matrix Λ. Then he could quickly find the four null vectors u_{sA}, where $s = 1, \ldots, 4$. Furthermore, $N-4$ of the equations of (7) could be solved for $N-4$ momenta, and the remaining momenta could be expressed in terms of them. And finally, the general solution of (7) is $\dot{y}_A = \dot{y}^0_A + \lambda^s u_{sA}$ where the $\lambda^s(x)$ are arbitrary spacetime functions.[24] When this solution is substituted into the Lagrangian, the null vectors do not contribute so that the Lagrangian then becomes a function of y_A

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[22] See Salisbury and Sundermeyer (2017).

[23] This two-index object should not be confused with the parameters introduced in BPSZ50.

[24] See Salisbury and Sundermeyer (2017) for details.

and π^B. On the other hand, the contribution to the Hamiltonian from $\pi^A \dot{y}_A$ becomes a function of y_A and π^B, plus a sum of primary constraints multiplying the arbitrary functions λ^s. Although Rosenfeld for some unknown reason did not calculate the full Hamiltonian for his tetrad field, he could easily have done so using his methods, as shown in Salisbury and Sundermeyer (2017).

I have gone through Rosenfeld's method in some detail to compare and contrast with the Bergmann school procedure. Robert Penfield was actually the first to obtain the unparameterized gravitational Hamiltonian, and he did so by using the quasi-inverse E_{AB} which satisfies the relations

$$\Lambda^{AB} E_{BC} \Lambda^{CD} = \Lambda^{AD} \tag{8}$$

and

$$E_{AB} \Lambda^{BC} E_{CD} = E_{AD}. \tag{9}$$

His thesis (Penfield 1950) contains slightly more detail than the published work (Penfield 1951). The basic idea is to use the matrix $D^C{}_A$ that transforms the matrix Λ^{AB} into a "bordered" matrix in which the final four rows and columns vanish. This feat is accomplished using the four null vectors u_{sA} as the final four rows, i.e., $D^{N-4+s}{}_A = u_{sA}$, making certain that the first $N-4$ rows are linearly independent. Using AB51's notation, I will represent the bordered matrix with a prime, so that therefore $\Lambda'^{CD} = \Lambda^{AB} D^C{}_A D^D{}_B$. Continuing with their notation, I represent the resulting $N-4$-dimensional non-vanishing invertible matrix by $\Lambda'^{C^* D^*}$, where the starred indices range from 1 to $N-4$. Let the inverse be $G_{A^* B^*}$. It results that $E_{AB} = G_{C^* D^*} D^{C^*}{}_A D^{D^*}{}_B$.

Penfield did not refer explicitly to this quasi-inverse. Rather, he noted that corresponding to the bordering procedure, there was a related transformation of the $\dot{y}_A$, namely, $\dot{y}'_A = D^{-1C}{}_A \dot{y}_C$, such that $\Lambda^{AB} \dot{y}_A \dot{y}_B = \Lambda'^{AB} \dot{y}'_A \dot{y}'_B = \Lambda'^{A^* B^*} \dot{y}'_{A^*} \dot{y}'_{B^*}$. Therefore, only these specific linear combinations of the velocities were fixed by (7). Furthermore, (7) can be solved for these velocities as follows. First contract with $D^C{}_A$ to get

$$D^C{}_A \pi^A = 2\Lambda'^{CD} \dot{y}'_D + 2D^C{}_A \Lambda^{ABa} y_{B,a}, \tag{10}$$

yielding $D^{C^*}{}_A \pi^A = 2\Lambda'^{C^* D^*} \dot{y}'_{D^*} + 2D^{C^*}{}_A \Lambda^{ABa} y_{B,a}$, which can be solved to give

$$\dot{y}'_{A^*} = \frac{1}{2} G_{A^* C^*} \left(D^{C^*}{}_B \pi^B - 2D^{C^*}{}_D \Lambda^{DBa} y_{B,a} \right) =: \frac{1}{2} G_{A^* C^*} D^{C^*}{}_D \bar{\pi}^D. \tag{11}$$

Note that therefore velocities $\dot{y}^0_A$ that can be expressed in terms of the momenta are

$$\dot{y}^0_A = \frac{1}{2} D^{B^*}{}_A D^{C^*}{}_D G_{B^* C^*} \bar{\pi}^D, \tag{12}$$

and the general solution is

$$\dot{y}_A = \dot{y}_A^0 + u_{\alpha A} w_\alpha = \frac{1}{2} D^{B^*}{}_A D^{C^*}{}_D G_{B^* C^*} \bar{\pi}^D + u_{\alpha A} w_\alpha. \tag{13}$$

In addition, taking $C = N - \alpha$ in (10), we get the primary constraints

$$g_\alpha := D^{N-\alpha}{}_A \left(\pi^A - 2\Lambda^{ABa} y_{B,a} \right) = u_{\alpha A} \bar{\pi}^A = 0. \tag{14}$$

Now substituting (13) observe that the Lagrangian density becomes

$$\begin{aligned} \mathfrak{L} &= \Lambda^{AB} \dot{y}_A^0 \dot{y}_B^0 + 2\Lambda^{ABa} \left(\dot{y}_A^0 + u_{\alpha A} w_\alpha \right) y_{B,a} + \Lambda^{AaBb} y_{A,a} y_{B,b} \\ &= \frac{1}{4} G_{A^* B^*} D^{A^*}{}_C D^{B^*}{}_D \bar{\pi}^C \bar{\pi}^D + \Lambda^{ABa} D^{C^*}{}_A y_{B,a} G_{C^* D^*} D^{D^*}{}_E \bar{\pi}^E \\ &+ 2\Lambda^{ABa} u_{\alpha A} w_\alpha y_{B,a} + \Lambda^{AaBb} y_{A,a} y_{B,b}. \end{aligned} \tag{15}$$

Also

$$\pi^A \dot{y}_A = \left(\bar{\pi}^A + 2\Lambda^{ABa} y_{B,a} \right) \left(\frac{1}{2} D^{C^*}{}_A G_{C^* D^*} D^{D^*}{}_E \bar{\pi}^E + u_{\alpha A} w_\alpha \right) \tag{16}$$

Therefore the Hamilton density is

$$\mathfrak{H} = \pi^A \dot{y}_A - \mathfrak{L} = \frac{1}{4} G_{A^* B^*} D^{A^*}{}_C D^{B^*}{}_D \bar{\pi}^C \bar{\pi}^D - \Lambda^{AaBb} y_{A,a} y_{B,b} + w^\alpha g_\alpha. \tag{17}$$

This is AB51's equation (4.9), although they did not give the explicit form for the quasi-inverse. It appears in Penfield's thesis (Penfield 1950) and also in Penfield (1951). The significance of this result cannot be overemphasized. It applies to every general relativistic Lagrangian – including the Dirac and ADM Lagrangians which differ from the Einstein Lagrangian merely in the choice of S^μ. In each case we have a sum of contributions of arbitrary spacetime functions multiplying the primary constraints.

It is noteworthy also that the procedure is essentially the same that had been developed by Rosenfeld in 1930. The velocities $\dot{y}'_A$ are recognized as his $\dot{y}_A^0$ where he chooses $\dot{y}'_{N-s} = 0$. Then the general solution for the velocities is obtained by adding the $w_s(x)$ multiplying the null vectors of the Legendre matrix. I maintain that if Rosenfeld had felt so inclined, he could have undertaken a straightforward modification of his case two to address Penfield's problem. He did not because he had a much more ambitious goal: to devise a quantum theory for all known particles experiencing all known forces, including gravity. This interaction for spinorial fields required the use of tetrads.[25] It is also remarkable, as AB51 observe in a footnote, that the same construction of a particular solution $\dot{y}^0_A(\pi, y_B, y_{A,a})$ can be employed for the non-quadratic parameterized theory.[26] They use the same notation as Rosenfeld to represent it. And then as Rosenfeld noted, the general solution becomes $\dot{y}_A = \dot{y}^0_A + w^\alpha u_{\alpha A}$.

The main focus of AB51 is the construction of the phase space generators of infinitesimal general coordinate transformations, with the field variables transforming according to (3). They begin by considering the corresponding variation of the momenta (7), with the remarkable conclusion that these variations do not depend on higher time derivatives of the descriptors ξ^μ that appear in (3). Isolating the terms containing the second time derivatives, we have

$$\bar{\delta}\dot{y}_A = F_{A\mu}{}^{B0}\ddot{\xi}^\mu + \ldots = u_{\mu A}\ddot{\xi}^\mu + \ldots \tag{18}$$

But according to (7), $\bar{\delta}\pi^A = 2\Lambda^{AB}\bar{\delta}\dot{y}_B + \ldots$ and there is therefore no contribution from $\ddot{\xi}^\mu$ since $\Lambda^{AB}u_{\mu B} = 0$. This presents a puzzle that only much later did Bergmann identify in print, "During the early Fifties those of us interested in a Hamiltonian formulation of general relativity were frustrated by a recognition that no possible canonical transformations of the field variables could mirror four dimensional coordinate transformations and their commutators, not even at the infinitesimal level. That is because (infinitesimal or finite) canonical transformations deal with dynamical variables on a three-dimensional hypersurface, a Cauchy surface, and the commutator of two such infinitesimal transformations must be an infinitesimal transformation of the same kind. However, the commutator of two infinitesimal diffeomorphisms involves not only the data on a three-dimensional hypersurface but their 'time' dreivatives as well. And if these data be added to those drawn on initially, then, in order to obtain first-order 'time' derivatives of the commutator, one requires second-order 'time' derivatives of the two commutating diffeomorphisms, and so forth. The Lie algebra simply will not close."[27] In more detail, suppose one carries out an infinitesimal transformation $x^\mu_1 = x^\mu + \xi^\mu_1(x)$, followed by a second, $x^\mu(x_1) + \xi^\mu_2(x_1) \approx x^\mu + \xi^\nu_1(x) + \xi^\mu_2(x) + \xi^\mu_{2,\nu}(x)\xi^\nu_1(x)$. Then one finds the difference when the operations are carried out in reverse order. This is the Lie algebra commutator $\xi^\mu_{1,\nu}\xi^\nu_2 - \xi^\mu_{2,\nu}\xi^\nu_1$.

But then the commutator of the infinitesimal transformation with this descriptor, commuted with a third transformation, should in principle yield second time derivatives of the original descriptors ξ^μ_1 and ξ^μ_2, etc. There was a clear conflict with the diffeomorphism Lie algebra since the Poisson bracket commutator of two of these generators could not yield second time derivatives of the descriptors ξ^ν_1

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[25] See Salisbury (2009) for more context and his correspondence with Dirac!

[26] It is interesting that this statement applies also to the relativistic string, but this is not the approach that I took in the thesis (Salisbury 1977) that I wrote under Bergmann's direction nor in the follow-up preprint (Salisbury and Bergmann 1981) and publication (Salisbury 1984).

[27] Bergmann (1979, 175).

and ξ_2^ν, etc. Nevertheless, AB51's conclusion at this time was that the symmetry generator could be expanded as

$$\mathfrak{C} = {}^0A_\mu \xi^\mu + {}^1A_\mu \dot{\xi}^\mu, \tag{19}$$

with the understanding that $\bar{\delta} y_A = \left\{\int d^3x \mathfrak{C}, y_A\right\}$ and $\bar{\delta}\pi^A = \left\{\int d^3x \mathfrak{C}, \pi^A\right\}$. It did not depend on higher time derivatives of the descriptors ξ^μ. (We will see later how this apparent inconsistency was resolved.)

AB51 then required that under an infinitesimal coordinate transformation the Hamiltonian $H = \int d^3x \mathfrak{H}$ must retain (17) in the same form. Considering the special case in which the arbitrary functions w^α depend explicitly on the spacetime coordinates but not on the canonical variables, this is the condition that $\bar{\delta} H = \int d^3x \delta w^\alpha g_\alpha$. Writing out the variation of H generated by (19), we conclude that

$$\int d^3x \delta w^\alpha g_\alpha = \int d^3x \left(\{\mathfrak{C}, H\} + \frac{\partial \mathfrak{C}}{\partial t}\right) \tag{20}$$

$$= \int d^3x \left(\left\{{}^0A_\mu, H\right\}\xi^\mu + \left\{{}^1A_\mu, H\right\}\dot{\xi}^\mu + {}^0A_\mu \dot{\xi}^\mu + {}^1A_\mu \ddot{\xi}^\mu\right), \tag{21}$$

where (19) was used in the second line. Next they used the fact that at the fixed time at which these spatial integrals are calculated, each order of time derivative can be changed arbitrarily, and therefore the coefficients of each order of time derivative in δw^α needed to match with the corresponding coefficient on the right-hand side. First they focused attention on the arbitrary $\ddot{\xi}^\mu$ term in (21). On the one hand, they knew from (18) that the only term in $\bar{\delta} \dot{y}_A$ that is dependent on $\dddot{\xi}^\mu$ is $u_{\mu A} \dddot{\xi}^\mu$. But from (13), they knew that the $\dddot{\xi}^\mu$-dependent term in $\bar{\delta} \dot{y}_A$ must be $\delta w^\mu u_{\mu A}$ since the variation of the phase space variables does not depend on this higher time derivative. It follows that $\delta w^\mu = \ldots + \ddot{\xi}^\mu$. So finally they concluded that the ${}^1A_\mu$ must be the primary constraints g_μ.

Next, regarding the coefficient of $\dot{\xi}^\alpha$ on the right-hand side of (21), it is clear from the left-hand side of (20) that it must be proportional to the primary constraints, i.e., $\left\{{}^1A_\mu, H\right\} + {}^0A_\mu$ must be a linear combination of the g_α. Similarly, looking at the coefficient of ξ^α, it follows that $\left\{{}^0A_\mu, H\right\}$ must also be a linear combination of the g_α. It is noteworthy that the existence of secondary constraints does not follow from these results. Rather, AB51 were aware that the time rate of change of the primary constraints ${}^1A_\mu$ needed to be set equal to zero in order to maintain consistency with the Lagrangian theory. Indeed, they wrote that if the Poisson brackets of the primary constraints with the Hamiltonian did not vanish identically, then "they must be set equal to zero, and the requirement then becomes that the Poisson bracket of these expressions with the Hamiltonian vanish, and so on until a point is reached where no new constraints are being obtained."[28] AB51 thus insisted that the ${}^0A_\mu$

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[28] Anderson and Bergmann (1951, 1023).

and ${}^1A_\mu$, “together with the hamiltonian, form a function group.”[29] It is noteworthy that they did not deduce that the Hamiltonian itself must be a linear combination of constraints, i.e., that it must vanish.

AB51 have until recently been identified as having been the first to demonstrate that secondary constraints arise in general relativity. But that honor really belongs to Rosenfeld. He actually proved it in an argument of the type that Bergmann and Schiller later employed in 1953. His starting point was the identity (4) – although as I pointed out earlier he really looked at two special cases that did not include the Einstein Lagrangian.[30] One immediately deduces from the identity (4) the existence of a (vanishing) conserved charge, noting that $\bar{\delta}\mathfrak{L} = \mathfrak{L}^A\bar{\delta}y_A + \left(\frac{\partial\mathfrak{L}}{\partial y_{A,\mu}}\right)_{,\mu}$ and rewriting (4) as

$$0 \equiv \mathfrak{L}^A\bar{\delta}y_A + \left(\frac{\partial\mathfrak{L}}{\partial y_{A,\mu}}\bar{\delta}y_A + \bar{\delta}S^\mu - \left(\mathfrak{L} + S^\nu_{,\nu}\right)\xi^\mu\right)_{,\mu} . \tag{22}$$

So when the field equations are satisfied, i.e., $\mathfrak{L}^A = 0$, the current $\mathfrak{C}^\mu := \frac{\partial\mathfrak{L}}{\partial y_{A,\mu}}\bar{\delta}y_A + \bar{\delta}S^\mu - \left(\mathfrak{L} + S^\nu_{,\nu}\right)\xi^\mu$ is conserved, $\mathfrak{C}^\mu_{,\mu} = 0$. But since this current depends on the arbitrary time-dependent ξ^μ, it must vanish. And furthermore, in calculating the time rate of change of $d^3x\mathfrak{C}$ where $\mathfrak{C} := \mathfrak{C}^0$, one obtains precisely the AB51 relation on the right-hand side of (19) where one already knows that ${}^0A_\mu$ and ${}^1A_\mu$ are constraints. In addition, Rosenfeld showed explicitly that $d^3x\mathfrak{C}$ generated the correct variations of the phase space variables under infinitesimal diffeomorphisms. It is remarkable that he also did not recognize that the vacuum general relativistic Hamiltonian vanished – even though the particular generator $d^3x\mathfrak{C}$ with $\xi^\mu = \delta^\mu_0$ was ostensibly the Hamiltonian – except for a possible spatial surface integral![31] Schiller himself used this vanishing conserved charge in his thesis (Schiller 1952), and he was therefore apparently the first in the Syracuse group to note its role as the generator of coordinate symmetry transformations.[32]

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[29] Anderson and Bergmann (1951, 1023).

[30] See Salisbury and Sundermeyer (2017) and in particular equation (37).

[31] Josh Goldberg has informed me that Bergmann himself took some time to come to this realization.

[32] I should add parenthetically that the evidence suggests that Rosenfeld was probably aware of the daunting challenge one faced in finding a constraint algebra that corresponded to the conventional diffeomorphism Lie algebra. Regarding the algebra he confined his attention to spatial diffeomorphisms whose generators did satisfy a closed Poisson bracket algebra. This discussion is in his Sect. 6 (Rosenfeld 1930, 2017). See also (Salisbury and Sundermeyer 2017, 43–44).

3 Anderson, Bergmann and Schiller, and Lagrangian Approaches to Quantum Gravity

After reviewing the fundamentals described above, roughly the latter half of Anderson's 1952 thesis (Anderson 1952) was devoted to a quantum gravitational attempt modeled on Schwinger's Lagrangian approach. The idea was that a Lagrangian approach could prove to be simpler to implement given that in configuration-velocity space the primary constraints are identities. The hope was then to deduce a quantum commutation relation among field variables and time derivatives by assuming, as did Schwinger, that the variation of quantum transition amplitudes was generated by a Hermitian operator. He proposed an approximation procedure for implementing this idea, assuming a quadratic Lagrangian operator of the form

$$\hat{L} = \hat{y}_{A,\mu}\hat{\Lambda}^{A\mu B\nu}\hat{y}_{B,\nu} + \hat{\Delta}, \tag{23}$$

where $\hat{\Lambda}$ and $\hat{\Delta}$ are functions of the undifferentiated $\hat{y}_A$. He notes that in considering variations $\delta\hat{y}_A$ he cannot alter the order of factors, and it is therefore impossible to deduce the field equations in the conventional manner (say with all $\delta\hat{y}_A$ to the right). He argues that c-number variations are excluded since then it would not be possible to undertake finite transformations $\hat{y}_A = \hat{y}_A(\hat{y}'_B)$. His conclusion, adopted later by Bergmann and Schiller (1953), is that the quantum Lagrangian may not be associated with a general variational principle. He does require, instead, that the varied Lagrangian differ identically from the original only by a total divergence, and this does lead to the canonically quantized Hamiltonian approach with the usual cast of primary and secondary constraints.

Thus Anderson proposed to implement a Schwinger variational principle of the form

$$\delta\left\langle\alpha'_1, t_1|\alpha'_2, t_2\right\rangle =< \alpha'_1, t_1|\delta\int d^4x\hat{L}|\alpha'_2, t_2 >, \tag{24}$$

where the α' constitute a complete set of eigenvalues of the operators $\hat{\alpha}$. It is noteworthy that his approximation procedure was modeled after the Gupta-Bleuler procedure in quantum electrodynamics, in which the vanishing of the positive frequency components of gauge constraint is employed as a condition on quantum states. Anderson cited in this regard the Schwinger and Feynman techniques as expounded in Dyson's 1951 Cornell lectures (Dyson 2007) and his groundbreaking 1949 paper (Dyson 1949).[33]

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[33] Anderson explained to the author and Rickles in 2011 that his exposure to Dyson did not come from Syracuse, but rather from a visit to Mexico in the summer of 1951 where he worked with Alejandro Medina and "tried to understand Dyson's paper, and the renormalization program. So then I got very much involved in quantum field theory."

In 1953 Bergmann and his student Ralph Schiller BS53 (Bergmann and Schiller 1953) decided to pursue a Schwinger-type Lagrangian approach to quantum gravity. They claimed, however, that it was not possible to formulate a Schwinger-like quantum action principle that would be valid for this wider class of solution variations. Indeed, if this had been possible, then it presumably would have been possible to deduce a quantum commutator algebra for all the gravitational metric variables. Instead, they showed that in the vacuum case it was sufficient to restrict the variations to those engendered by general coordinate transformations – even though, as mentioned above, the generator of these variations vanished. Indeed, in the Lagrangian formulation, the primary constraints vanish identically, and the remaining constraints are nothing other than the four Einstein equations $G_{0\mu} = 0$ that do not involve second time derivatives of the metric.

As in the papers cited previously, BS53 considered arbitrary generally covariant theories, but now with field operator variables $\hat{y}_A$ (with the "hat" signifying an operator) described by a Lagrangian $L(\hat{y}_A, \hat{y}_{A,\mu})$, and for the purpose of this discussion of the differences that arise with operators, I will spell out the new identities that arise under general coordinate transformations as a consequence of operator factor ordering. I work with Anderson's quadratic Lagrangian (23). Assuming a change in coordinates $x'^\mu = x^\mu + \xi^\mu$, the corresponding variations of y_A are $\bar{\delta}\hat{y}_A = F_{A\mu}{}^{B\nu}\hat{y}_B\xi^\mu_{,\nu} - \hat{y}_{A,\mu}\xi^\mu$ where the $F_{A\mu}{}^{B\nu}$ are constants and $\bar{\delta}\hat{y}_A(x) := \hat{y}'_A(x) - \hat{y}_A(x)$ is minus the Lie derivative in the ξ^μ direction. The variation in the Lagrangian operator is

$$\bar{\delta}L = \bar{\delta}\hat{y}_{A,\mu}\hat{\Lambda}^{A\mu B\nu}\hat{y}_{B,\nu} + \hat{y}_{A,\mu}\hat{\Lambda}^{A\mu B\nu}\bar{\delta}\hat{y}_{B,\nu} + \left\{\frac{\partial\hat{\Delta}}{\partial\hat{y}_C}\cdot\bar{\delta}\hat{y}_C\right\} + \hat{y}_{A,\mu}\left\{\frac{\partial\hat{\Lambda}^{A\mu B\nu}}{\partial\hat{y}_C}\cdot\bar{\delta}\hat{y}_C\right\}\hat{y}_{B,\nu},$$

where the curly bracket and dot notation employed by BS53 denotes the insertion of the variation where the vacancy occurs in each derivative. Continuing, we have

$$\begin{aligned}\bar{\delta}L = {}& \left(\bar{\delta}\hat{y}_A\hat{\Lambda}^{A\mu B\nu}\hat{y}_{B,\nu} + \hat{y}_{A,\nu}\hat{\Lambda}^{A\nu B\mu}\bar{\delta}\hat{y}_B\right)_{,\mu} + \left\{\frac{\partial\hat{\Delta}}{\partial\hat{y}_C}\cdot\bar{\delta}\hat{y}_C\right\} + \hat{y}_{A,\mu}\left\{\frac{\partial\hat{\Lambda}^{A\mu B\nu}}{\partial\hat{y}_C}\cdot\bar{\delta}\hat{y}_C\right\}\hat{y}_{B,\nu}\\ & -\bar{\delta}\hat{y}_A\left(\hat{\Lambda}^{A\mu B\nu}\hat{y}_{B,\nu}\right)_{,\mu} - \left(\hat{y}_{A,\nu}\hat{\Lambda}^{A\nu B\mu}\right)_{,\mu}\bar{\delta}\hat{y}_B\\ := {}& \left(\bar{\delta}\hat{y}_A\hat{\Lambda}^{A\mu B\nu}\hat{y}_{B,\nu} + \hat{y}_{A,\nu}\hat{\Lambda}^{A\nu B\mu}\bar{\delta}\hat{y}_B\right)_{,\mu} + \left\{\hat{L}^A\cdot\bar{\delta}\hat{y}_A\right\}.\end{aligned}$$

It is presumed that this variation satisfies identities that correspond in the classical realm to the Lagrangian varying as a density of weight one plus a total divergence, as discussed earlier. So as in the classical case one ends up with three identities, namely, the vanishing of the coefficients of each order of time derivative of the arbitrary c-number descriptors ξ^μ. The crucial result is that the authors determine that the following 16 quantum operator field equations must vanish,

$$\left\{\hat{L}^{A} \cdot F_{A\mu}{}^{B\nu} \hat{y}_{B}\right\} = 0. \tag{25}$$

The authors maintain, although they do not give a proof, that the classical Einstein field equations (and Einstein-Maxwell when the additional gauge symmetries are included) result in the limit as $\hbar \to 0$.[34] Specializing the Lagrangian symmetry to rigid translation in time, they also obtain the quantum generator of time evolution in the Lagrangian framework which takes the form $\left\{\frac{\partial \hat{L}}{\partial \dot{\hat{y}}_A} \cdot \dot{\hat{y}}_A\right\} - \hat{L} := \hat{H}$ with the corresponding Schrödinger equation $\hat{H}\Psi = i\hbar\frac{\partial \Psi}{\partial t}$.

They also have a general expression for the quantum generator of general coordinate transformations, $\int d^3x \left(\left\{\frac{\partial \hat{L}}{\partial \hat{y}_{A,0}} \cdot \bar{\delta}\hat{y}_A\right\} - \hat{Q}^0\right)$ where $\hat{Q}^0$ arises from the divergence term in the variation of the Lagrangian. Using this generator it is possible to deduce commutation relations as does Schwinger in the Lorentz covariant case. There are, however, commutators that cannot be determined. Lorentz covariant quantum electrodynamics is cited as an example. Since one is working explicitly with Lagrangian expressions the momentum conjugate to $\hat{A}_0$ vanishes identically. (It is a primary constraint.) So the Lagrangian expression for the $U(1)$ generator is $\frac{1}{2\pi}\int d^3x \partial^{[0}\hat{A}^{s]}\xi_{,s}$, and requiring that $\delta\hat{A}_\rho = \xi_{,\rho}$ does not lead to a definite commutation relation involving $\hat{A}_0$. The authors' conclusion is that "the commutation relations between the $\hat{y}_A$ and $\dot{\hat{y}}_A$ are not determined completely," and "preliminary examination shows, however, that the variables whose time derivatives remain indeterminate are precisely the ones whose time derivatives are also indeterminate in the classical theory." Thus, the quantum theory must deal exclusively with observables, fields which have vanishing Poisson brackets with the generators of general coordinate transformation symmetries!

4 Generators of General Canonical Transformations and Reduced Phase Space Algebra

One more significant innovation in the BS53 paper was the identification within the Lagrangian framework of a generally covariant system of transformations of the y_A and $y_{A,\mu}$ that would correspond in phase space to general canonical transformations – including changes that could alter the form of the field equations. The general class would however include transformations that produced physically distinct solutions to these equations. The authors identified as canonical transformations those that did not introduce higher time derivatives in the field equations. They then focused

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[34] For vacuum general relativity, we read off from $\bar{\delta}g_{\mu\nu} = -g_{\mu\nu,\alpha}\delta\xi^\alpha + 2g_{\alpha(\mu}\delta\xi^\alpha_{,\nu)} =: -g_{\mu\nu,\alpha}\delta\xi^\alpha + F_{(\mu\nu)\rho}{}^{(\alpha\beta)\sigma}\delta\xi^\rho_{,\sigma}$ that $F_{(\mu\nu)\rho}{}^{(\alpha\beta)\sigma} = 4\delta^\sigma_{(\mu}\delta^{(\beta}_{\nu)}\delta^{\alpha)}_\rho$. The proposed field operator equations are then $\left\{\hat{L}^{\sigma\alpha} \cdot \hat{g}_{\alpha\rho}\right\} = 0$.

on those transformations that would in the classical realm not change the form of the field equations and would therefore generally involve the addition of a divergence $Q^{\mu}_{,\mu}$ to the varied Lagrangian. Representing those configuration-velocity transformations that did not alter the equations of motion as $\bar{\delta} y_A = f_A(y_B, y_{B,\rho})$, they were able to show that even in the presence of constraints, it was still possible to write these permissible variations in terms of the momenta π^C as

$$f_C = \frac{\partial}{\partial \pi^C}\left(\pi^B f_B - Q^4\right).$$

This feat was achieved by expressing the velocities $\dot{y}_A$ as functions of the momenta, satisfying constraints $g_i(y_A, y_{B,a}, \pi^C) = 0$ and arbitrary variables w^i. This rendered meaningful the derivatives with respect to π^A of the velocity argument that appeared in $f_A(y_B, y_{B,a}, \dot{y}_C)$. The chain rule for differentiation using these new variables was valid, however, only for functions F satisfying the condition $\frac{\partial w^i}{\partial \dot{y}_A}\frac{\partial F}{\partial \pi^A} = 0$. As a consequence it turned out that the generator $C := \pi^B f_B - Q^4$ was independent of w^i.

Bergmann and I. Goldberg continued in 1955 (BG55) (Bergmann and Goldberg 1955a) this investigation of general canonical transformations and its subgroup of invariant transformations (i.e., those corresponding to diffeomorphisms and perhaps additional gauge symmetries), but in this instance in phase space. Their substantial achievement has gone largely unrecognized. They invented a new non-canonical phase space bracket which they modestly called an "extended Dirac bracket," but as I noted above, I refer to it as the Bergmann-Goldberg bracket. This bracket, in a sense to be explained, uniquely expresses the algebra of diffeomorphism-invariant observables, but it is formulated in a general manner applicable to any Hamiltonian model possessing gauge symmetry. Remarkably, the resulting reduced phase space algebra of constants of the motion in general relativity is obtained without the imposition of coordinate conditions. Rather, the scheme requires the identification of null vectors of the symplectic form in addition to variables (not necessarily canonical) for the constraint hypersurface. Then the idea was that invariant variables could be identified, invariant in the sense that they do not change in the null directions. As we shall see below, this is equivalence to constructing diffeomorphism invariants.

The authors worked with a finite-dimensional phase space of dimension $2\mathfrak{N}$. I will address later the extension to field theory. Specifically, let ζ^{μ} be the configuration – momenta set q_k, p_k, with the canonical equations of motion $\dot{\zeta}^{\mu} = \epsilon^{\mu\nu}\frac{\partial H}{\partial \zeta^{\nu}}$, where

$$\epsilon^{\mu\nu} = \begin{pmatrix} 0 & I \\ -I & 0 \end{pmatrix}.$$

Let $C^a(\zeta^{\mu}) = 0$, $a = 1, \ldots, N$, represent constraints. Now confine attention to the constraint hypersurface which we assume to be covered by the phase space functions y^m. It is convenient to let $Y^\alpha := y^m, C^a$ cover the full $2\mathfrak{N}$-dimensional phase space. BG55 showed that those transformations δy^m on the constraint hypersurface that preserve the constraints are generated by functions $A(y)$, such that

$$\epsilon_{mn}\delta_A y^n = \frac{\partial A}{\partial y^m}, \tag{26}$$

where $\epsilon_{mn} := \zeta^\mu_{,m}\epsilon_{\mu\nu}\zeta^\nu_{,n}$.

Most interesting for us at the moment is the situation in which ϵ_{mn} is singular. Indeed, let us suppose that the constraints are all first class, in which case there will exist N independent null vectors $U^m_{(s)}$, i.e., $\epsilon_{mn}U^n_{(s)} = 0$. Then contracting $U^m_{(s)}$ with (26), it follows that the generator must satisfy the condition

$$\frac{\partial A}{\partial y^m}U^m_{(s)} = 0. \tag{27}$$

BG55 then showed that the commutator of two transformations satisfying this condition, with generators A and B, has a generator

$$\frac{\partial A}{\partial y^m}\delta_B y^m - \frac{\partial B}{\partial y^m}\delta_A y^m + \epsilon_{mn}\delta_A y^m \delta_B y^n =: \{A, B\}_{BG} \tag{28}$$

This is the definition of the Bergmann-Goldberg bracket. Most importantly, they proved that this commutator generator also satisfied the condition (27), i.e.,

$$\frac{\partial\{A, B\}_{BG}}{\partial y^m}U^m_{(s)} = 0.$$

The authors then wrote this expression in terms of the quasi-inverse η^{mn} of the matrix ϵ_{mn} – an object that we encountered earlier in a different context. This has the property

$$\eta^{lm}\epsilon_{mn} = \delta^l_n - U^l_{(s)}V^{(s)}_n,$$

where $V^{(s)}_n$ is a null vector of η^{ln}. Thus multiplying (26) by the quasi-inverse there results

$$\delta_A y^m = U^m_{(s)}V^{(s)}_n\delta y^n - \eta^{mn}A_{,n}.$$

Inserting this into (28) gives, taking (27) into account,

$$\{A, B\}_{BG} = \eta^{mn}A_{,m}B_{,n}. \tag{29}$$

We need one more consequence of our assumption that the constraints are first class, i.e.,

$$\left\{C^a, C^b\right\} = c^d_{ab} C_d.$$

Note that

$$\left\{C^a, Y^\alpha\right\} \epsilon_{\alpha n} = \epsilon^{\mu\nu} C^a_{,\mu} Y^\alpha_{,\nu} \epsilon_{\rho\sigma} \zeta^\rho_{,\alpha} \zeta^\sigma_{,n} = \epsilon^{\mu\nu} C^a_{,\mu} \epsilon_{\rho\sigma} \zeta^\sigma_{,n} \delta^\rho_\nu = \delta^\mu_\sigma C^a_{,\mu} \zeta^\sigma_{,n} = \delta^a_n = 0,$$

or, expanding the left-hand side, we find that on the constraint hypersurface

$$\left\{C^a, y^s\right\} \epsilon_{sn} = -\left\{C^a, C^b\right\} \epsilon_{bn} = -\left\{C^a, C^b\right\} c^d_{ab} C_d \epsilon_{bn} = 0.$$

In other words, $\delta^a y^s := \{C^a, y^s\}$ is a null vector of ϵ_{sn}. But, as we have seen, not only are the generators A and B invariant under this null transformation, but so is the commutator. The condition (27) now has a clear physical meaning. It assumes the form

$$0 = \frac{\partial A}{\partial y^m} \delta^a y^m = \left\{A, C^a\right\}.$$

In other words the permissible generators must be invariant under the action of the gauge group. Thus we have the remarkable result that the Bergmann-Goldberg bracket gives the algebra satisfied by variables that are invariant under the action of the constraints. In the context of general relativity, these invariants are constants of the motion – in addition to their independence of spatial coordinates! The algebra of observables of the reduced phase space is however not unique because of the arbitrariness present in the quasi-inverse. A specific choice will result from the imposition of coordinate conditions. Indeed, when all the constraints are rendered second class in this manner, BG55 have proven that their generalized bracket becomes the Dirac bracket. There is another significant aspect of this result that has not received the attention it is due, perhaps in part because Bergmann and his collaborators have never stated it explicitly. Or they might not have fully appreciated the significance of their result as is suggested by the Bergmann-Janis correspondence that will be discussed below. The fact is that one can obtain diffeomorphism invariants through the imposition of appropriate coordinate conditions – appropriate in the sense that they render the original set of first-class constraints plus coordinate conditions second class. The proof follows from the authors' requirement that their generalized brackets are valid only for variables that are invariant under the action of the original first-class generators. Before this paper appeared, the argument had been made that simply by restricting the constrained phase space through coordinate conditions, one was proceeding stepwise to the construction of invariants – although a direct demonstration that the variables satisfying the eventual Dirac bracket algebra were indeed invariant under the action of the diffeomorphism generators was lacking.

But on the other hand, to carry out the construction of the reduced algebra, one must be in possession of invariants. Knowledge of the null vectors can serve in this search, as BG55 briefly illustrated with classical electromagnetism. In this case the first-class constraints are $\pi^0 = \frac{\partial \mathcal{L}}{\partial \dot{A}_0} = 0$ and $\frac{\partial \pi^a}{\partial x^a} = 0$ where $\pi^a = \frac{\delta \mathcal{L}}{\delta \dot{A}_a}$. This second constraint is the statement that the longitudinal canonical momentum π_l^a (the longitudinal electric field) is determined by the charged sources. The natural choice for the variables y^m in this case is $y^m(\vec{x}) = \left(A_0, A_{l\,a}, A_{t\,b}, \pi_t^c\right)$, where the t subscript represents the transverse field. As we have seen the constraints generate variations in null directions of ϵ_{mn}, namely, producing arbitrary variations in A_0 and $A_{l\,a}$. The resulting Bergmann-Goldberg algebra is therefore the algebra satisfied by the gauge invariant transverse electric and magnetic fields.

5 Gravitational Observables, Coordinate Conditions, and Intrinsic Coordinates

Bergmann and his collaborators had therefore reached a definitive positive conclusion on the nature of the algebra of observables in classical general relativity, in both the Hamiltonian and Lagrangian approaches. Observables were invariant under the action of the full diffeomorphism group, and the Bergmann-Goldberg bracket represented their commutator algebra. But there remained a problem. One still needed to find a complete set of invariant functionals that represented observables. Newman initiated an iterative construction of invariants in his 1956 thesis (Newman 1956), continued in a joint publication with Bergmann in 1957 (Newman and Bergmann 1957). But the evidence suggested that it might perhaps be helpful to try a new tack, modeled in part on success in constructing gauge invariants in electromagnetism as Bergmann explained in 1956 (Bergmann 1956). Bergmann's student Allen Janis completed a thesis in 1957 (Janis 1957) in which he investigated how to find invariants through the imposition of coordinate conditions. The thesis confined attention to what he called "Lorentz-type" conditions of the form

$$C^j(y_A, y_{A,\rho}) = C^{*j}(y_A) + C^{jA\rho}(y_B)y_{A,\rho} = 0, \tag{30}$$

corresponding to the Lorenz gauge in electromagnetism.[35] A 1958 joint publication (Bergmann and Janis 1958) confined the analysis to coordinate conditions that do not depend on time derivatives $\dot{y}_A$. The idea in both instances is to add to the Lagrangian, as did Fermi in electrodynamics, a term that with suitable initial conditions renders the Lagrangian non-singular while imposing the gauge

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[35] A January 1957 APS abstract with Bergmann (Janis and Bergmann 1957) is similarly limited in scope.

conditions. The appropriate term to add is $\frac{1}{2}a_{ij}C^iC^j$, and the initial data must be chosen consistent with (30). As in the previous papers, the focus here was to identify the temporal boundary terms that corresponded to the implementation of a canonical change in the physical state. The new consideration here was that infinitesimal changes needed to respect the coordinate conditions, i.e., neither the form of the conditions nor their zero value (modulo the equations of motion), were permitted to change under variations $\bar{\delta} y_A$. They required that the equations of motion retained their form and also that the Lagrangian might be altered by a total time derivative. This latter condition took an altered form since it was required to hold only for data that satisfied the coordinate condition. Therefore the requirement was that $\bar{\delta} L' = \dot{Q} + \frac{1}{2}\delta a_{ij}C^iC^j$, where they reverted to the finite-dimensional case. The generator boundary terms C'^j again took the form $C' = -Q + \frac{\partial L'}{\partial \dot{q}_k}\bar{\delta} q_k$. And as in the earlier papers, it was possible to show that $\dot{C}' + M^k\bar{\delta} q_k = 0$ where $M^k = 0$ are the Euler-Lagrange equations. The C' are therefore generally non-vanishing constants of the motion. They recognized that there might still remain some gauge freedom after the imposition of coordinate conditions, and these were to be factored out, just as the invariant transformations were factored out in forming the reduced algebra represented by the Bergmann-Goldberg algebra. Furthermore, and this was the message of the paper, these reduced algebras were identical, thereby showing that the imposition of coordinate conditions was legitimate. The equivalence was demonstrated explicitly for electromagnetism (in fact, focusing on only one Fourier mode of oscillation). They did express some misgivings about the practical use of this method as one was still faced with the technical challenge of constructing constants of the motion whose corresponding transformations respected the coordinate conditions. In closing they mention an alternative approach due to Komar and Géhéniau and Debever.

Komar had already in his 1956 thesis (Komar 1956) (written under Wheeler's direction) used the Géhéniau and Debever (Géhéniau and Debever 1956) results as a means of distinguishing physically inequivalent solutions of Einstein's equations. Komar argued that in what he called "asymmetric" spacetimes, i.e., spacetimes that possess no Killing symmetries, the four independent Weyl scalars, formed with up to second derivatives of the metric, could be employed to define what he called "intrinsic coordinates." And in the frame in which these coordinates were employed, the metric components would be uniquely determined and hence diffeomorphism invariants.[36] Komar even made a specific proposal for the coordinates, though

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[36] It is not clear when Bergmann became aware of Komar's work. Géhéniau summarized his joint work with Debever at the July 1955 Bern meeting. They proved that there existed at most 14 independent second-order spacetime scalars. Bergmann posed a question, following the presentation, relating to the existence of only four second differential order scalars that existed for vacuum spacetimes that possessed no symmetry. There is an edited proof (which does not appear to be in Bergmann's handwriting) of the Géhéniau article that was to appear in the Bern proceedings. The proof is in the Syracuse Bergmann archives (SUBA) in a folder labeled Bern Correspondence and does not mention Komar. The document states that Bergmann later had a private discussion with Wigner, and Wigner's response to Bergmann's inquiry follows. However,

without proof that his choices corresponded to spatial and timelike directions. Komar published a detailed proposal for the implementation of intrinsic coordinates in 1958 (Komar 1958), focusing first on the implications for the initial value problem in general relativity. He was able to show that the intrinsic coordinate choice led to unique temporal evolution. But of special concern to him was whether it would be possible to isolate from the redundant set of metric components and time derivatives a non-redundant set of four constants of the motion that would label physically distinguishable spacetimes. He addressed this question in studying the initial value problem.

Allen Janis, in an unpublished draft written in 1958,[37] did address the issue of the relationship in general between invariants and their associated algebra and the variables that satisfied the Dirac algebra after the imposition of constraints. The former he called "primary observables" and the latter "secondary observables." This proposed distinction elicited a revealing discussion in correspondence in 1958 between Allen Janis and Bergmann. Bergmann's first remark concerns gauge freedom that might remain if a residual asymptotic invariance group is present, in which case he observes that an invariance group that "contains no arbitrary functions of *four* coordinates" would be admissible. He also notes that in any case, as is true with the Komar approach, there will remain a redundance of observables. He then points to the recent work of Arnowitt, Deser, and Misner – to be published and also the work of Arnowitt and Deser reported "at our meetings at Zurich and Neuchatel, who pointed out that the secondary observables can often be interpreted as primary observables, so that the distinction can often be more historical than actual. Take, as an example, the 'transverse' portion of some electromagnetic variable, e. g., the vector potential. One may either define the transverse vector potential as a certain functional of the vector potential (by means of an integral projection operator), so that it qualifies as a primary observable, without reference to a restricted coordinate frame; or one may introduce a special radiation gauge, in which case the transverse vector potential is simply the vector potential. Certainly, Komar's observables are

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the ultimate published version contains the comment "The question that must be decided (and that Komar in Princeton has also addressed) concerned the characterization of definitively distinct solutions of Einstein's gravitational equations." "Die Frage, die entschieden werden sollte (und die auch Herr Komar in Princeton angegriffen hatte) betraf die Charakterisierung wesentlich verschiedener Lösungen der EINSTEINschen Gravitationsgleichungen." It is likely that Bergmann heard the basis of his question directly from Komar at the April 1955 Washington meeting of the American Physical Society, where they were both present on the same day. Komar likely referred in his report (Komar 1955) to an observation that would appear in his Ph.D. thesis (Komar 1956), citing the as yet unpublished (Géhéniau and Debever 1956) result that in a generic vacuum spacetime, there exist four independent scalar invariants of second differential order. We know that Komar did go to Syracuse as a postdoctoral researcher, presumably at the beginning of the 1957 academic year. He stayed at Syracuse until 1963, promoted eventually to Associate Professor (Goldberg 2013).

[37] A. Janis (1958). *True observables and the generalized equivalence problem*. SUBA, Correspondence folder.

interpretable either as primary or as secondary observables, depending on one's point of view."[38]

Correspondence by Bergmann with Dirac in 1959 sheds more light on the state of affairs with coordinate conditions at this time.[39] The letter was in response to Dirac's 1959 publication (Dirac 1959) of a suggestion of suitable coordinate conditions in general relativity. Dirac had published in 1958 (D58) (Dirac 1958a) his groundbreaking paper in which he simplified the primary constraints in general relativity through the addition of a total divergence to the Lagrangian, thereby eliminating time derivatives of $g_{0\mu}$. These components were thus freely prescribable. Bergmann wrote that "(1) …regardless of the motive in introducing the metric g_{rs} on the initial hypersurface, the canonical transformation that you first published a year ago to simplify and kill the primary constraints, is both legitimate and successful. At this stage the total number of canonical field variables is reduced from twenty to twelve." He then goes on to discuss the proposed coordinate conditions. "(2) What I have found most remarkable is the manner in which you have introduced coordinate conditions to change the secondary first-class constraints into second-class constraints, and eventually to reduce the theory to four canonical field variables. As far as I can see, your procedure and the one that Komar and I are still working on supplement each other: Your coordinates and field variables are intuitive, and results obtained will lend themselves to physical interpretation in terms of concepts with which we are familiar from other field theories. Your expansions will break down, or at least become unwieldy, if the field should be strong. You yourself have called attention to this fact. Komar and I work principally with closed-form expressions. Our variables are developed locally and require no solution of partial differential equations; but even if and when we complete our construction of the complete Lie algebra of our observables, including the dynamical laws, our results will be difficult to relate to more conventional concepts and procedures. Perhaps someone here will attempt to prove in some detail the mathematical equivalence of our two approaches. I do hope that we shall soon have another chance to compare notes on our progress personally, on either side of the Atlantic Ocean."[40]

An addendum to the same letter, dated October 14, raises another issue related to the problem of time. "(3) When I discussed your paper at the Stevens conference yesterday, two more questions arose, which I should like to submit to you; To me it appeared that because you use the Hamiltonian constraint H_L to eliminate one of the non-substantive field variables, κ, in the final formulation of the theory your Hamiltonian vanishes strongly, and hence all the final field variables, i.e., $\tilde{e}^{rs}$, $\tilde{p}^{rs}$, are 'frozen' (constants of the motion). I should not consider that as a source of embarrassment, but Jim Anderson says that in talking to you he found that you now look at the situation a bit differently. Can you enlighten me? If you have no

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[38] Letter from Bergmann to Janis dated September 15, 1958. SUBA, Correspondence folder.

[39] Letter from Bergmann to Dirac dated October 9, 1959. SUBA, Correspondence folder.

[40] Letter from Bergmann to Dirac dated October 9, 1959. SUBA, Correspondence folder.

objection, I should communicate your reply to Anderson and a few other participants in the discussion."

Dirac responded on November 11, 1959,[41] first with the terse acknowledgement "I fully agree with your comments (1) and (2)." Then he addressed the frozen time issue. "If the conditions you introduce to fix the surface are such that only one surface satisfies the conditions, then the surface cannot move at all, the Hamiltonian will vanish strongly and all dynamical variables will be frozen. However, one may introduce conditions which allow an infinity of roughly parallel surfaces. The surface can then move with one degree of freedom and there must be one non-vanishing Hamiltonian that generates this motion. I believe my condition $g_{rs} p^{rs} \simeq 0$ is of this second type, or maybe it allows also a more general motion of the surface corresponding roughly to Lorentz transformations. The non-vanishing Hamiltonian one would get by substituting a divergence term from the density of the Hamiltonian." We are of course aware that Bergmann had much earlier concluded that observables must be independent of the coordinate time – and this would imply that the Hamiltonian resulting after the imposition of coordinate conditions would vanish identically, simply because the vanishing Hamiltonian constraint would be explicitly solved to express dependent degrees of freedom in terms of an independent set. This is a point that Anderson made in an undated letter to Bergmann commenting on a pre-publication draft he had obtained of Dirac's paper.[42] It turns out here that Dirac's coordinate condition did not fully eliminate the freedom in fixing the coordinate time. There remained a physically spurious one-parameter freedom. This issue was later partially addressed in Anderson's 1964 comparison of coordinate fixation techniques (Anderson 1964).[43] It turns out that to fully eliminate this freedom, the coordinate time must appear explicitly in a coordinate condition – as it does with Bergmann-Komar intrinsic coordinates.[44]

Indeed, while Bergmann and Komar were convinced that observables could not depend on coordinate time, they were also well aware that intrinsic time dependence was not excluded! They made this case quite explicitly in 1962 in their contribution to the Infeld Festschrift (Bergmann and Komar 1962) in a section entitled "Time-dependent solutions." They proposed interpreting an appropriately chosen function of canonical variables as the time. The idea was to choose a constant of the motion C and find the canonical conjugate to it, θ. Then since the Poisson bracket of C with the Hamiltonian H weakly vanishes, H must be independent of θ – at least on the constraint hypersurface. Thus one can solve for C and write $C + h(\phi, \pi) = H = 0$, where the arguments of h are the remaining phase variables with C and θ excluded.

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[41] Letter from Dirac to Bergmann dated November 11, 1959. SUBA, Correspondence folder.

[42] Letter from Anderson to Bergmann, undated. SUBA, Correspondence folder.

[43] SUBA, Correspondence folder. There is a related undated letter. Anderson writes "Enclosed is what I hope is a corrected and correct version of the paper we discussed over the phone. As I mentioned then, I agree in the main with your comments and in fact appreciated them." Unfortunately we are not in possession of the earlier draft.

[44] See Pons et al. (1997) for a proof.

Let {, } represent the original Poisson bracket and [,] the bracket formed with ϕ, π. A variable A will then satisfy the equation of motion

$$\frac{dA}{d\theta} = \{A, H\} = [A, h] + \left(\frac{\partial A}{\partial \theta} \frac{\partial H}{\partial C} - \frac{\partial C}{\partial \theta} \frac{\partial \theta}{\partial C} \right) = [A, h] + \frac{\partial A}{\partial \theta}.$$

6 Dirac Variables and Bergmann's Interpretation

I have already mentioned, in conjunction with AB51, the puzzle regarding the failure to implement the full diffeomorphism Lie algebra. In fact, although AB51 did not remark on this fact, the mystery deepened with the deduction in this article that the associated Poisson bracket algebra did not arise for the wide class of generally covariant field theories they considered. This broad class included the quadratic Einstein model (2) although they did not display the explicit form for the secondary constraints for this specific case. They did however construct the general secondary constraints, and they displayed in their equation (7.7) the full algebra of these among themselves and also with the primary constraints – without calling attention to the Lie algebra puzzle. Bergmann's resolution was inspired by Dirac's gravitational Hamiltonian paper D58. One can best appreciate its significance in citing a passage from Bergmann's letter to Nathan Rosen in 1973, in which he proposes that Dirac be invited to talk at the 7th General Relativity and Gravitation conference to be held in Tel Aviv. He writes "Having through an extended period wrestled with the same problems that he succeeded in solving – a viable Hamiltonian version of general relativity, I have the profoundest respect for his genius, second only (in my personal experience) to Einstein."[45] I had not fully appreciated until recently, after rereading his *Handbuch der Physik* article, that Bergmann is not referring here to Dirac's earlier constrained Hamiltonian dynamics algorithm, developed concurrently with Bergmann, but to Dirac's apparently unwitting solution of the algebra conundrum.

As noted above, Dirac was the first to publish a simplified version of the primary constraints in general relativity in which they appear as vanishing momenta (Dirac 1958b). DeWitt had already reported a similar result at a Stevens meeting, although for the parameterized theory. This had inspired Anderson to seek an analogous result in the unparameterized model which he published shortly after Dirac (Anderson 1958). With the addition of an appropriate divergence to the Einstein Lagrangian, Dirac showed that the four momenta conjugate to $g_{0\mu}$ must vanish. It follows immediately that the $g_{0\mu}$ are arbitrary. He was also the first to make the critical discovery that the canonical variables g_{ab} and π^{cd} were invariant under diffeomorphisms that left the spatial coordinates on a given spacelike hypersurface of constant x^0 fixed. In his 1962 *Handbuch de Physik* article (Bergmann 1962), Bergmann called these variables "D-invariant" in Dirac's honor. Under an arbitrary

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[45] Letter from Bergmann to Rosen dated September 26, 1973. SUBA, Correspondence folder.

infinitesimal diffeomorphism $x'^{\mu} = x^{\mu} + \xi^{\mu}(x)$, the change in D-invariants by definition does not depend on time derivatives of the ξ^{μ}. Although Dirac did not demonstrate this explicitly, he clearly knew that given any vector A_{μ}, in addition to A_a constituting a D-invariant, there is a fourth D-invariant $A_L := A_{\mu} n^{\mu}$, where $n^{\mu} = -g^{0\mu} \left(-g^{00}\right)^{-1/2} = \left(N^{-1}, -N^{-1} N^a\right)$ is the unit normal to the $x^0 = const$ hypersurface.[46] Dirac astutely reasoned that the time rate of change of the phase space variables would correspond to the naught component of a vector, namely, the variation of a variable η under a change in time would be of the form

$$\delta\eta = \int d^3x \, \{\mathfrak{H}_0, \eta\} \, \delta t, \tag{31}$$

and the task was then to express $\mathfrak{H}_0$ in terms of D-invariants using $\mathfrak{H}_L := \mathfrak{H}_{\mu} n^{\mu}$. We read off that $\mathfrak{H}_0 = \frac{1}{n^0} \left(\mathfrak{H}_L - \mathfrak{H}_a n^a\right) = N \mathfrak{H}_L + N^a \mathfrak{H}_a$.

Bergmann realized that this expression for the Hamiltonian could be obtained from an object $\int d^3x \left(\mathfrak{H}_L \epsilon^0 + \mathfrak{H}_a \epsilon^a\right)$ that generates variations of D-invariants corresponding to infinitesimal coordinate transformations – provided that these transformations involved a metric field dependence of the form

$$\xi^{\mu} = \delta^{\mu}_a \epsilon^a + n^{\mu} \epsilon^0, \tag{32}$$

since if one sets $\xi^{\mu} = \delta^{\mu}_0 \delta t$ in this expression it follows that $\epsilon^0 = N$ and $\epsilon^a = N^a$, and therefore the change in any variable η under time evolution is given by (31). As far as I am aware, Bergmann was the first to note and publish this observation.[47] The implications are of course profound, for as Bergmann showed, this dependence eliminated the higher time derivatives that appear in the standard Lie algebra, as discussed previously. Indeed, Bergmann derived the modified commutator of two infinitesimal transformations of the form (32), obtaining

$$\epsilon^{\rho} = \delta^{\rho}_a \left(\epsilon^a_{1,b} \epsilon^b_2 - \epsilon^a_{2,b} \epsilon^b_1\right) + e^{ab} \left(\epsilon^0_1 \epsilon^0_{2,b} - \epsilon^0_2 \epsilon^0_{1,b}\right) + n^{\rho} \left(\epsilon^a_2 \epsilon^0_{1,a} - \epsilon^a_1 \epsilon^0_{2,a}\right). \tag{33}$$

Note that no time derivatives of the descriptors appear in this expression. Thus the problem described in Sect. (2) with the original diffeomorphism Lie algebra is resolved. Strangely, Bergmann did not give the corresponding Poisson brackets of

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[46] In 1989, Bergmann (1989, 298), characterized Dirac's procedure as having "first appeared by magic." It is possible that he might have been inspired to employ the normal by Weiss's work (Weiss 1936) – which he nominally supervised. But I now doubt this. The Weiss construction employs the covariant normal. It does not depend on the metric. But most significantly his canonical momenta are simply the conjugates of the field derivatives with respect to the parameter time, and his formalism did not contemplate reparameterization covariance – the context in which a notion of D-invariance would arise.

[47] Bergmann (1962), equation (27.11).

the generators $\mathfrak{H}_L$ and $\mathfrak{H}_a$ although it is straightforward to derive them[48] and he must surely have known this bracket algebra which is now known as the Dirac algebra – even though Dirac apparently never published it. Higgs actually gives a partial result (Higgs 1958, 1959), but the offending bracket

$$\left\{\mathfrak{H}_L, \mathfrak{H}'_L\right\} = \int d^3x'' \mathfrak{H}_a(x'') e^{ab}(x'') \left(\delta^3(x - x'') + \delta^3(x' - x'')\right) \frac{\partial}{\partial x^b} \delta^3(x - x') \tag{34}$$

is missing. The first appearance in print I have found is in DeWitt (DeWitt 1967), equation (4.26a). The e^{ab} represents the inverse of the three-metric g_{ab}, and its appearance signifies that the group now constitutes exclusively a transformation group in phase space – even though every element can be associated with a specific four-dimensional general coordinate transformation, with none excluded.[49]

7 Conclusions

I have focused in this essay on the implications of the general covariance symmetry of Einstein's general theory of relativity as they were progressively investigated by Peter G. Bergmann and his collaborators at Syracuse University from 1949 to 1962. Bergmann believed that this underlying symmetry needed to be taken into account in the passage from the classical theory to an eventual quantum theory of gravity – in essence because it implied that spacetime coordinates could not of themselves carry physical information. In his mind the situation was similar to that in quantum electrodynamics where physical observables needed to be invariant under the action of the $U(1)$ gauge symmetry group. The situation in general relativity was of course enormously complicated by the fact that the gauge symmetry transformations of the metric field were themselves born of a corresponding transformation of spacetime coordinates – yet these very coordinates in the classical context not only served as identifying spatial labels but also tracked the field evolution in time. In fact, the underlying unity of classical spacetime would render this distinction between space and time labels as contrary to the principle of general relativity. Spatial field labels would under a four-dimensional diffeomorphism transform into combined spatial and temporal indicators.

We can discern a clear evolution in the Syracuse group's grasp and tentative resolution of the technical challenge they faced in attempting to implement this symmetry. It seemed natural that if the starting point was to be Einstein's theory one would need a procedure for reinterpreting the metric field and its spacetime derivatives as quantum operators. They first exploited identities that arose among classical field and velocity variables as a consequence of general covariance. The

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[48] He and Komar give the derivation later in Bergmann and Komar (1972).

[49] In Pons et al. (1997) we call it the "diffeomorphism-related group."

initial intent was to then perform a Legendre transformation to phase space and pursue a canonical quantization approach, to convert Poisson bracket relations to quantum commutators. The task was complicated by the appearance of primary constraining relations among these variables since they would lead to quantum inconsistencies, and there did not exist a procedure analogous to those employed in quantum electrodynamics that could be employed to eliminate them. It seemed preferable to work exclusively with variable functionals that were invariant under the action in phase space of the diffeomorphism group. An early attempt was made to formulate a quantum Schwinger-like action principle that would yield quantum commutators among field configuration and velocity operators, and its failure strengthened the case for first constructing classical invariants and then quantizing. The central objective was then to develop a phase space formalism that would incorporate general canonical transformations that might or might not correspond to a change in spacetime coordinates and in the process identify the gauge symmetry subgroup. Both invariant and general infinitesimal transformations were revealed in the classical temporal boundary terms that have served for finite-dimensional (non-field theoretic) systems as the foundation principle for Hamilton-Jacobi approaches. As had Rosenfeld much earlier (but for tetrad gravity), the Bergmann group showed that these latter vanishing terms would serve as the canonical generators of four-dimensional diffeomorphisms. The projection from configuration-velocity to phase space necessitated a procedure for finding the general solution of linear equations involving the singular Legendre matrix. The Bergmann school method differed mainly in outward appearance from that employed earlier by Rosenfeld. Dirac's roughly contemporaneous method was conceptually distinct and operationally somewhat simpler. And as we have seen Pirani and Schild succeeded in exploiting it to publish the first gravitational Hamiltonian – albeit using Bergmann's parameterized model. And Bergmann's student Penfield obtained the first non-parameterized Hamiltonian only shortly later. It should be stressed, however, that neither Pirani and Schild nor Dirac himself ever concerned themselves with the realization of diffeomorphism symmetry as a canonical transformation group.

We should also be cognizant of the fact that everything I have summarized so far occurred before the putative start of the Renaissance of general relativity in 1955 (see Chap. 1 in this volume). Major advances were made already in 1930, and one could contend that there did exist a degree of continuous progress in quantum gravity in the intervening period – at least in the period commencing in 1948.[50]

The next chapter in the Syracuse story concerns the construction of classical gravitational invariants. These invariants would satisfy a Poisson bracket algebra that would transform between physically distinct solutions of Einstein's equations. The numerical values of the invariants would indeed fix equivalence classes under the action of the diffeomorphism group, and the algebra could be understood as representing the factor group of the group of canonical transformations modulo

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[50] See Blum and Rickles (2018) for further documentation. See also Rickles (2020) for a magisterial analysis of this early period.

the group of diffeomorphisms. The Bergmann-Goldberg construction represented this algebra, but it essentially required knowledge of invariants. Newman made some progress in an iterative construction. Another alternative, explored by Janis, was to impose appropriate coordinate conditions. But perhaps the most attractive possibility – to me, at least – was proposed by Komar and then jointly investigated with Bergmann. It was to use the classical geometry itself to locate spacetime landmarks that could be used as intrinsic spacetime coordinates. All metric components relative to this intrinsic coordinate system would be invariants. Yet this possibility was not fully exploited, and I speculate that this might be related to Dirac's delineation of new gravitational variables in 1958. On the one hand, he argued that what we now know as the gravitational lapse and shift variables ought to be simply eliminated as canonical phase space variables since their evolution in time was arbitrary. And on the other hand, and perhaps even more importantly, in (Bergmann 1962) Bergmann interpreted Dirac's gravitational Hamiltonian as reflecting the fact that his diffeomorphism generator actually produced canonical variations that corresponded to compulsory metric-dependent diffeomorphisms – the decomposition of infinitesimal diffeomorphisms into true three-dimensional diffeomorphisms and metric-dependent diffeomorphisms in the direction perpendicular to the temporal foliation. Both of these advances brought into question whether the generator constructed by Anderson and Bergmann in 1951 actually preserved the full four-dimensional diffeomorphism symmetry. This may explain why in the late 1960s Bergmann and Komar began to look closely at a Hamilton-Jacobi approach to observables in general relativity – a chapter in the Syracuse saga that will appear soon (Salisbury 2020b). But they did continue to seek a group theoretical interpretation of Dirac's canonical generator, with the groundbreaking paper BK72 (Bergmann and Komar 1972). Referring to D58, Bergmann observed in 1979 that "At the time of the Dirac papers the nature of the commutators that he constructed was not entirely clear. Had Dirac merely discovered a new Lie algebra, or was his Lie algebra the germ of a group? If so, what was the nature of the group? In 1971 A. Komar and I were able to answer that question. There was a new group."[51] They showed that the generator was that of a transformation group of the metric variables – including the three-metric. This came about because of the appearance of the inverse three-metric e^{ab} in (33). In fact, as a consequence of the spatial derivatives – reflected also in the descriptor algebra (33) – derivatives of this metric arose at increasingly higher order with nested commutators. BK72 concluded that the metric dependence was spatially non-local. Bergmann stressed the compulsory metric dependence in a later essay (Bergmann 1979) in which he referred to the "fading world point." Each individual symmetry mapping, viewed as a functional of the three-metric, "sends a given point into a cloud of points, depending on the particular Cauchy data involved ... What this whole analysis may teach us is that the world point by itself possesses no physical reality."[52]

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[51] Bergmann and Komar (1972, 175).

[52] Bergmann and Komar (1972, 176).

We witness in this period in the early 1960s the emergence of a competing quantum gravitational formalism – Wheeler's geometrodynamics. He was inspired by the Feynman path integral approach in flat spacetime in which the action served as a quantum phase. In his vision a quantum transition amplitude between initial and final temporal states played a primary role. Although he did recognize that the theory must take into account the three-dimensional spatial diffeomorphism symmetry, he believed that the full four-dimensional symmetry was lost. It is replaced by what he called "multi-fingered time," a notion that first appears in print in his 1967 Battelle lectures (Wheeler 1968). There was a related substantial debate, beginning in 1960, involving Wheeler, his student Sharp, Misner, Bergmann, and Komar over the so-called thin sandwich conjecture in which Wheeler claimed that the three-geometry already carried information about time.[53] Arnowitt, Deser, and Misner (Arnowitt et al. 1962b) (ADM) of course invented their own first-order gravitational formalism in this same period and with a similar disavowal of four-dimensional covariance. Their view paralleled that of Dirac in this regard, and in fact their ultimate theory is equivalent to Dirac's, as interpreted by Bergmann, whereby the contemplated infinitesimal diffeomorphisms are understood as undergoing a perpendicular decomposition.[54] The upshot for all was that the fourth so-called scalar constraint needed of course to be imposed, but there was no clear relation to the original four-dimensional diffeomorphism symmetry. Rather, it acquired a "dynamical interpretation."[55] The reference is to Bryce DeWitt's suggestion in the early 1960s to Wheeler that the constraint could be implemented in a Hamilton-Jacobi formalism, in a form that is now known as the Wheeler-DeWitt equation. This formalism has since the 1960s overtaken the field, and the reasons for its dominance certainly merit a careful historical analysis. Granted, related to its presumptive correctness is the companion so-called problem of time which continues to occupy the minds of physicists and philosophers. I can perhaps not render an objective judgment as I and my collaborators carry on the Bergmann tradition, but in my opinion the term renaissance does not apply to quantum general relativity. Rather, what we encounter, in my opinion, is more analogous to the pre-Renaissance loss of classical scholarship.

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[53] See Salisbury (2020b) for a detailed analysis.

[54] Some authors do dispute this equivalence. See in particular (Kiriushcheva and Kuzmin 2011).

[55] Research status report for period December 1, 1964 to May 31, 1965, Bryce S. DeWitt Papers, Archives of American Mathematics, Dolph Briscoe Center for American History, University of Texas at Austin, Box 4RM235.

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Closing in on the Cosmos: Cosmology's Rebirth and the Rise of the Dark Matter Problem

Jaco de Swart

1 Introduction

In 1974, two landmark papers were published by independent research groups in the USA and Estonia that concluded on the existence of *missing mass*: a yet-unseen type of matter distributed throughout the universe whose presence could explain several problematic astronomical observations (Einasto et al. 1974a; Ostriker et al. 1974). The publication of these papers indicates the establishment of what is currently known as the "dark matter" problem – one of the most well-known anomalies in the prevailing cosmological model. According to this model, 85% of the universe's mass budget consists of dark matter. After four decades of multi-wavelength astronomical observations and high-energy particle physics experiments, the nature of this mass is yet to be determined.[1]

The origin of the dark matter problem serves more than only to illustrate the persistence of this fascinating anomaly. In this paper, I argue that the early justification for the existence of dark matter lays bare the foundation and formation of the contemporary discipline of cosmology. In the original 1974 papers, the two research groups put together a series of earlier published observations and interpreted them as signaling the presence of unseen mass. Crucially, both papers reflected on the cosmological significance of their conclusions: the tenfold-increased mass of the universe they had found, agreed with a decades-old cosmological model in which

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[1] For an overview of the physics, see, e.g., Bertone et al. (2005), Bertone (2010), and Bertone and Tait (2018).

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the universe is "geometrically closed." This model was "believed strongly" by physicists "for essentially nonexperimental reasons" (Ostriker et al. 1974, L1). Dark matter mattered because it could accommodate such a model.

The establishment of dark matter's existence is discussed in more detail in a previous paper (de Swart et al. 2017). In the current paper, I explore the significance of the above argument. Where did this "closed universe" come from, how can we understand its legitimacy, and what does it tell us about the practice of cosmology and its history? I argue that the reasoning which helped to establish the dark matter problem is closely entangled with the maturation of the discipline of cosmology, from the 1950s to the 1970s. Specifically, understanding the way that dark matter came to matter reveals the development of postwar cosmology's methodological character. It illuminates the two faces of this character that emerged in the 1970s: a fruitful hybrid of principle-based deductive and observational-based empirical approaches – two faces that still show in cosmological discussions today.[2]

This paper builds on many authoritative works that have been written on the history of cosmology in the second half of the twentieth century.[3] These works have particularly focused on the theoretical developments that brought about the emergence of the big bang cosmological paradigm in the early 1970s. The current paper, instead, centers on studying the changes in how cosmology was practiced. Where in the 1950s there was no unanimously celebrated theory, method, or practice that defined what a science of the universe looked like, by the early 1970s, there was an established theoretical and observational cosmological canon. I analyze this development by tracing the continuities and discontinuities in what it meant to do cosmology during this period.

My take on the establishment and foundations of modern cosmology centers around understanding the rise of dark matter as an inescapable part of its history and as an early exemplar of its methods. I argue that dark matter's establishment particularly lays bare the hybrid character of modern cosmology: the dark matter problem arises in the application of a combination of methodological strategies, in which the roots of both "rationalist" and "empiricist" approaches to cosmology reverberate. Dark matter's confluence of different methodological styles is traced back to two historical developments: the wake of general relativity theory's blooming development in the 1950s and the wealth of observations and institutional changes that remade the astronomical sciences in the 1960s.

The essay then is split up in four parts and has four different goals: (1) illustrate the history of cosmology's methodological foundations in the 1950s; (2) discuss the relation between cosmology and the revival of research on general relativity in the late 1950s; (3) indicate the fundamental conceptual and institutional changes in

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[2] Contributions to methodological discussions on inflation, string theory, and the multiverse often emphasize either empirical data or deductive thought. For this opposition, see, e.g., Ellis and Silk (2014).

[3] For broader overviews of the history of cosmology in the second half of the twentieth century, see in particular North (1965), Kragh (1996, 2006), Smeenk (2003), and Longair (2013).

the practice of astronomy during the 1960s, which signals cosmology's "rebirth"; and (4) argue that by the 1970s, instead of a single cosmological method appearing victorious, cosmology synthesized different scientific norms and practices, as shown by the rise of the dark matter problem.

2 The Foundations of Cosmology in the 1950s

Partially due to the fruits of postwar observational programs in astronomy, the early 1950s knew increased attention to cosmological issues. There were new estimates of the rate of expansion of the universe – the Hubble constant, H_0 – by Baade (1956) and Humason et al. (1956); radio astronomers agreed on the extragalactic nature of observed radio sources (Ryle 1956); and new catalogues of galaxies appeared from surveys of unprecedented size done by the Lick and Palomar observatories (Shane and Wirtanen 1954; Abell 1959). Although extragalactic scales started to be systematically explored (cf. Smith 2008), good and cosmologically relevant observations were still considered scarce and hard to obtain. Indeed, empirical novelties were only partially responsible for the increased attention to cosmological research in the 1950s. A large part of the increasing work and publications on cosmology in that period was due to a clash between two theories of the cosmos: relativistic cosmology and the steady-state theory of the universe. Due to this clash, cosmology as a science was under severe scrutiny during the 1950s.

Relativistic cosmology was the approach rooted in Einstein's 1917 effort to treat the entire universe with his relativistic field equations. Although Einstein initially introduced a static cosmological model, relativistic cosmology was believed to imply an evolutionary model of the universe from the 1930s onward. This new dynamical interpretation owed its existence to the observational work of Milton Humason and Edwin Hubble and theoretical developments initiated by Alexander Friedmann and Georges Lemaître. George Gamov had connected relativistic cosmology with nuclear physics in the 1940s, but, by the 1950s, relativistic cosmology was still mainly understood in terms of the Friedmann-Lemaître picture: as a model (or set of models) in which the dynamics of Einstein's field equations describe the expansion of the universe. This model was the early stage of what today is understood as the big bang theory.[4]

Criticism of the expanding relativistic model was hardly new, but it received a very concrete form in 1948 with the "steady-state theory." The theory was introduced by three Cambridge physicists, Hermann Bondi and Thomas Gold

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[4] Note that the idea of an expanding universe did not intrinsically involve the hypothesis of a cosmic origin or what was known as Lemaître's "primeval-atom" hypothesis. This idea was mainly celebrated by Lemaître and Gamov, but it was no integral part of relativistic cosmology at that time. Furthermore, note that the name "relativistic cosmology" is very much a convention. Some form of relativity was necessarily used in all cosmologies (cf. McCrea 1953, 350). For a detailed exposé on early relativistic cosmology, see especially Kragh (1996).

(1948) and Fred Hoyle (1948), as an attempt to resolve a discrepancy between the relativistic theory and observations. This discrepancy mainly consisted of a mismatch between the timescales of the relativistically predicted age of the universe and the much larger estimated age of the Earth.[5] Bondi, Gold, and Hoyle also had serious reservations about the unphysical and unscientific nature of the idea of a cosmic origin, which, although not unanimously celebrated by relativists, was implied by an expanding universe. Their alternative was a universe that on the large scale is steady and unchanging. It was infinite of age with a constant average density. This solved both the time-scale difficulty and the need for a big bang. The steady-state theorists notoriously hypothesized a "continuous creation of matter" to reconcile their ideas with a constant average density in an expanding universe. These ideas were met with skepticism by many relativists.

Steady-state theorists had challenged the physical foundations of relativistic cosmology. The resulting debate between these parties dominated cosmological practice in the 1950s. Just as Helge Kragh has emphasized, these debates show the fundamental philosophical character of cosmological practice in the 1950s.[6] At the same time, the explicit questioning of its foundation hints to cosmology's pre-paradigmatic stage as a discipline; there lacked an unanimously acclaimed theory, method, or practice that defined what a science of the cosmos looked like. I explore these debates to particularly lay bare the methodological positions that form the roots of current-day cosmology.

2.1 "Empiricists" and "Rationalists"

The very nature of cosmology as a proper science of the universe was at stake in the debate between steady-state theory and relativistic cosmology. How do you study the cosmos and what methods should guide this inquiry? Because of the severe friction between the groups of theorists, such philosophical questions started to be elaborately discussed in the 1950s. The different methodological positions in this debate were often divided into two camps by the involved theorists. Steady-state theorist Bondi wrote about "extrapolating" versus "deductive" attitudes in cosmology (Bondi 1952, 5), whereas relativist George McVittie identified them as "empiricist" and "rationalist" approaches (McVittie 1961a, 12). These two styles were argued to roughly coincide with the two theories of the cosmos: steady-state theory had a deductive approach where relativistic cosmology was based on

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[5] After the revision of the Hubble constant by Baade in 1952, the mentioned time-scale problem became less problematic for relativistic cosmology, although it did not fully disappear. See also Bondi (1952, 140).

[6] Helge Kragh has treated the details of these discussions in his 1996 book (Kragh 1996, 219–251). Much of the tensions of the methodological debates in 1950s cosmology are rooted in early debates from the 1930s. See, e.g., Gale and Urani (1999).

extrapolation of general relativity theory.[7] We will see that a strict division in approaches might not do historical justice to all theorists, but the dichotomy does highlight the prevailing major methodological tensions.

The split between "rationalist" and "empiricist" approaches chiefly concerned what primacy one would award to physical principles. In 1932, Edward Milne formulated an extension of Einstein's famed principle of relativity – the idea that all frames of reference know the same laws of nature. Milne's extended principle held that not only the laws of nature but the universe itself must appear to have the same structure to every observer (Milne 1932, 10). This idea has since been known as the "cosmological principle." In more contemporary terms, the principle holds that the matter distribution in the universe is homogeneous and isotropic on large scales.[8] The cosmological principle quickly turned into a central element of attempts to formulate theories for the structure of the universe. However, the prominence given to the principle in building and justifying these theories differed widely.[9] That is, should the principle serve as an addendum to theoretical exploration or does this principle form the very condition of possibility of a science of the universe?

For Bondi and Gold, the cosmological principle explicitly served the latter goal. In their 1948 paper, they reasoned that the cosmological principle is a logical necessity for the universe to be intelligible at all, as it protects the universal applicability of physical laws. Given that physical laws are tested locally, Bondi and Gold argued that there is no reason to assume that they could not differ throughout the universe. Then, in pursuing a science of the cosmos, one should provide grounds on which it can actually be assumed that the local physical laws hold everywhere the same. A system of cosmology should be principally concerned with this, Bondi and Gold wrote; it should be able to justify the "unrestricted repeatability of all experiments" (Bondi and Gold 1948, 252). For them, relativistic cosmology lacked a way to guarantee this unrestricted repeatability.

The steady-state theorists found the grounds on which to justify the universal application of known physical laws in what they called the "perfect cosmological principle" Bondi and Gold (1948, 254). Their extension of the cosmological principle said that the large-scale universe looks the same on every position in both space *and* time. To avoid any dependencies of physical laws, in future and past, one should not only assume that the universe looks the same everywhere but also that it is unchanging. Hence, as Bondi put it, one should "postulate that position in

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[7] Many authors used different terminology for these two styles. William McCrea, for example, wrote about "deductive" and "astrophysical" attitudes in cosmology (McCrea 1953, 332).

[8] As with many physical concepts, the exact formulation of the cosmological principle differed between authors. Dennis Sciama formulated the cosmological principle as "[e]ach particle always sees an isotropic distribution of particles around it" (Sciama 1960, 312). McVittie put it slightly differently: "[t]he development of the universe appears to be the same for each observer of an equivalent set, every one of whom assigns co-ordinates by the same method" (McVittie 1952, 96).

[9] In a review of cosmology in 1953, McCrea wrote: "All the theories to be discussed require their models to conform to the cosmological principle (CP), though we shall see that they do so for somewhat different reasons" (McCrea 1953, 326).

space and time is irrelevant" (Bondi 1952, 11). Without the perfect cosmological principle, "cosmology," they wrote, "is no longer a science" (Bondi and Gold 1948, 255). Because of their primacy of a priori principles, these steady-state theorists were identified as rationalist or deductivist.

For Hoyle's version of the steady-state theory, things worked a bit differently. He worked on a relativistic extension of Einstein's field equations, and did not share Bondi and Gold's worries about the validity of physical laws. Hoyle emphasized that formulations of these laws in terms of fields would mean that their validity is guaranteed everywhere. In a similar fashion, he disagreed about the role of the perfect cosmological principle. He remarked in 1949: "[i]t is believed that the wide [perfect] cosmological principle should follow as a consequence of primary axioms of the field form [. . .] and should not appear itself as a primary axiom" (Hoyle 1949, 371).

Despite Hoyle's different attitudes, it seems that we can still make sense of the fact that he was often grouped with Bondi and Gold in having a deductive approach. This comes from the fact that Hoyle's justification of a steady-state theory in 1948 hinged primarily upon the introduction of continuous creation of matter (Hoyle 1948). This initial assumption was often criticized on being an "arbitrary alteration" of the field equations (Heckmann, in Stoops 1958, 76). There was no experimental evidence for such an alteration, and hence it was argued there was no reason to abandon other fundamental principles (e.g., energy conservation) because of this idea. Hoyle, who was involved in studies of stellar nucleosynthesis, was no less concerned with observations than were the proponents of relativistic cosmology. His disagreement with relativistic cosmology, however, does seem to have been considered a matter of principle.

The claim of having a deductivist or rationalist approach to cosmology was not restricted to steady-state theorists. In the 1930s, Edward Milne's introduction of a kinematic relativistic world model – a description of the universe without a need for Einstein's theory – was found to be based on a hypothetico-deductive method. Lemaître even traced rationalist philosophical attitudes in cosmology back to Leibniz.[10] During the 1950s, however, this approach began to be discussed programmatically and more explicitly. This happened as steady-state theorists used their methodological convictions to defend their theory. In his 1952 textbook on cosmology, Bondi wrote that "it is a dangerous habit of the human mind to generalize and to extrapolate without noticing that it is doing so" (Bondi 1952, 6). Thomas Gold more specifically warned against uncritical extrapolation of known physical laws: "[n]o prejudices about physical principles must be used there when they would be based merely on the acquaintance with a much smaller scale of

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[10] See Milne (1935). In 1958 Lemaître stated: "As far as I can see, the inclination to rely on an a priori principle is related to Leibnitz [sic] philosophical attitude which made him to believe that there is some esthetical design in the Universe or even that the Universe is determined as being the best possible one" (Lemaitre 1958, 2).

the physical world" (Gold 1956, 1721). In these discussions, the dichotomy of approaches to cosmology started to take shape.

The methodology behind relativistic cosmology was less explicitly discussed; it was formulated mainly as a response to the steady-state theory. The cosmological principle was also less central in relativistic cosmology. The principle was used as an additional criterion for obtaining special solutions of the field equations, not as an axiomatic statement. Influential cosmologists in the 1950s advocating an approach to cosmology that opposed that of Bondi and Gold included McVittie and astronomer and philosopher Herbert Dingle. Especially the latter was fierce in his response to the steady-state theorists. In 1956, Dingle, former president of the Royal Astronomical Society, wrote the following: "[s]ome cosmologists have returned to the discredited practice of inventing arbitrary general principles, with no justification except that they seem 'right,' and fitting phenomena to the requirements of the principles" (Dingle 1956, 234).

Most relativists were similarly, but much less aggressively, in pursuit of an empirical methodology. German cosmologist Engelbert Schücking and astronomer Otto Heckmann, for example, found that "[a] theory constructed on a sound foundation of empirical data ought not to be discarded unless [. . .] new facts turn up that cannot be fitted into the framework of this theory" (Schucking and Heckmann 1958, 149). The theory to which Schücking and Heckmann refer here is Einstein's theory of general relativity. For relativistic cosmologists, the legitimacy of Einstein's theory was given priority over principles. When in the mid-1950s a revival of interest and trust in the potential of Einstein's theory commenced, this served as an offensive against the deductive approach.

3 Relativity's Renaissance, Cosmology, and Mach's Principle

Although the flourishing modern era of gravitational-wave astronomy and black hole physics would seem to suggest otherwise, research on the physics of gravity has been waxing and waning throughout the twentieth century. Whereas the subject bloomed shortly after Einstein's introduction of the theory of general relativity, historian Jean Eisenstaedt has shown that from the mid-1920s to the 1950s, gravitational physics knew a low-water-mark period of stagnated research activity (Eisenstaedt 1986, 1987, 1989). Only in the mid-1950s did the tide turn for gravity's relative weight in physics research. During a period characterized by community formation and the recognition of the general theory of relativity's untapped physical potential, an integrated research field of gravitational physics arose. Physicist Clifford Will dubbed this period the "renaissance of general relativity" (Will 1986, 1989).

Research on gravity during the renaissance period knew an enormous shift to experimental and observational issues in relativity. These issues included, for example, the possible measurement of gravitational waves.[11] With this experimental focus, and in combination with the usage of new mathematical tools, the renaissance period knew a vast exploration of general relativity as the fundamental theory of gravity. These developments during general relativity's renaissance have been elaborately discussed by Blum et al. (2015, 2016, 2017), and I will follow their heuristic periodization.[12] What has been discussed in lesser extent is how general relativity's developments reverberated in cosmological research. The "global transformation in the character of [general relativity]" that Blum et al. have addressed (Blum et al. 2017, 98) further implied a transformation in the relationship between cosmology and relativity. In particular, the increased authority of the general relativity theory during its renaissance emphasized a relativity-based extrapolating attitude to cosmological research.

Most steady-state theorists were trained relativists. They joined in meetings on gravitational physics, and brought the discussion on how to approach cosmology to the attention of gravity scholars. One of the most influential scholars in the new field of gravitational physics that responded to this issue was Princeton professor John Wheeler. During one of relativity's renaissance famous meetings – the 1957 Chapel Hill conference – he clearly expressed his view on how to approach cosmology. He commented on the work of steady-state theorist Thomas Gold by stating that "one should not give up accepted ideas of wide applicability such as general relativity but should investigate them completely" (Wheeler, in Rickles and DeWitt-Morette 2011, 129). Wheeler's take on cosmology was the relativistic view that started to dominate the discussions during the renaissance of general relativity: an approach to cosmology that centered around the extrapolation of general relativity.

This view became even more ingrained as confidence in the potential of the theory of general relativity kept increasing. By the early 1960s, the general opinion was that general relativity was fundamentally true and that this needed little debate. Venerable physicist and Princeton colleague of Wheeler, Robert Dicke, wrote that "[specialists] take it as axiomatic that general relativity is correct in all its details and that one must compute with this theory" (Dicke 1964, 1). That general relativity was axiomatic also held for how cosmology was perceived to be done. In 1962, Wheeler reflected on the transformation that the status of general relativity had undergone:

> Increasing numbers of investigators share the conviction that Einstein's 1915–1916 analysis of the curvature of space by energy is a unique theory, of unrivalled scope and reasonableness, against which no objection of principle or observation has ever been sustained, and out of which one should now try to read the deeper meaning and consequences. Among these consequences some of the most interesting have to do with the dynamics of the universe. (Wheeler 1962, 40)

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[11] See Peebles (2017) for an elaborate historical overview of the history of experimental gravitational physics. For more on the history of gravitational waves during that period, see Blum et al. (2018).

[12] See also, e.g., Eisenstaedt (2006) and Lalli (2017).

Between about 1955 and 1963, the relation between cosmology and relativity transformed. Where Einstein's former research assistant Peter Bergmann wrote in 1957 that cosmology was "not intimately connected" with other aspects of relativity (Bergmann 1957, 352), this sentiment had changed by the early 1960s. General relativity was supplying a new "rationale" for practicing cosmology, as Wheeler put it (Wheeler 1962, 75). The increasing confidence in the theory of relativity made cosmological research relevant and urgent: cosmology became the very consequence and testing grounds for the theory of general relativity.

General relativity as a rationale for cosmology meant that, in the early 1960s, the climate changed around the steady-state versus big bang theory debate. As the steady-state theory indirectly challenged general relativity through its dismissal of relativistic cosmology, the latter began to be the more favored one. Helge Kragh similarly reflected that relativistic cosmology's increasing status is "probably related to the simultaneous revival of interest in the general theory of relativity" (Kragh 1996, 318). He also pointed out that Hoyle's, McCrea's, and later versions of the steady-state theory were being designed to conform to the mathematics of general relativity through Einstein's field equations, which again emphasizes the gravitational theory's increasing authority (Kragh 1999, 398).

The "empiricist" approach became the dominant way to address the method of cosmology. But how "empirical" was this approach really? According to McVittie, avid defender of relativistic cosmology, the general relativity point of view was "that scientific cosmology should be based on the laws of physics as we know them from experiment and observation rather than on hypotheses and principles laid down a priori" (McVittie 1961b, 1232). However, as I hope will be clear, one could be quite skeptical of McVittie's remarks on the method of relativistic cosmology. That is, the less empirical and more *philosophical* foundations of Einstein's theory itself also kept lingering within cosmology.

3.1 The Mach Connection

A sharp reader might have already been aware of the perhaps confusing identification of relativistic cosmology as "empirical" in approach. The confusion comes from the fact that general relativity theory's methodological underpinning is much more elusive.[13] "[T]he origins of General Relativity are mainly philosophical rather than observational," Robert Dicke reflected in 1961 (Dicke 1962, 4). One of these philosophical foundations has been specifically recognized: the idea known as Mach's principle. This principle was a persistent subject in discussions on general relativity when it accompanied the rebirth of interest in the theory during the

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[13] For a comprehensive overview of the genesis of general relativity, see Renn (2007a). For an in-depth discussion of Einstein's methodology and its development, see van Dongen (2010).

1950s.[14] Understanding the role of this principle helps to understand the ambiguous methodological character of cosmological practice, both in relativistic and steady-state cosmology.

Ernst Mach's famed principle is reminiscent of his elaborate critique on Newton's absolute notions of space and motion. Opposing Newton, Mach had argued that mechanics is not based on absolute but on the relative position and movement of bodies. This relationist view had far-reaching implications for Mach's notion of inertia. Because there is no absolute rotation, he argued that an observer on the surface of a sphere should notice no physical difference in either having the sphere rotates with respect to distant objects or having distant objects rotate around the sphere. However, we know that on the surface of a rotating sphere (e.g., the earth), one experiences inertial forces like that of the centrifugal force. With Mach's insights, "the principles of mechanics" could be so understood "that even for relative rotations centrifugal forces arise" (Mach 1960, 284). Because of this reasoning, Mach suggested there exists a relationship between local inertial forces and the distant celestial bodies. This is what has come to be known as Mach's principle.[15]

Einstein was profoundly influenced by Mach's ideas on inertia; for a long time, he was concerned with having a theory of gravity that fully abides by Mach's mandate.[16] As relativity revived in the 1950s, so did these concerns. The principle came to have a wider interpretation, and more generally was understood as the idea that the local inertial frame depends on, or, in a stronger form, is determined by the mass distribution of the universe. In this sense, the adjective "Machian" was used for any connection between local dynamics and the structure of the universe.[17] During the renaissance of general relativity, many august physicists took Mach's principle to be fundamental to gravity theories. Much research was done on how to make Einstein's field equations come in full accord with the principle and whether one could think of a true Machian theory of gravity.

Dennis Sciama, who is often recognized as one of the fathers of modern cosmology, was central in the revival of work on Mach's principle. His doctoral work under the supervision of Paul Dirac resulted in a novel take on the "origin of inertia," in which he proposed a type of long-range interaction with distant matter (Sciama 1953). Mach's principle was put high on the list of the newly developing research agenda of gravitational physics. Referring to Sciama's work, Wheeler orated at the Chapel Hill conference in 1957 that one of general relativity's

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[14] Many elaborate studies have been done on Mach's principle, its general importance, and its role in the theory of general relativity. See specifically Barbour and Pfister (1995). The writing of a longue durée history of the principle seems to have not yet been attempted.

[15] Einstein was the first who had formulated Mach's ideas on inertia as a "principle" (Einstein 1918, 16).

[16] For the role of Mach's principle in the development of general relativity, see, e.g., Hoefer (1994), Renn (2007b), Barbour (2007), Lehmkuhl (2014), and Janssen (2014).

[17] There were many different and more technical definitions of Machian ideas. See Goenner (1970) and references therein for examples of the use of "Mach's principle" and "Machian" in the 1950s and 1960s. For a pre-1950s definition of Mach's principle, see, e.g., Robertson (1933).

important open problems was “spelling out Mach’s principle in a better-defined way” (Wheeler 2011, 46). Robert Dicke was similarly concerned with Mach’s ideas. He wrote about its importance on many occasions in the late 1950s and early 1960s and was influential in discussing experiments on testing Mach’s principle through possible mass anisotropies in the universe.[18] In 1961, he and Carl Brans proposed what came to be known the Brans-Dicke theory of gravitation, to come into accordance with Machian ideas (Brans and Dicke 1961).

In cosmology too, Mach’s principle was of great importance during the 1950s. It served as a foundation for both steady-state and relativistic cosmology. Mach’s principle formed a central motivation for the steady-state theorists’ “perfect cosmological principle.” That is, when there exists a Machian connection between the cosmic distribution of mass and local inertia, a different mass distribution in the earlier universe might imply different local laws of physics. The steady-state theorists tried to avoid such a possibility with their reasoning.[19]

Relativistic cosmology’s relation with Mach’s principle had deeper and more complex historical ties. In Einstein’s cosmological work of 1917, he again emphasized that inertia should follow from a body’s gravitational interaction with all the other masses in the universe. In any model of the universe, he hence reasoned, a mass at sufficient distance from all the other masses should have its inertia fall to zero. Einstein tried different ways of implementing this Machian condition. He found it most satisfactory to use it as a selection criterion: the relativistic model that can realize the Machian condition is the correct one. For Einstein, this model was a universe that is spatially closed.[20] Einstein later more specifically reflected on his epistemological preference for a closed universe:

> [T]his idea of Mach’s corresponds only to a finite universe, bounded in space [. . .]. From the standpoint of epistemology it is more satisfying to have the mechanical properties of space completely determined by matter, and this is the case in a closed universe. (Einstein 1922, 108)

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[18] See, e.g., Dicke (1959, 1962) and for experimental test also Cocconi and Salpeter (1960) and Dicke (1961). For a historical overview of Dicke’s important work, see Peebles (2017).

[19] For example, Bondi wrote that “[f]or in any theory which contemplates a changing universe, explicit and implicit assumptions must be made about the interactions between distant matter and local physical laws. These assumptions are necessarily of a highly arbitrary nature, and progress on such a basis can only be indefinite and uncertain. [. . .] If the uniformity of the universe is sufficiently great none of these difficulties arise” (Bondi 1952, 12).

[20] As both Chris Smeenk (2014) and Carl Hoefer (1995) have clearly spelled out, Einstein first tried to use Mach’s statement as a boundary condition to the field equations. Later, in his 1917 cosmology paper, he had turned away from this perspective. Instead, he used the fact that a spatially closed universe has no boundary region. Einstein noted: “[f]or if it were possible to regard the universe as a continuum which is finite (closed) with respect to its spatial dimensions, we should have no need at all of any such boundary conditions” (Einstein 1987, 427). For an in-depth discussion of Einstein’s 1917 paper, see O’Raifeartaigh et al. (2017).

Whether or not adding the assumption of a closed universe would make general relativity truly Machian was, and still is, disputed.[21] What cannot be disputed is that the preference for a closed universe became widespread. Having a universe that is closed was even seen as an integral part of Einstein's theory of relativity. In 1958, Wheeler considered the following to be the meaning of the term "Einstein's theory":

> In speaking about Einstein's theory [. . .] we mean not only the system of differential equations associated with his name, but also two further points, the present tentative arguments for which he gives in his book [*The Meaning of Relativity* (1922)]:
> (1) The universe is closed;
> (2) No "cosmological" term is to be added to the field equations. (Wheeler 1958, 98)

Although by the early 1960s general relativity appeared "victorious" in cosmology, as Kragh has put it (Kragh 1999, 398), this did not mean the empiricist approach had swept away every a priori principle or rationalistic tendency. Given that general relativity was a "theory of principle" by its very origin, there was, on a fundamental level, no approach that can be regarded as a clear-cut winner in cosmological methodology. On the surface, general relativity was applied cosmologically in an empiricist manner, but, underneath, relativity had many "rationalistic" features that echoed through in cosmology. These features were decreasingly explicit from the mid-1960s onward. From that moment, the efforts of relativity, astrophysics, and cosmology became dominated by newly observed phenomena.

4 The Golden Age: A Cosmological Turn

The gained trust in the theory of general relativity manifested itself in what Kip Thorne (1994) has called a "golden age" for relativity: from 1963 onward, the theory became largely celebrated because of its applications to astronomy and astrophysics. This golden age got much of its stature due to a wealth of newly observed astronomical phenomena and immense conceptual changes in the understanding of the universe's structure and contents. During this period, research on cosmology again became re-characterized. It became less driven by relativists and was increasingly integrated into the interests of astronomers and astrophysicists, which stressed an observational research program in cosmology (see also Chap. 1 in this volume). I will give three examples of these changes of interests.

Firstly, as Robert Smith (2008) has also argued, astronomy increasingly shifted toward extragalactic phenomena. New catalogues of galaxies based on the surveys of the Palomar and Lick observatories in the late 1950s made it possible to better understand the large-scale distribution of galaxies.[22] This included, for example, the

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[21] For more information on whether general relativity is Machian, see e.g., Dicke (1962) and more generally (Barbour and Pfister 1995) and references found in footnote 16.

[22] These catalogues include Zwicky et al. (1961), Vorontsov-Velyaminov et al. (1962), and Arp (1966).

conclusions that *clusters* of galaxies were a fundamental part of this distribution.[23] The advent of radio astronomy in the 1950s had similarly influenced extragalactic astronomy; it opened up a wavelength region that spanned more than a factor of 1000. The Cambridge C3 radio catalogues supplied astronomers with a wealth of new data that led to the observation of unidentified phenomena in galaxies and the universe: radio luminosities that were orders of magnitude larger than their optical counterparts and objects like double radio sources, radio stars, and radio galaxies.[24]

The great postwar sky surveys were accompanied with new instrumentation that similarly influenced the turn to extragalactic astronomy. In 1961, Hubble's successor at Mt. Wilson observatory, Allan Sandage, had published an optimistic piece on how the 200-inch Hale telescope, that first saw light in 1949, could potentially discriminate between different cosmological models. He wrote: "[r]enewed interest in the cosmological problem is evidenced by the number of recent papers which treat the fitting of observational data to predictions of the theory" (Sandage 1961, 356). Sandage's analysis of possible cosmological tests and available observations revitalized a program in observational cosmology.

The second illustration of the golden age's conceptual changes is how relativity became a genuine subject of concern for astronomers and astrophysicists. This was connected to an observation that was particularly influential in making the 1960s a golden age for general relativity: the first observation of a quasar by Maarten Schmidt in 1963. The brightness of this radio object was 100 times larger than any known galaxy, implying an explosive energy release. The unknown nature of this quasar acted as a boundary problem that brought astronomy and gravitational physics close together.

As an immediate response to the quasar discovery, the first of a very successful series of "Texas Symposia on Relativistic Astrophysics" was held in Dallas, Texas. Here astronomers, astrophysicists, cosmologists, gravitational physicists, and nuclear physicists participated to discuss the nature of these quasars. The main motivation for the symposium was indeed the energy related to the quasars: it was calculated to be more than 10^{60} ergs, which is the energy contained in the rest mass of a million solar masses (cf. Sciama 1971a, 61). The theoretical requirements needed for an outburst of such enormous energy had "so far ruled out nearly all of the explanations and theories put forward to explain such extraordinary events," the invitation to the symposium read (Robinson et al. 1965, v).

Gravitational physicists were involved in searching for a mechanism of gravitational collapse that might cause such outbursts of energy. Or, as John Wheeler noted at the Texas symposium, "attention has turned to gravitational collapse as

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[23] In 1957, George Abell wrote that "[p]rior to 1949, only a few dozen clusters were known. [...] In recent years, however, two independent photographic programs have indicated that clusters of galaxies are far more numerous than was formerly thought, and that indeed they may be fundamental condensations of matter in the universe" (Abell 1957, 3).

[24] For the radio catalogues, see Edge et al. (1959) and Bennett and Smith (1962). See also (Sciama 1971a, 49–82).

a mechanism by which in principle a fraction of the latent energy of matter much larger than 1 percent can be set free" (Harrison et al. 1965, 1). Proposed mechanisms included the formation of objects that we now understand as black holes and neutron stars. The conference illustrates that the quasar observation had indeed brought physicists and astronomers very close together. At the dinner of the first Texas Symposium, Thomas Gold, of steady-state fame, made this quite clear: "[e]veryone is pleased, the relativists [. . .] who are suddenly experts in a field they hardly knew existed; the astrophysicists for having enlarged their domain, their empire, by the annexation of another subject – general relativity" (Gold 1965, 470).

The third point that demonstrates changing concerns in the 1960s was the increasing focus on the origin and evolution of the universe. Quasars were the oldest objects astronomers had observed, and because of their age, they started to be used to track the evolution of galaxies. Their age gave insight in the early evolutionary state of galaxies and how galactic properties change with time. In the 1960s, the topic of galaxy evolution began to be explored. "The study of evolution of galaxies is now in an early stage of development comparable to that of the study of stellar evolution in 1935," Thornton Page reflected in 1964 (Page 1964, 804). Better knowledge of galactic evolution meant that galaxies could be used as probes for the evolution of kinematic and gravitational properties of the universe. The evolutionary picture of galaxies emphasized what the study of galaxies could mean for cosmology; the initial conditions of galaxies are closely tied to the contents and evolution of the universe.

The above examples are far from a complete overview of the flood of phenomena that entered astronomical research in the 1960s. Also included in this list are the observation of the cosmic microwave background in 1964 and the first pulsar observation in 1967. Both these observations had an enormous theoretical impact, but a detailed discussion of this impact would go beyond the purpose of this paper. The given examples suffice to show how the focus of astronomers and astrophysicists was rapidly changing in the 1960s. The flood of observations had swamped the earlier philosophical reflections, in favor of a "renaissance in observational cosmology," as Dennis Sciama called it (Sciama 1971b). Indeed, in 1970, the National Research Council reported to US Congress that "the rapid pace of discovery in astronomy and astrophysics during the last few years has given this field an excitement unsurpassed in any other area of the physical sciences" (National Research Council 1972, 55).

The dominance of observations in the 1960s redetermined the objects of interest for both physicists and astronomers and blurred the boundaries between physics and astronomy. This is also illustrated by the manpower distribution in astronomy.

4.1 Manpower and Textbooks

The renewed interest of astronomers and physicists was accompanied by large transformations in the institutional landscape of astronomy in the 1960s. Specifically, physicists were starting to flood astronomy in the late 1960s and early 1970s. By the early 1970s, astronomy had many more and very different practitioners compared to earlier decades.

In the USA, the 1960s knew an enormous increase in astronomy graduate students, quite probably related to the explosive demands of the space program. The number of degrees awarded in astronomy had increased tenfold by 1970, compared to a decade earlier. The number of awarded astronomy degrees grew at an accelerated rate: where between 1920 and 1960 the number of astronomy Ph.D. degrees awarded grew around 4% every year, between 1960 and 1970 this rose to a 20% annual increase. Not only the number of students increased. The total number of personnel employed in the field of astronomy, both technical and scientific, almost tripled in the 1960s.[25]

Besides an absolute increase in manpower, the background of people that worked in astronomy also changed. The field became increasingly dominated by physicists. While in 1966 26 percent of the astronomy personnel with Ph.D.s had received their doctorates in physics, by 1970 this had increased to 45 percent. In 1970 it was projected that it took only two more years until there were more people with Ph.D.s in physics working in astronomy, than people with Ph.D.s in astronomy.[26] Although the funding channels were stagnating in astronomy, the discipline was doing relatively well compared to physics in the early 1970s; in 1971 the unemployment rate of Ph.D.s in physics was more than four times as high as in astronomy.[27] This could explain part of the large influx of physicists into astronomy.

Indeed, the institutional character of astronomy had changed by the early 1970s, and physics seemed to have taken a larger place within the discipline. At the same time, the subject of cosmology was becoming increasingly popular. The use of "cosmologist" as profession began to circulate in the 1960s (Kragh 2006, 200), and the number of publications on cosmology increased with an order of magnitude between 1965 and 1975 (de Swart et al. 2017, 4). Furthermore, with the increasing number of students, many new textbooks on cosmology appeared in the early 1970s: Jim Peebles' "Physical Cosmology" (1971); Dennis Sciama's "Modern Cosmology" (1971); Weinberg's "Gravitation and Cosmology" (1972); Hawking and Ellis' "The Large Scale Structure of Space-Time" (1973); and Misner, Thorne, and Wheeler's "Gravitation" (1973).[28] These books again show the boundary-crossing character of cosmology: every single author of these books was a physicist by training, and many of them discuss astronomical observations, cosmological theory, and gravitational physics.

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[25] National Research Council (1973, 327).

[26] National Research Council (1972, 55–56); National Research Council (1973, 332).

[27] National Research Council (1973, 337). David Kaiser (2002) has in great detail written about the case of manpower in American physics after World War II.

[28] More examples of textbooks are Robertson and Noonan's "Relativity and Cosmology" (1968), Wolfgang Rindler's "Essential Relativity: Special, General, and Cosmological" (1969), and Zeldovich and Novikov's "Relativistic Astrophysics: Stars and Relativity" (1971).

Where the mid-1960s was dominated by the renaissance of observational cosmology, in the early 1970s, perhaps with the influence of physicists, theoretical cosmology also flourished. Backed by radio star counts and the cosmic microwave background, relativistic cosmology had become the canon cosmological theory, in which different models could be explored. There was a practical way in which theoretical cosmology also started to flourish. Between 1967 and 1970, 32% of the astronomer Ph.D. recipients found that their research was restricted by the lack of availability of observing time on local and national telescopes.[29] While observational capacities were lacking, theoretical possibilities were widely available. By 1970, "theoretical astrophysics" was the research subject that enjoyed most the interest of professional astronomers, according to the National Research Council.[30]

The boundary between physics and astronomy was blurring, and astronomical research underwent a cosmological turn: research was primed toward the study of the universe.[31] Cosmological research formed a hybrid environment that blended extragalactic astronomy, nuclear physics, astrophysics, and gravitation physics. This new *physical* cosmology, as it was sometimes referred to, was accommodated by astronomy's increased institutional scale and inflow of physicists. This again had consequences for how cosmology was perceived to be done.

5 Birth and Rebirth: Cosmology and the Dark Matter Problem

By the 1970s, the status of cosmological research had radically transformed. Cosmology now was deeply driven by observations and had become a commonplace subject that crossed the boundaries between physics and astronomy. Following Merleau-Ponty and Morando (1976), it seems we can justly signify this transformation as a "Rebirth of Cosmology."[32] Quite similar to what happened in the 1950s to general relativity – as a theory, and as a field of research – cosmology had become a respectable part of the physical sciences.

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[29] National Research Council (1973, 349).

[30] National Research Council (1972, 60).

[31] This notion is related, although distinct, from what Blum et al. call "the astrophysical turn of general relativity" (Blum et al. 2018, 8). They introduce this term to signify the refocusing of the research agendas of relativists because of the astronomical discoveries of the 1960s. What I try to describe here with a "cosmological turn" is aimed to be more inclusive: it is the change during the 1960s, in which *astronomical* practices more generally – and not just relativity scholars – turned toward understanding the structure and evolution of the universe.

[32] Merleau-Ponty and Morando (1976) seem to use two interpretations of "The Rebirth of Cosmology," the title of their book: either as one of both cosmological revolutions instigated by Newton and Einstein or in the sense of the narrower period in which cosmology had transformed to be the frontier of science by 1976. I use it in the latter sense.

Cosmology's rebirth is most visible in how its status as a "proper" subject of study became undisputed. The achievements of cosmology, "especially in the last few years," Dennis Sciama wrote in 1971, "constitute a revolution in our knowledge and understanding of the Universe with no parallel in the whole recorded history of mankind" (Sciama 1971a, vii). Where, in the 1950s, renowned radio astronomer Martin Ryle had been notoriously skeptical of the whole cosmological enterprise,[33] he was awarded the Nobel Prize in 1974 for his radio-astronomical work and invention of the aperture synthesis technique, which had been of "crucial significance" for cosmology.[34] It was the first time the prestigious prize was awarded for astronomical research. Ryle's student, Malcolm Longair, portrayed the shift in the status of cosmology:

> Because of the increased confidence in the hot [Big Bang] model and the larger number of real facts about the universe, topics which in the past were questions of pure speculation have become susceptible to detailed quantitative analyses which can be checked against the observations. (Longair 1971, 1126)

The speculative days were over, and little traces were left of the great philosophical discussions of the 1950s. Early textbooks like that of McVittie (1956) had vividly discussed the nature of scientific laws, and Bondi (1952) devoted whole sections to the cosmological principle, the problem of inertia, and the differences between physics and cosmology. Textbooks of the early 1970s avoided such elaborate philosophy. Instead, the observational potential of cosmology was celebrated, and textbooks and monographs exhibited a shift to an implicit but familiar "empiricist" style of reasoning. As Steven Hawking and George Ellis, both students of Sciama, wrote in their 1973 textbook: "we shall take the local laws of physics that have been experimentally determined, and shall see what these laws imply about the large scale structure of the universe" (Hawking and Ellis 1973, 1).

Another example of the overwhelming dominance of the "empiricist" approach is Dennis Sciama's celebrated oeuvre on cosmology. While in the 1950s Sciama wrote a popular book that was themed around Mach's principle – titled *The Unity of the Universe* (Sciama 1959) – his later book barely mentions Mach. Instead, he emphasized that "[t]he first question must be: can [the great flood of new observations] be understood in terms of the known laws of physics?" (Sciama 1971a, 101). Mach's principle was considered in great extent by pre-1960s textbooks, but publications from the 1970s had mostly left these discussions aside.

The new generation of relativists and cosmologists were barely exposed to the philosophical underpinnings of cosmology. The tendency among the new pupils in gravitation and cosmology was clear: the phenomena came first, not the principles. Yet the vivid methodological discussions of the 1950s did have their repercussions

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[33] In the 1950s Ryle noted "[c]osmologists always lived in a happy state of being able to postulate theories which had no chance of being disproved [. . .]" (Ryle quoted in Kragh 1996, 309).

[34] "Press Release: The 1974 Nobel Prize in Physics." https://www.nobelprize.org/prizes/physics/1974/press-release/. Accessed 30 Jan 2018. For more on the curious relationship between the Nobel Prize and the astronomical sciences, see Kragh (2017).

in cosmological research of the 1970s. Similar to how cosmology became a hybrid environment of astronomers and physicists, a fruitful hybrid of approaches to cosmology came into use. Stylistic remnants of deductive approaches can be recognized in the a priori preferences that existed for certain cosmological models.

5.1 A Closed Universe

In the reborn science of the cosmos, there were many different models in which the theory of the explosive universe could be realized. From the Friedmann equations followed three different cases for the evolution of the universe. These were given by the value of the curvature of the universe: negative (spatially open), positive (spatially closed), or a special model which had no intrinsic curvature (spatially "flat"). This spatial curvature was indirectly measurable, and in the early 1970s, there was optimism about being able to distinguish between these models observationally.

"[B]eginning in the 1960s a flood of new discoveries has enriched our picture of the universe and has begun to provide a basis on which to distinguish between competing cosmological models," Allen Sandage wrote in 1970 (Sandage 1970, 34). For him, cosmology was a "search for two numbers": the Hubble constant and the deceleration parameter. The latter, in practice, was directly related to the mass density of the universe.[35] This parameter became a central research subject in cosmology and extragalactic astronomy. The density of the universe relates to its destiny, and measuring it could determine which of the cosmological models corresponds to the universe: open or closed. The density was often expressed in terms of Ω, the density relative to the "critical density": $\Omega = \rho/\rho_{critical}$. The critical density ($\Omega = 1$) was the density of a universe that was "flat," which was a model introduced by Einstein and de Sitter (1932). Less mass would mean an open universe ($\Omega < 1$); more mass would mean a spatially closed universe ($\Omega > 1$).

Although Sandage, as an observer, indeed emphasized that observations will determine which model is correct, preconceptions about the shape of the universe still lingered. As discussed in Sect. 3, Mach's principle had left a strong imprint on the way relativistic cosmology was done. The principle was "conceived as the requirement that the universe be closed," as Wheeler had put it just before the "Golden Age" of relativity took off (Wheeler 1962, 74). Nearing the end of the 1960s, a closed universe was still a much-preferred model for many cosmologists and relativists. "*Philosophically*, there might be a preference," Wolfgang Rindler wrote in 1967, "the choice $k = 1$ [a positively curved universe] might appear desirable. It implies closed space sections that would, in some sense, validate Mach's principle according to which the totality of matter in the universe and

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[35] In a universe without a cosmological constant, q_0 and ρ are directly related by the equation $\rho/\rho_c = 2q_0$, with ρ_c the critical density.

nothing else determines the local inertial frames" (Rindler 1967, 29–30, emphasis in original). Dennis Sciama had a different preference but similar reasoning for preferring a universe with a specific density:

> [...] the Einstein-de Sitter model is the one where the total energy of the universe is zero, the kinetic energy and the negative gravitational potential energy just balancing. Well, if you think that kinetic energy manifesting inertia is due to gravitation, then you might intuit that the most Machian way of having one made by the other would be if there's equal amount of energy, which would give you uniquely the Einstein-de Sitter model, I still have a secret hope that that might turn out so, but it may well not. (Sciama, in Weart 1978)

The implicit preference for a closed universe was also expressed in observational studies. "One would particularly like to know whether there is enough mass to close the universe," Princeton physicists Peebles and Partridge wrote in 1967, in a piece on estimating the mass density of the universe (Peebles and Partridge 1967, 713). However, there was a crucial discrepancy between such observations and a spatially closed cosmological model: the measured value of the mass density of the universe was typically around the order of 10^{-31} gr. cm^{-3}, two orders of magnitude lower than the mass needed to close the universe.[36] In his 1972 textbook, Steven Weinberg wrote:

> [I]f one tentatively accepts the result that q_0 is of order unity [$\Omega \geq 1$], then one is forced to the conclusion that the mass density of about $2 \times 10^{-29} g/cm^3$ must be found somewhere outside the normal galaxies. But where? (Weinberg 1972, 478)

The idea of a closed universe still found resonance with the older generation of relativists and cosmologists, like John Wheeler (1974). But also the newer generation of cosmologists worried about the implication of this "theological" idea that the universe ought to be closed:[37]

> Where [can] the missing mass be hiding if it is demanded, on theological or other grounds that $\Omega \geq 1$ [$\rho \geq \rho_c$]. (Gott et al. 1974, 550)

The discrepancy between the observed mass density and the density needed for a closed universe made that a new problem appeared: the "problem of the so-called 'missing mass'," as Geoffrey Burbidge called it, the mass missing to close the universe (Burbidge 1972, 493). Such mass would ultimately vindicate Mach's principle that was so vigorously pursued in earlier decades, but this reason behind preferring a closed universe did seldom enter discussions in the 1970s. Although its origin was little regarded, the philosophical preference for extra mass did have real implications for astrophysical and cosmological observational programs. Research was set to uncover potential yet-unseen intergalactic matter and to explore how

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[36] See, e.g., Oort (1958), Peebles (1971), Shapiro (1971), Noonan (1971), Weinberg (1972, 478), and Burbidge (1972, 493). The critical density is $\rho_c = \frac{3H^2}{8\pi G} \sim 10^{-29}$ gr. cm^{-3}.

[37] J. Richard Gott received a Ph.D. in 1973, Jim Gunn in 1966, David Schramm in 1971, and Beatrice Tinsley in 1966.

to theoretically accommodate such a finding.[38] An answer to the demand for extra mass came in 1974, from two independent collaborations of astronomers and physicists.

5.2 The Birth of Dark Matter

Although cosmology had received the status of an empirical science in the early 1970s, there still lingered part of a "rationalist" character in its practice. To have a closed universe – the cosmological model initially inspired by Ernst Mach – would mean a density much higher than was determined by observations of the luminosity of stars and galaxies. The dark matter problem finds its birth in this context, where two observations were recognized as indicating the existence of this "missing mass."

The first of these observations concern the dynamics of galaxies. Much of the wealth of data on galaxies that was acquired in the 1960s and early 1970s was related to radio astronomy. Radio-astronomical studies of galaxies were done in great number, with new telescopes like the Owens Valley Radio Observatory interferometer and the Westerbork Radio Synthesis Telescope. With radio astronomy, galactic properties could be measured much beyond a galaxy's luminous radius. One of these measured properties was the rotational velocity of galaxies. By the early 1970s, it was found that these rotation curves had a peculiar characteristic: the velocity of rotation stayed "flat," i.e., constant out to large distances where there was no mass to account for this velocity.[39]

Around the same time, clusters of galaxies were found to be fundamental to the universe's structure, and a related anomaly that was first reported in the 1930s regained attention. In clusters of galaxies, the masses of individual galaxies did not add up to make sense of the observed dynamical state of the cluster. That is, the cluster's observed stability could not be explained only by the visible mass.[40] Both the flat rotation curves and the cluster discrepancy were well-known curiosities in the late 1960s and early 1970s. Only in 1974, however, these observations were linked together and interpreted as signaling the existence of missing mass. This was done by two independent research groups.

In their 1974 article, the Princeton collaboration of astronomer Jerry Ostriker and physicists Jim Peebles and Amos Yahil incorporated the observations mentioned

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[38] In a 1972 review, George Field wrote that "[t]he main interest in IGM [inter-galactic matter] stems from the evidence that galactic matter constitutes only a small fraction of the critical cosmological density of matter and energy [. . .]" (Field 1972, 227–228).

[39] For examples of these radio-astronomical studies, see, e.g., Roberts and Rots (1973); Rogstad and Shostak (1972); Rogstad et al. (1973). Influential optical studies were also done by Freeman (1970) and Rubin and Ford (1970).

[40] Important overviews of the problematic dynamics of clusters include Neyman et al. (1961), Page (1967), Rood et al. (1970), and Field and Saslaw (1971). The cluster problem was first remarked by Fritz Zwicky (1933).

above into a single argument. They connected different distance scales on which masses of galactic systems were dynamically calculated, to make an estimate of the mass density of the universe. Their analysis showed that the observations corresponded to a linear increase of mass with radius. The interest of the authors to bring these observations of galactic masses together was profoundly cosmological and their conclusion even more so. Their introduction read as follows:

> There are reasons, increasing in number and quality, to believe that the masses of ordinary galaxies may have been underestimated by a factor of 10 or more. [. . .] the current estimate (Shapiro 1971) for the ratio of gravitational energy to kinetic energy in the universe is about $\Omega = 0.01$. If we increase the estimated mass of each galaxy by a factor well in excess of 10, we increase the ratio by the same amount and conclude that observations may be consistent with a Universe which is "just closed" ($\Omega = 1$) – a conclusion believed strongly by some (cf. Wheeler 1973) for essentially nonexperimental reasons. (Ostriker et al. 1974, L1)

In the establishment of the dark matter problem, the demand for extra mass acted as a motivation to put together already existing observations and issues of mass and create a picture consistent with a closed universe. The paper of Wheeler to which the authors referred again emphasized the attractive power of a closed universe and "the mystery of the missing mass" that it entailed (Wheeler 1974, 686).[41] As discussed in Sect. 3, this belief in a closed universe was a remnant of Einstein's epistemological considerations concerning general relativity back in the beginning of the twentieth century.

The argument given by Ostriker et al. shows how cosmological practice was still embedded in the philosophical roots of cosmology and relativity. In the early 1970s, cosmology was born anew, fed by observations and the testability of its models. But the formation of this cosmological paradigm all but meant that a single approach to cosmology was practiced. Dark matter, in this sense, exemplifies the hybridity of the reborn cosmology: hybrid in the sense of its participants, being both astronomers and physicists; hybrid in its matters of concern, ranging from scales of galaxies to clusters; and, most illustratively, hybrid in its methodology, combining detailed observations with a priori conceptions of the cosmos. The rationale for this belief was no longer explicitly discussed as observational programs had become dominant, but, in practice, a closed universe kept being an influential and fruitful part of cosmological research.

Two months before the Princeton group, a similar argument was published by astronomer Jaan Einasto and cosmologist Ants Kaasik and Enn Saar from Tartu Observatory in Estonia in the USSR. They similarly frame their results in terms of the critical density needed to close the universe:

> Evidence is presented that galaxies are surrounded by massive coronas exceeding the masses of known stars by one order of magnitude [. . .] the total density of matter in the galaxies being 20% of the critical cosmological density. (Einasto et al. 1974b, 2)

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[41] Note that it was even the case that a tenfold increase in galactic masses for the authors meant that "observations may be consistent" with a hundredfold increase in the universe mass density (from 0.01 to 1).

Only with these two publications in 1974 the series of observations that are now considered evidence for dark matter were unambiguously interpreted as signaling the existence of yet-unseen mass, observations that acquired their meaning in the context of an age-old a priori consideration.

6 Conclusion

Between 1950 and 1970, the field of cosmology revolutionized. The cosmological program that was initiated by Einstein in 1917 truly materialized during this postwar period. Where in the early 1950s cosmology was dominated by meta-scientific discussions, the field had established itself as a respectable empirical science by 1970. This rebirth of cosmology was kick-started by a renovation in physics: the renaissance of the theory of general relativity between 1955 and 1963. The promise of general relativity theory was rediscovered during that era, and this reverberated in its application to cosmology. General relativity's renaissance was followed by a period in which an overwhelming number of cosmologically relevant observations were done and cosmology's ambiguous methodological foundations seemingly fell into oblivion. By the early 1970s, there was a newly established theoretical and observational cosmological canon; cosmology was born anew as a genuine physical science.

Although they are often discussed independently from one another, I argue that the rise of the dark matter problem in 1974 is most illustrative of cosmology's new paradigmatic shape. It lays bare four aspects of cosmology's rebirth. (I) It shows how cosmology had become a hybrid field of both physicists and astronomers: the two landmark papers that put the dark matter problem on the map were both due to such an interdisciplinary collaboration. (II) It demonstrates how the flood of new astronomical observations appearing in the 1960s fed into cosmological research: dark matter's claim to fame was built on two observations that are reminiscent of this decade of observational abundance. (III) Perhaps more tentatively, the rise of dark matter illustrates how modern cosmological practice involves drawing together a vast amount of different scales that are relevant for understanding the evolution and structure of the universe: the dark matter problem rose from data on galactic rotation curves and the extragalactic dynamics of galaxy clusters. (IV) The 1974 papers highlight that multiple methodological strategies underlie cosmological practice. In dark matter's case, anomalous observations were made relevant by relating them to the potential spatial closure of the universe, a model that was preferred purely on a priori basis.

The latter point has been central to my paper. Although in the 1970s cosmology became a respected subject to be studied empirically, it remained richly permeated with extra-theoretical considerations. A hybrid of "rationalist" and "empiricist" methods came to characterize cosmological practice after its mid-century rebirth. I argue that to understand the practice of modern cosmology, we must forgo normative claims on which approach is the correct one and instead accept that cosmology's methodology is fundamentally plural in character. Cosmology is a "methodological omnivore"; rather than focusing on a single method or test, the field drives on the convergence of multiple evidential means. This notion is used by philosopher Adrian Currie to describe the historical sciences (Currie 2015), and the analysis presented in the current paper seems a case in point to understand cosmology in a similar fashion.

Cosmology's omnivorous character again surfaces in research on contemporary hot topics. Particularly in discussions on the merits of inflation theory, the almost dialectic tension between empirical and deductive strategies in cosmology resurfaces, as is clear in a recent controversy published in *Scientific American* (Ijjas et al. 2017). Similarly, nonempirical arguments are still widely used in discussions surrounding the multiverse (cf. Kragh 2009). But also within empirical programs, different observational strategies are combined: the search for dark matter recently was argued to enter a new era "with the new guiding principle being 'no stone left unturned'" (Bertone and Tait 2018, 54). Perhaps this then is a lesson to draw from cosmology's history and the way the dark matter problem was brought to light: scientific knowledge of the universe is not acquired by a single approach but by a hybrid of different methods.

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Gravitational-Wave Research as an Emerging Field in the Max Planck Society: The Long Roots of GEO600 and of the Albert Einstein Institute

Luisa Bonolis and Juan-Andres Leon

1 Introduction

In this chapter, we explore the interplay between the *renaissance* of general relativity and the advent of relativistic astrophysics following the story of how gravitational-wave detection found its place in the Max Planck Society, one of the world's most prestigious research organizations, based in Germany and conducting basic research in the natural, life, social sciences, and the humanities. Based on this premise, we will then outline how the development of this research eventually led in 1995 to the building of the British-German interferometer GEO600 and to the foundation of the Max Planck Institute for Gravitational Physics, the Albert Einstein Institute.[1] Since the mid-1980s, the original aims of Max Planck researchers included building a fully sized 3-km gravitational-wave interferometer, but in the early 1990s, they were forced to scale down to a pilot facility. Instead, GEO600 had a fundamental role in developing and testing advanced key technologies that contributed to the unprecedented sensitivity of the last-generation interferometers, Advanced LIGO and Virgo, which successfully detected for the first time gravitational waves in September 2015 (LIGO Scientific Collaboration & Virgo Collaboration 2016).

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[1] The present contribution results from our larger work on the history of astronomy, astrophysics and space sciences in the Max Planck Society performed within the Research Program "History of the Max Planck Society" (http://gmpg.mpiwg-berlin.mpg.de/en/, accessed 1/8/2019).

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The Max Planck Society was founded in 1948 on the ashes of the Kaiser Wilhelm Society, whose origin dates back to the beginning of the past century. This unique scientific organization is formed today by more than 80 institutes and research facilities, and it is principally financed by public funds. Over four decades, gravitational-wave research rose inside the Max Planck Society to prominence from its humble origins as a "dark horse," backed by theoretical astrophysicists and particle physicists but generally marginalized by observational astronomers.[2] Our central focus is on the early decades up to the point when the field becomes firmly established and legitimate outside narrow circles, a period during which researchers in these fields circulate in small groups among different Max Planck Institutes initially in the Munich area (Physics and Astrophysics, Extraterrestrial Physics, Plasma Physics, Quantum Optics), before maturing into fully dedicated sites in Hannover and Potsdam, coinciding with the formation of the large international collaborations such as LIGO, Virgo, and GEO600, and culminating in their unification under the Max Planck Institute for Gravitational Physics, the Albert Einstein Institute.

In the Max Planck Society, gravitational-wave research began in the late 1960s, when general relativity was becoming "one of the most active and exciting branches of physics" based on the premises laid in the post-World War II period by the process dubbed "Renaissance of general relativity" (Will 1989, 7), which marked the return of Einstein's theory of gravitation to mainstream physics.

Recent scholarship has actually shown that the revival of the field started already in the 1950s as a consequence of a variety of elements, but it was mainly due to two factors, combining epistemological with sociological aspects: the discovery of the untapped potential of general relativity—as created by Einstein—as a tool for theoretical physics, and the emergence of a real community of relativists and cosmologists (Blum et al. 2015, 2016, 2017; Lalli 2017, 2020; Lalli et al. 2020).[3]

While this process was consolidating, toward the end of the 1950s, astronomical observations and calculations helped to spread the belief that general relativistic effects might be significant not only for cosmology, but might be of key importance also for interpreting the existence of violent events in the nuclei of galaxies and in isolated astronomical objects, as revealed by radio astronomy. The identification of celestial bodies of a new type, the quasi-stellar radio sources (*quasars*) announced in early 1963 (Hazard et al. 1963; Schmidt 1963; Oke 1963; Greenstein and Matthews 1963), the discovery of a rapidly pulsating radio source (*pulsar*) in 1967 (Hewish et al. 1968), as well as the serendipitous detection in 1964 of the Cosmic

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[2] For an account of the experimental search for gravitational waves, from the early days of the quest to the very recent past, up until 2004, when the book was first published, see (Collins 2004). The book focused in particular on the sociology of doing science and the growth from small-scale research to large-scale projects, but it also covered many aspects of the development and interpretation of gravitational-wave research in unprecedented detail. A book for a wider audience, reconstructing the history behind the first detection of gravitational waves on September 14, 2015, was written by Hartmut Grote, one of the scientists working at the German interferometer GEO600 (Grote 2018), its English version was published in 2019 (Grote 2019).

[3] See also (Will 1993).

Background Radiation (Penzias and Wilson 1965)—a supposed relic of the very early universe—appeared to be astrophysical bodies or phenomena pertaining to the realm of Einstein's theory.

Quasars were soon connected either with collapsing "superstars" or with very compact remnants resulting from supernova explosions. And indeed, investigations about the final fate of super-dense stars had arisen, since the 1930s, the necessity of taking into account Einstein's general theory of relativity. In 1939, Robert Oppenheimer and Hartland Snyder focused the attention on the process of gravitational collapse itself (Oppenheimer and Snyder 1939; Bonolis 2017; Almeida 2020). For the first time, it became evident that this phenomenon is of basic importance for the understanding of the nature of space and time, being a unique process where a fully relativistic theory of gravitation could be seen at work and the validity of general relativity might be confronted with observational evidence. Oppenheimer and Snyder's pioneering and foundational work—in fact one of the cornerstones of relativistic astrophysics—was completely ignored at the time and almost forgotten during World War II. Only during the post- and Cold War period did implosion and explosion problems, related to the design of thermonuclear weapons, bring about renewed interest in investigations on highly dense stellar matter and on the abandoned problem of gravitational collapse within Einstein's theory. New tools, typical of post-war science, were now available: The impressive advances in nuclear science combined with the first powerful computers, designed to perform the complex calculations for thermonuclear weapons, were now used to calculate the equation of state of condensed stellar matter up to the endpoint of thermonuclear evolution.

In the 1950s, two physicists—John Wheeler in the United States and later Yakov Zeldovich in the Soviet Union—emerged from their respective hydrogen bomb efforts and took up where Oppenheimer and his collaborators had left off in 1939, at the outburst of the war. Wheeler rediscovered Oppenheimer's forgotten papers and was led to a systematic study of general relativity. As he recalled in his autobiography: "It was actually nuclear physics and quantum theory that drew me into relativity" and somewhere else he added "It is hardly possible for someone interested in nuclear physics and relativity, as I was and am, not to get interested in stars. My Princeton friend Martin Schwarzschild [the well-known astrophysicist, son of Karl Schwarzschild] drew me into stellar atmospheres. From there it was natural to fall, so to speak, into the center of stars" (Wheeler 1998, 228 and 292).[4] Wheeler even decided to teach a course to really delve into general

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[4] Martin Schwarzschild remembered how astrophysical problems were used to test computers also used for thermonuclear research. John von Neumann, who had built at Princeton the first electronic computer MANIAC, "was very interested to have a problem which was nonlinear and sufficiently complicated to really need the whole power of his machine, but where lots of hand computations for checks were available; and therefore the stellar evolution work, which I think von Neumann also considered interesting in itself, though not all that deeply—he thought that that was an excellent one. So actually next to the official major program, the meteorological dynamics for which the machine officially was funded, stellar evolution got the biggest share of time." Martin

relativity and thus, around his wide research project, Princeton became one the most lively research centers, contributing to fire up the great revival of general relativity. Zeldovich and Wheeler had remarkable parallel interests: Zeldovich, already one of the most influential USSR scientists, had a strong background in nuclear physics and had been one of the main creators of the first Soviet nuclear weapon. He thus saw that the physics of stars and the physics of nuclear explosions have much in common and reoriented his research agenda toward the physics of matter under extreme conditions and especially toward cosmological issues and like Wheeler had a strong influence on other theorists within his seminar (Sunyaev 2004).

These scientists and other groups all over the world inspired a new generation of young theorists who laid the foundations for investigations on what is still one of the major challenges of relativistic astrophysics, namely, the equation-of-state at the center of a super-dense star, a main area of application of general relativity, together with gravitational waves and cosmology.

And indeed Wheeler further recalled, “What interested me was not the center of an ordinary star like our Sun, cooking away and generating thermonuclear energy. I was interested in the center of a cold dead star” (Wheeler 1998, 292). In the second half of the 1950s, assembling all the available theoretical information, Wheeler and his collaborators Ken Harrison and Masami Wakano constructed a semi-empirical equation of state for matter in its absolute ground state to the endpoint of thermonuclear evolution, at all stages of compression, up to supranuclear densities. Computer simulations confirmed previous theoretical results obtained during the 1930s by Subrahmanyan Chandrasekhar and by Oppenheimer and collaborators, which for the first time were brought together into a single overall picture (Wheeler et al. 1958, Fig. 12). Wheeler’s group presented this work at the eleventh Solvay Conference held in June 1958 devoted to the “Structure and evolution of the Universe” that gathered some of the most distinguished astronomers and experts in cosmology and general relativity.[5] The topic chosen for the Solvay congress meant

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Schwarzschild: Interview by David DeVorkin and Spencer Weart, December 16, 1977, Session III. Transcript, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA (from now on AIP), https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4870-3, accessed 30/7/2019. Schwarzschild also remembered that Wheeler was joined for a whole year by the theoretical astronomer Louis Henyey, who spent the period 1951–1952 at Princeton University where he was involved in the classified defense work on Project Matterhorn, the US top-secret project to control thermonuclear reactions. Martin Schwarzschild: Interview by William Aspray, Princeton, November 18, 1986. Transcript. N. J. Charles Babbage Institute. Retrieved from the University of Minnesota Digital Conservancy, http://hdl.handle.net/11299/107629, accessed 7/7/2019. Henyey realized that what he had learned at Princeton from von Neumann was extremely useful for the stellar interior and developed what came to be known as the “Henyey method,” which became the standard tool for the theory of stellar interior (Henyey et al. 1959).

[5] See especially the session “Matter-energy at high density; end point of thermonuclear evolution” in their contribution to the proceedings: “In seeking the consequences of Einstein’s theory for the structure and evolution of the universe we have been forced to consider what happens during contraction. Such implosion can be expected to lead not only to the dynamic instabilities just discussed, but also to unprecedentedly high densities of matter and radiation. Such densities pose unsolved problems to general relativity and elementary particle physics” (Wheeler et al. 1958,

an acknowledgment of cosmology as part of physics, thus mirroring the growing influence of results coming from radio astronomy and from nuclear astrophysics on the debates on cosmological models.[6] It was the first of this kind within the Solvay series, but it was also the first in establishing an official connection between astrophysics and general relativity. More in general, at the end of the 1950s, the considerable revival of interest in compact stars, and in the properties of matter at high densities, led to discussions and investigations on topics such as neutron stars or the possibility of gravitational collapse to a singularity in space-time.[7]

Technological progress during World War II had opened new horizons in the study of astronomy, and the realm of radio stars and very distant radio galaxies had become a subject of investigation. During the 1960s, the discovery of quasars—or "superstars" as they were originally called—but in particular of the pulsars—discovered in 1967 and immediately identified as neutron stars—for the first time appeared to offer the chance to solve the conflict between the already developed theory of the structure, evolution, and final fate of massive compact stars and the fact that such objects, since a long time discussed as theoretical entities by a few physicists, had not yet been observed. As opposed to cosmology, astrophysics had

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124). In his relevant report at the Solvay conference, Ambartsumyan suggested that the nuclei of galaxies are often centers of large-scale activity proceeding in different form and that the radio galaxies are not the products of collision of galaxies, as was accepted at that time, but are systems in which ejections from the nuclei of tremendous scale take place: "Apparently we must reject the idea that the nucleus of a galaxy is composed of common stars alone. We must admit that highly massive bodies are members of the nucleus which are capable not only of splitting into parts that move away at a great velocity but also of ejecting condensations of matter containing a mass many times exceeding that of the Sun [. . .]" (Ambartsumyan 1958, 266).

[6] The opening lecture was given by Georges Lemaître, giving a full account of his theory of the "primaeval atom." George Gamow, instead, was not invited. He was the main supporter of a universe emerging from a singularity, mockingly termed "big bang" by Fred Hoyle, the main opponent of this theory, who gave first a talk on the steady-state theory and then a second one on the origin of elements in stars. Radio astronomical observations as a source of information on the structure of the Universe were also discussed during the Solvay conference.

[7] The possibility of using the new computing facilities to investigate the physics of supernova explosions and of the formation of the elements, actually led Alastair G. Cameron, working at Chalk River, Canada's largest nuclear science and technology laboratory, to construct 20 neutron star models by integrating the general relativistic equations of hydrostatic equilibrium of the neutron gas, which discussed the transformation of neutrons into other baryons such as hyperons at very high densities and showed, among other results, that neutron stars could be probable products of the supernova process (Cameron 1959). Toward the end of the 1950s/early 1960s, discussions on the question of the equation of state of matter at ultrahigh densities in connection with the problem of the last stage of the evolution of heavy stars was intensifying. See for example (Salpeter 1960; Zeldovich 1962a; Zeldovich 1962b) and in particular David Beckedorff's dissertation on the terminal configurations of stellar evolution made under the supervision of Charles W. Misner (Beckedorff 1962). Apart from Cameron's article, which attracted attention of theoreticians toward neutron stars, other pioneering attempts had been made by taking into account the presence in dense stars of various elementary particles also through investigations on the nuclear interaction between nucleons and hyperons, some of which had been discovered in cosmic rays (Ambartsumyan and Saakyan 1960; Hamada and Salpeter 1961).

remained "non-relativistic" up to the early 1960s. The processes during which massive cosmic bodies and systems of bodies reach a relativistic stage became a central subject of the emerging field for which the term "relativistic astrophysics" was coined in 1963 (Robinson et al. 1965). This brand-new field was creating—and in turn was being created by—the growing dialogue and cooperation between astrophysicists and relativists on the widening scenario set up by the *renaissance* process already dawning into the "astrophysical turn" of general relativity (Blum et al. 2018; see also Lalli et al. 2020 and Chap. 1 in this volume).[8]

Relativistic astrophysics, however, could start in earnest only after the discovery of the pulsars. As a new class of radio sources characterized by the emission of short pulses of radiation having an extremely constant repetition frequency, immediately identified as rotating magnetized neutron stars (Gold 1968),[9] such super-dense stellar cores left behind after a supernova explosion dramatically brought back to the attention of a large section of theoretical and experimental astrophysicists the basic issues of the physics of gravitationally collapsed objects.[10] The clear evidence for the existence of such compact astrophysical objects, for which it was recognized that Einstein's theory of general relativity plays an essential role, made even plausible the reality of more exotic theoretical entities such as "black holes," as stressed by Wheeler and Ruffini: "No one who accepts relativity has seen any way to escape their existence" (Ruffini and Wheeler 1971, 39).[11]

As remnants of gravitational collapse following a supernova explosion—a main candidate event for the emission of gravitational waves—pulsars were immediately recognized as promising sources of such waves, whose search was going to become a main experimental branch of relativistic astrophysics as well as a fundamental test of Einstein's theory outside the solar system.

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[8] The process of the renaissance of general relativity has been analyzed in the already mentioned publications (Blum et al. 2015, 2016, 2017). Kip Thorne, on the other hand, designated the period 1964–1975 as the "golden age" of general relativity (Thorne 1994, 258–299), which he actually identified with the explosion of interest toward black hole research. For a different point of view on the concepts of "low water mark between 1925 and 1955," "renaissance," and "golden age" of general relativity, see (Goenner 2017b).

[9] Pulsars are characterized primarily by the emission of sharp flashes of radio waves at almost exactly maintained time intervals. Toward the end of the same year, two pulsars were found in known sites of supernovae explosions, the source PSR 0833 in the Vela Remnant and the source NP 0532 in the Crab Nebula. This led to the general acceptance of the rotating neutron star hypothesis for the basic nature of these objects, as suggested by Thomas Gold.

[10] The authors pointed out that the radiation might be "associated with oscillations of white dwarf or neutron stars," thus focusing attention on a very dense astrophysical object. For a more general treatment, see (Hewish 1970).

[11] The term "black hole" began to circulate and was officially launched by John Wheeler in 1968 (Wheeler 1968). However, it is not clear who used it first, although it appears that in reality it circulated as early as September 1963, during the first Texas conference, as reported in the issue dated January 24, 1964, of *Life* magazine by Al Rosenfeld, *Life*'s science editor, who had heard the term mentioned during the symposium (Siegfried 2013).

In the wake of the burst of interest brought by these major multifaceted developments, and thanks to the visionary support of its director, the well-known astrophysicist Ludwig Biermann, the Max Planck Institute for Astrophysics[12] in Munich was among the first to enter experimental gravitational-wave research, taking advantage of the existing technical and experimental capabilities headed by the pioneer of electronic computing machines Heinz Billing. In parallel, general relativity was established as a new research field at the institute, appointing a series of renowned experts. Through their strong participation in the early debates on gravitational-wave detection by Joseph Weber, Munich researchers acquired a dominant position at an early stage, supported by the recognition for their theoretical and experimental work. This privileged position was then mobilized for the timely reconversion of their experimental program from resonant bars to laser interferometry. In this venture, they took significant advantage of the shared resources, of expertise and infrastructure from other nearby Max Planck Institutes in the Munich area. In fact, between the early 1970s and the late 1990s, the gravitational waves research group was part of different institutes of the Munich family, before being finally absorbed in 2002 by the Max Planck Institute for Gravitational Physics—the "Albert Einstein Institute"—already established in Potsdam since 1995.

This chapter tells the story of the origins and itinerant life of this research group up until it found a home at a new dedicated Max Planck Institute. And while it gives an account of how gravitational physics entered the Max Planck Society, and sufficient allusions are made to the large-scale global collaborations that over the past two decades eventually resulted in the successful detection of gravitational waves, this chapter focuses mainly on the early phase of the story. Further detail on the twenty-first century stage of this global dynamic of competition and collaboration constitute ongoing research by many historians and sociologists since the successful detection of gravitational waves in 2015. Those studies can further elucidate the dynamic by which Germans contributed to instrumental developments while renouncing the infrastructural protagonism to the American (LIGO) and French-Italian (Virgo) collaborations.

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[12] Historically, Ludwig Biermann's Institute for Astrophysics was part of a larger entity called the Max Planck Institute for Physics and Astrophysics headed by Werner Heisenberg. These institutes shared the same building and benefitted from being part of a common umbrella "Max Planck Institute" which also included since 1963 the Institute for Extraterrestrial Physics. Only in the early 1990s these were formally divided into separate, fully fledged Max Planck Institutes. In this chapter, however, to distinguish it clearly from other organizations around the world, we use the term "Max Planck Institute for Astrophysics" to refer to the entity headed by Biermann and his successors.

2 Relativistic Astrophysics, Quasars, and the Beginning of the "Golden Age" of General Relativity

The lead up to gravitational-wave experiments undertaken in the early 1970s at the Max Planck Institute for Astrophysics was mainly rooted in the constantly growing interest within Ludwig Biermann's group in the new field of relativistic astrophysics, which radically transformed our view of the universe from the early 1960s onward. During the previous decade, the advent of radio astronomy had revealed that much in the universe is of an explosive nature and that violent events exist in galactic nuclei (Burbidge 1956; Mayer et al. 1957).[13] Astrophysicists had tried to understand the source of the tremendous energy stored in cosmic rays and the magnetic fields of some powerful radio galaxies (Burbidge 1959).

From the late 1940s/early 1950s, the problem of the origin of high-energy cosmic-ray particles and the mechanisms accelerating them with the related emission of synchrotron radiation, the source of the radio signals, had been a topic of great interest for Biermann. The inner workings of the Sun and stars had been his specialized area since the beginning of his scientific career during the 1930s, and, as a result, their structure and evolution had constantly been one of the key research topics at the Max Planck Institute for Astrophysics. In particular, compact stars had attracted his attention at least since 1931, when he participated in the hot debate about theories on super-dense matter in white dwarfs, which became a subject of correspondence between Biermann and Pascual Jordan in the 1940s (Biermann 1931).[14] As can be inferred both from publications and correspondence, during the 1940s and 1950s Biermann worked on topics involving astrophysical plasmas and magnetic fields in space—in general on cosmical electrodynamics—with his collaborators, notably Reimar Lüst, who would later become director of the Max Planck Institute for Extraterrestrial Physics, and Arnulf Schlüter, who would lead the Institute for Plasma Physics (Biermann and Schlüter 1950; Biermann and

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[13] The idea that synchrotron radiation could supply an efficient mechanism by which individual sources (both galactic and extragalactic) could radiate large radio powers had a great effect also in optical astronomy. The discovery of linearly polarized light emission from the Crab Nebula—confirming the synchrotron nature of radio emission—meant that astronomers had a new tool for studying high-energy processes, thus becoming more oriented toward a high-energy universe manifested in the various radio sources with optical identification.

[14] On these developments, see (Bonolis 2017). Biermann's correspondence with Pascual Jordan in 1946 was also about internal constitution of dense stars with neutron cores, a result of Jordan's extension of his interest in cosmology to astrophysics. On October 2, 1946, Jordan was asking Biermann's opinion on these issues (Archives of the Max Planck Society [from now on AMPG], III. Abt., ZA 1, Nachlass Ludwig Biermann [in the following, Biermann's papers will be cited in the abbreviated form NLB], No. 2). At that time, Jordan wrote a book on the constitution of stars, summarizing work published during the war, dealing in particular with super-dense matter. The book was reviewed by Biermann (Biermann and Jordan 1947). Biermann himself was working on super-dense white dwarfs (Biermann 1948). Jordan's interest was then turning to general relativity, creating the premise for future developments of this discipline in West Germany.

Schlüter 1951).[15] Growing evidence for the existence of relativistic plasma as an essential, major component of the universe—which had been a popular research topic at the Institute for Astrophysics—triggered the explosive growth of high energy and relativistic astrophysics since then. It became also clear that radio galaxies were among the most distant objects in the universe, while detection of polarized radiation from the Crab Nebula at optical wavelengths confirmed that radio emission was due to the synchrotron mechanism. The jets of the galaxy Virgo A, too, turned out to be synchrotron radiation emitted by ultra-relativistic electrons spiraling in magnetic fields. During a meeting of radio astronomers in Paris, which took place in summer 1958, immediately after the Solvay conference, Geoffrey Burbidge discussed the implications of synchrotron radiation coming from Cygnus A and showed that the energy needed to produce the high-energy particles was much greater than the expected energy from a collision of galaxies as hypothesized by the astronomer Walter Baade. It appeared that the nuclei of galaxies might host the necessary source of energy for such powerful processes. Such theoretical insight led Burbidge to stress that enormous energies were at stake (Burbidge 1959).

The realization that the energy released within strong radio sources can exceed an energy equivalent of millions of solar masses soon led William Fowler and Fred Hoyle to explore the possibility that "at the centers of the galaxies there are *star-like objects* with masses ranging from about 10^5 up to about 10^8 solar masses for abnormal galaxies [added emphasis]." Fowler and Hoyle's opinion was that "only through the contraction of a mass of 10^7–10^8 solar masses to the relativity limit can the energies of the strongest sources be obtained" (Fowler and Hoyle 1963a, 1963b, 170). This article appeared in August 1962, but in the meantime, Hoyle and Fowler took a further step. In February 1963, they argued that nuclear energy could not be the key to the problem, being unable to maintain sufficient internal pressure even to provide support against gravity for such massive astrophysical objects and observed that gravitational energy, instead, could be of decisive importance for bodies in that range of masses. The energies demanded by the strong sources were "so enormous as to make it clear that the relativity limit must be involved." As this limit was approached, "*general relativity must be used*" [added emphasis] (Fowler and Hoyle 1963a, 1963b, 535). "The conclusion was now clear; that at a certain stage of its

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[15] Biermann was considered to be an expert, and as such was asked to write a review article on this subject (Biermann 1953). Still in 1965, Biermann wrote to the Soviet astrophysicist Vitaly Ginzburg, who had been one of the first to theorize about the phenomenon of synchrotron emission and who continued to be focused on the problem of the origin of cosmic rays: "The publication of several new papers which seem relevant to questions of the origin of cosmic rays reminds me that I have still omitted to thank you for the copy of your and Dr. Syrovatsky's valuable monograph on the origin of cosmic rays, which was sent to me by the publisher. This field of research is in such an active development that it is most useful to have such a fine description of the present state of our knowledge. It will be most interesting to see how the growing body of information on the quasi-stellar sources on the one hand, and on more extended sources such as IC 443 on the other hand, will affect our ideas on cosmic rays in the years to come." L. Biermann to V. Ginzburg, April 8, 1965, AMPG, NLB, No. 31.

contraction (at about the size of the whole solar system) a very massive object would *implode* catastrophically, in about 100 seconds" (Hoyle 1963, 682).

Soon after, in the following March, Fowler and Hoyle's suggestions appeared to materialize in the "star-like" objects—with a very large redshift and corresponding unprecedented large radio and optical luminosities—whose identification was announced in four consecutive articles in *Nature* (Hazard et al. 1963; Schmidt 1963; Oke 1963; Greenstein and Matthews 1963). The dramatic recognition of these unusual objects was the result of a fruitful collaboration between radio and optical astronomers. The former provided precise positions of radio sources, which were then identified with star-like objects on photographic plates. In recognition of their small size, they were called *quasi-stellar* radio sources, soon renamed *quasars*.

The most pressing problem in astrophysics at the time became how to explain the mechanism whereby the most bizarre and puzzling objects ever observed through a telescope to date, which proved to be the most powerful energy sources in the sky, managed to radiate away the energy equivalent of five hundred thousand suns in short order. It was immediately connected with what Fowler and Hoyle had suggested in their February article, only a few weeks before the announcement: *gravitational collapse might be the driving force behind the large amount of energy emitted by strong radio sources*. Since such enormous energies must be emitted by regions less than one light-week across, collapsed objects became candidates for the engine of quasi-stellar radio sources. Their unusually high redshifts showed that they were the farthest objects detected in the universe. The most direct explanation for such large redshifts was that the quasars were extragalactic, their significant redshift reflecting the Hubble-Lemaître expansion of the universe. Fowler and Hoyle's proposed mechanism involving gravitational collapse—a purely relativistic phenomenon at the time not yet fully understood—turned a spotlight on the bonds between general relativity, astronomy, and astrophysics.

In December 1963, the first international Texas Symposium on Relativistic Astrophysics was held in Dallas, organized by three relativists: Ivor Robinson, Alfred Schild, and Engelbert Schücking. This event took place at a time when the complex process developing since the aftermath of World War II, which had put in motion the "renaissance" of Einstein's theory after a long period of stagnation, was being completed. After remaining cut off from mainstream physics for a generation, this formerly dispersed field was attracting an increasing number of practitioners, becoming the basis for the standard theory of gravitation and cosmology. New connections were now on the verge of being established with astrophysics and physical cosmology, through which general relativity would enter its "astrophysical turn," becoming an established branch of physics.

The first Texas event stemmed from the idea of having a small conference as an occasion to "put on the map" the recently created Southwest Center for Advanced Studies in Dallas,[16] which was part of a larger project aiming to promote

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[16] See *XXVII Texas Symposium December 8–13 2013, Dallas, Roundtable Discussion "Recollections of the Relativistic Astrophysics Revolution"* 2013, https://nsm.utdallas.edu/texas2013/

general relativity as a well-established research field at the University of Texas in Austin and in Dallas itself. The renowned relativist Ivor Robinson had just been appointed head of the Mathematical Physics Division of the recently created Southwest Center for Advanced Studies in Dallas, a successful result of Alfred Schild's far-sighted initiative—as leader of the Center for Research in Relativity Theory at the University of Texas in Austin (Lalli et al. 2020).[17] In 1962, Schild had also got Engelbert Schücking an associate professorship in the Austin mathematics department and, as Schücking himself recalled, "in the summer of 1962, while attending Andrzej Trautman's relativity conference in Warsaw, Poland [...] we persuaded Roger Penrose, Roy Kerr, Ray Sachs, Jürgen Ehlers, Luis Bel and others to flock to the newly created center of gravity in Austin" (Schücking 1989, 46–47).

Both Schücking and Ehlers had studied general relativity with Pascual Jordan, one of the pioneers of quantum physics, who had formed a research group in Hamburg back in the early 1950s, which was one of the seeds fertilizing the renaissance of general relativity.[18] Ehlers himself, emphasizing Jordan's wide-ranging interests, later recalled:

> [...] it was not astonishing that Jordan's seminar on General Relativity and cosmology which began in the mid-fifties and was carried on at the university of Hamburg for about fifteen years attracted many talented students, several of whom later attained professorships or research positions in physics or mathematics. At the post war time when General Relativity had almost been forgotten not only in Germany, Jordan recognized the importance of this field for future research. Due to Jordan and his students and collaborators the renaissance of General Relativity around 1955 took place not only in Syracuse, Princeton, Paris, London, Dublin, Leningrad and Warsaw, but also in Hamburg. Without this germ-cell of General Relativity in Germany a relativity group would presumably not have been created at a Max Planck Institute [...].[19]

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events/, accessed 31/7/2019. This special event dedicated to the 50th anniversary of the first Texas Symposium on Relativistic Astrophysics was organized during the XXVII Texas Symposium held in Dallas, December 8–13, 2013. It gathered veteran scientists recalling the circumstances that led to the first Texas meeting in 1963 and reflecting on the subsequent impact of such conferences. Schücking was not able to participate, but he shared his own recollections in a video (Schücking 2013).

[17] Schild had studied physics with Leopold Infeld, one of Einstein's disciples and collaborators, writing a thesis on cosmology.

[18] Ehlers earned his PhD in Physics at the University of Hamburg in 1958 with a dissertation entitled "Konstruktionen und Charakterisierungen von Lösungen der Einsteinschen Gravitationsfeldgleichungen" (Constructions and characterizations of solutions to Einstein's gravitational field equations). He obtained his habilitation in the same university in 1961 and for a short time held a position as assistant professor at the Christian Albrechts University in Kiel before moving to Syracuse University, New York, as a research associate that same year. After working several years with Alfred Schild's group, in 1965 he became associate professor at Austin, Texas, and since 1967 he was full professor there until 1971, when he moved back to Germany.

[19] Jürgen Ehlers, "Pascual Jordan – Originator of Quantum Field Theory and Founder of a Relativity School" in (Schutz 2003, 13). See also (Goenner 2017a; Lalli et al. 2020). For Ehlers' obituary, see (Allen et al. 2008).

As members of Jordan's group, Ehlers and Schücking became themselves key actors in the establishment and further evolution of the renaissance of general relativity. As we will see later, Ehlers' choice to leave Austin and go back to Europe is instrumental in our story.

On a hot July afternoon in 1963, Robinson, Schild, and Schücking were celebrating their reunion drinking strong martinis around a swimming pool in Dallas (Schücking 1989).[20] The idea of a meeting that could attract the attention on the new Southwest Center for Advanced Studies emerged, and crucially, Engelbert Schücking suggested organizing a conference on the "mysterious star-like objects," which had recently come to the fore and which were supposed to be connected to general relativity.[21]

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[20] At the beginning of his recollections, Schücking also mentioned a party to which all of them participated in that same evening and which took place at Manfred Trümper's. The latter had obtained his PhD in Hamburg with Pascual Jordan, and after being Bergmann's postdoc and subsequently becoming associate professor at the University of Texas, he would later become a member of Ehlers' research group on general relativity at the Max Planck Institute for Astrophysics.

[21] L. Marshall: Interview by Alan Mitchell, June 4, 1978. Transcript. Graduate Research Center of the Southwest, Center for Advanced Studies (SCAS) Collection, Special Collections and Archives Division, The University Archives, University of Texas at Dallas, Box 3, Folder 23. Schücking had been Pascual Jordan's student in Hamburg from 1952 onward. Astronomy had been one of his passions since he was a child and, at the age of 14, he was counting sunspots for Zurich Observatory. His first appointment was to Hamburg Observatory, at Bergedorf, where Walter Baade had worked since 1919, before emigrating to the United States in 1931, and where Biermann himself had spent some time from the end of the war up to 1947, when he moved to Göttingen at Heisenberg's Max Planck Institute for Physics. At the time, Bergedorf was considered "perhaps the principal observatory of Germany" (Kuiper 1946, 267). Schücking worked there from 1941 to 1962 with the astronomer Otto Heckmann, its director and head of the department of astronomy at Hamburg University, also known for his studies of relativity and cosmology. Heckmann and Schücking later co-authored several influential articles on relativistic and Newtonian cosmology. See p. viii and Schücking's Curriculum Vitae in (Harvey 1999, 515) and see also (Goldberg and Trautman 2018). Schücking recalled how his "interest in the physical aspects of cosmology was sparked" in 1955, when Walter Baade—perhaps the greatest observational astronomer of his time—had come from Pasadena to celebrate the inauguration of the Schmidt telescope. Baade had worked at Hamburg Observatory at Bergedorf from 1919 to 1931 and was then at Mount Wilson Observatory, where, together with Rudolph Minkowski, he had identified the optical counterparts of various radio sources, including Cygnus A, one of the brightest sources in the sky. The nature of such a source and the origin of its intense power was at the center of a large debate between astronomers, radio astronomers, and astrophysicists during the 1950s. Schücking was "absolutely fascinated by Baade's research" as he remembered in the mentioned video (Schücking 2013). He continued to follow developments in radio astronomy very closely during the 1950s, participating in conferences and meetings where radio sources and cosmological implications were continuously debated. In particular, along with Heckmann, he participated in the Solvay Conference of 1958 as well as in the Paris Symposium on Radio Astronomy of the International Astronomical Union held in July-August of that year. In 1961, Schücking visited the United States, spending most of his time in Syracuse, with Bergmann, but also having the opportunity to meet Felix Pirani and Fred Hoyle during his first visit to England. He then attended several conferences, where he met Georges Lemaître, Margaret, and Geoffrey Burbidge, and all the most influential US astronomers and astrophysicists. In 1962, he was appointed associate professor at the University of Texas at

They immediately involved Peter Bergmann in the organization of the conference. Bergmann, an influential relativist who had been Einstein's research assistant at the Institute for Advanced Study in Princeton since 1936, and had joined Syracuse University in 1947 also being active with a research appointment at the Yeshiva University, New York. Since the 1940s, he had established a center for relativity research at Syracuse University, which attracted all the leading relativists and became one of the very first active research groups on general relativity in the post-World War II period.

Consequently, the International Symposium on Gravitational Collapse and Other Topics in Relativistic Astrophysics, ultimately a "monster conference" hosting about 300 scientists, the first of a long series of Texas Symposia, was held in Dallas from December 16 to 18, 1963 (shortly after John Kennedy's assassination). Bringing together optical and radio astronomers, theoretical astrophysicists, and general relativists, it marked the start of a new era bridging the gap between the still exotic world of general relativity and the realm of astrophysics. Moreover, it officially opened the discussion on topics ranging from neutron stars to the possibility of gravitational collapse or a singularity in space-time, setting the stage for a dialogue between different scientific communities (Robinson et al. 1965).

A general consensus began to emerge from the awareness that general relativistic effects can play a dominant role in astrophysics and that astrophysical objects exist in the universe that are understandable *only* in terms of Einstein's theory. The classical works on gravitational collapse by the Indian physicist B. Datt and especially that by Robert Oppenheimer and Snyder published in the late 1930s (Datt 1938; Oppenheimer and Snyder 1939) and almost forgotten were being rediscovered and discussed, with acceptance of the situation that "stars with masses greater than the critical mass can reach a stage of catastrophic implosion in which general relativity becomes dominant" (Hoyle et al. 1964, 910).[22]

Austin. This background clarifies why Schücking was so quick to grasp the importance of the new discovery of quasars and the related astrophysical and cosmological implications.

[22] Such papers ("the classical implosion problem") were the starting point of a discussion by Hoyle, Fowler, and the Burbidges (Geoffrey, a theoretical astrophysicist and his wife Margaret, an astronomer) on various situations "envisaged as the final stages of evolution of a star which reaches the end point of thermonuclear evolution with a mass greater than the mass which can be supported either by degenerate electron pressure or degenerate neutron pressure" (Hoyle et al. 1964, 910). The process of gravitational collapse had been studied in detail since the 1950s by John Archibald Wheeler and his colleagues and presented at the first Texas conference. It was published as a separate volume from the general proceedings (Harrison et al. 1965).

3 A Privileged Role for Ludwig Biermann's Institute for Astrophysics Through Theory

Rudolf Kippenhahn, who would later become Biermann's successor as Director of the Max Planck Institute for Astrophysics, was heavily involved in research into the structure and evolution of stars, particularly with computer simulations.[23] He had participated in the first Texas conference, which officially launched the brand-new field of relativistic astrophysics, merging two seemingly distant fields, so far removed, that the organizers had to invent a new label for this.

In 1964, the detection of the cosmic microwave background radiation by Arno Penzias and Robert Woodrow Wilson (Penzias and Wilson 1965), together with the interpretation by Robert Dicke and his associates of such radiation as a signature of the Big Bang, provided a new element in favor of the Big Bang cosmological theory and marked the start of a new era for physical cosmology.[24]

In lessons held in 1965 at the Enrico Fermi summer school in Varenna, Italy—two years after the first Texas symposium—Kip S. Thorne clearly outlined how the early developments of relativistic astrophysics were connected with the ongoing "astrophysical turn" of general relativity:

> Astrophysics and general relativity influenced each other very little during the long period between the first few years of relativity theory and about 1963. In fact, during that period the absence of any extensive experimental or observational phenomena in which general

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[23] In the early 1960s, Kippenhahn used for such pioneering simulations a new version of the Henyey code he had learned in the United States and which had been improved at the Institute for Physics, running it on the computers built there by Heinz Billing during the 1950s, when no commercial electronic computers were still available.

[24] The phenomenon had already been predicted at the end of the 1940s by George Gamow's collaborators Ralph Alpher and Roger Herman within investigations on the origin of chemical elements in the Universe (Alpher and Herman 1948). The subsequent interpretation of the cosmic microwave background radiation (CMB) as the redshifted cool remnant of the thermal radiation of the hot early phases of the Big Bang, whose presence was to be expected if the expansion of the universe could be traced back to a time when the temperature was of the order of 10^{10} K, opened one of the most fruitful areas in observational cosmology (Dicke et al. 1965). CMB provides an omnipresent radiation background dominating all-sky images at millimeter and sub-millimeter wavebands, which are also characteristic of a wealth of molecular lines such as those observed in regions of star formation. On Robert Dicke's pioneering activity in the experimental study of gravity, see (Peebles 2017). If the radiation were truly the relic radiation from the early hot universe in thermal equilibrium, then it would have the famous blackbody spectrum whose formulation by Max Planck had initiated quantum theory in 1900. Measurements made by the COBE satellite, very precisely fitting with the expected blackbody curve with $T = 2.726$ K as predicted by the hot Big Bang theory, definitely corroborated the blackbody nature of the CMB spectrum, firmly establishing in the early 1990s that CMB is the relic thermal radiation from the primeval fireball that began our observable universe about 13.7 years ago (Mather et al. 1994). John C. Mather and George F. Smoot were awarded the Nobel Prize in Physics 2006 "for their discovery of the blackbody form and anisotropy of the cosmic microwave background radiation." The Nobel Prize in Physics 2006. NobelPrize.org. Nobel Media AB 2020. https://www.nobelprize.org/prizes/physics/2006/summary/, accessed 29/2/2020.

> relativistic effects might be important tended to insulate Einstein's theory from all other branches of physics. However, during the last three years a marked change has begun to occur: The discovery and investigation of quasi-stellar radio sources, of explosions in galactic nuclei, and of X-ray emission from supernova remnants have suggested to astrophysicists that strong gravitational fields might, after all, play an important role in astrophysical phenomena. At the same time, major advances in the techniques of radio and optical astronomy have enabled astronomers to begin to determine the cosmological structure of the universe—which structure is believed to be governed by general relativity—and the development of powerful new experimental techniques has made possible new and improved tests of Einstein's theory. Because of these developments, strong gravitation physics as described by general relativity is rapidly becoming of interest to astrophysicists, and astrophysics is rapidly becoming of interest to relativists (Thorne 1966).

Thorne, who was a member of the group led by John Wheeler at Princeton, one of the major centers of general relativity in the 1950s–1960s,[25] fully lived the emergence and development of relativistic astrophysics during the 1960s–1970s, with a strong focus on relativistic stars, black holes, and gravitational waves.

This backdrop—connecting a new phase in the study of general relativity and gravitation to the emergence of relativistic astrophysics—and the new perspectives opening up in general for astrophysical sciences are definitely mirrored by research activities performed at Ludwig Biermann's Institute for Astrophysics. The section "Aufbau und Entwicklung der Sterne" (Stellar structure and evolution) suddenly became the most extended in the 1964 research report where, for the first time, a few lines dedicated to general relativity appear (Biermann and Lüst 1965).

From 1964, the young researcher Peter Kafka began to work on topics related to general relativity and cosmological questions at Biermann's institute in Munich (Biermann and Lüst 1965, 61).[26] He investigated the problem of gravitational collapse in general relativity, but in particular he explored the space-time distribution of the quasars and radio galaxies as deduced from observational evidence (Biermann and Lüst 1966, 71). From radio astronomical observations, it appeared that there were relatively more quasars at larger distances, and so that must mean they were more common in the early life of the universe. This could be explained as an effect of its evolution: If their redshifts were of cosmological origin, quasars—whose very nature was still object of debate—must have existed only very far away in time

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[25] See Blum and Brill's Chap. 5 in this volume.

[26] Peter Kafka later recalled that at the time he made his "Diplom" in Physics, Arnulf Schlüter, one of Biermann's older collaborators, who had become director of the Max Planck Institute for Plasma Physics in 1965, had developed an interest in general relativity and asked him to work on this topic for his dissertation. Peter Kafka: interview by Michael Langer, March 27, 1999. Live-Gespräch-Sendung "Zwischentöne," Deutschlandfunk, http://www.gegen-den-untergang.de/zwischentoene1999.html, accessed 11/4/2018. At that time, quasars were discovered and so it became quite clear that general relativity would have a growing role in astrophysics and in cosmology and as an expert in such topics Kafka got a stable position at the Max Planck Institute for Astrophysics (Biermann and Lüst 1966). At the institute, cosmological questions related to the distribution of clusters of galaxies were also examined by G. O. Abell of the University of California and associated with the Mount Wilson and Palomar Observatories, an expert in extragalactic studies, then guest of the Institute. On such research issues, see also (Kafka 1968a).

and space, contradicting the perfect cosmological principle, which was at the core of steady-state cosmology. The counting of radio quasars as recognized sources at cosmological distances might thus help to confirm the existence of the Big Bang model, ruling out the steady-state model of the universe, according to which the expanding universe would maintain a constant average density, with matter being continuously created to form new stars and galaxies.[27] However, as Kafka pointed out in his article in *Nature*, there was disagreement "about the meaning of relations between the observed numbers, redshifts and brightnesses of quasars." He then concluded, "no decision can be made, *from a statistical count of quasars*, between steady state and other cosmological models [added emphasis]" (Kafka 1967).[28] Through Kafka, the Institute for Astrophysics was involved in such debates. He had been invited as one of the commentators at the end of the first day (dedicated to quasi-stellar radio sources) of the Third Texas Symposium held in Dallas in January 1967 and on that occasion had the opportunity to discuss the problem of the distribution of quasars in the Universe and observational cosmology with all the participants involved in this new field.[29]

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[27] The static universe proposed by Fred Hoyle and, independently, by Hermann Bondi and Thomas Gold, rejected the idea of an initial singularity, maintaining that a steady-state universe could be compatible with the drifting apart of galaxies if new matter (approximately one hydrogen atom per cubic kilometer per year) were continuously generated in the intergalactic space. Since the mid-1950s, complete new catalogues of radio sources had shown that the number of intense sources increased with distance, while from the steady-state theory they were expected to be uniformly distributed throughout the universe. Apparently, the most distant objects of the universe, quasars, had an impact in cosmology. If the high redshift of observed quasars was of cosmological origin, it meant that they were at distances such that the universe was much younger than it is now when the radio waves were emitted. This implied that galaxies produced more radio waves in the past, and thus began to call attention to the conflict between the Big Bang as a theory of cosmic expansion from a hot early universe and the steady-state cosmology, according to which the observable universe is basically the same on the large scale at any given time, a view called the "Perfect Cosmological Principle." An intense controversy developed between proponents of different theories of the universe, as discussed in (Kragh 1996).

[28] In his article, Kafka also mentioned having used a method programmed on a computer and announced that details would be provided in an internal report of the Institute for Astrophysics, in preparation (Quasars and Cosmology. Institutsbericht MPI-PAE/Astro 2/67). See also (Kafka 1968b). In 1968, a short paragraph entitled "Kosmologische Fragen und Quasars" was included in the Annual Report (Biermann and Lüst 1969, 87). In June, Biermann wrote to Kafka: "Thank you for the reprint of your article in Nature and the copy of your article "Quasars and Cosmology" which appeared as an institute report. Congratulations for the invitation to attend the summer school on astrophysics at Lincoln, Nebraska [. . .] After you return, I would like you to tell me in more detail about the present position of the Burbidges on the question of the distance of the Quasars." Biermann to Kafka, June 6, 1967, AMPG, NLB, No. 18. The issue of possible cosmological interpretation of the red shift of quasars and the counting of quasars was also discussed in a draft of a letter from Biermann to W. Mattig, who had sent him his article on the subject (Biermann to W. Mattig, January 21, 1969, AMPG, NLB, No. 19).

[29] Kafka to Biermann, February 27, 1967. AMPG, NLB, No. 18. In this long letter, Kafka gave a very detailed report on the conference, and Biermann answered that he himself would be at the University of Texas in a couple of weeks, hoping "to learn there something more about the present

4 Pulsars, Black Holes, and the Possible Evidence for the Existence of Gravitational Waves

As we have seen, in bringing the suggestion that pulsars had to be identified super-dense stellar cores left behind after a supernova explosion, this breakthrough discovery provided the first definite proof of the existence of these highly compact stars—previously only theoretical entities—in which the central densities can be as high as 10^{18} kg/m^3, meaning that the effects of general relativity are strong.[30] This radically widened the perspective, firmly establishing the belief that strong gravitational fields may be of key importance for quasars, for violent events in the nuclei of galaxies, for supernova explosions and remnants, for the death by collapse of very massive stars, and, in general, for the very compact astrophysical objects that were beginning to populate the universe of the 1960s. Toward the end of the decade, black holes, exotic objects having hitherto only a purely theoretical status, became serious—albeit much debated—astrophysical hypotheses. The longstanding commitment at the Max Planck Institute for Astrophysics to study the structure and evolution of stars, also performed with computer simulations, developed into research on very dense stars such as white dwarfs or neutron stars.[31]

During the 1960s, the epoch of X-ray astronomy was also beginning. According to Yakov Borisovich Zeldovich's estimations, the shock wave originating when the gas surrounding a neutron star falls onto its surface should produce radiation primarily in the X-ray range. Moreover, plasma oscillations might arise in this zone (Zeldovich and Shakura 1969). The discovery of the first radio pulsars, which

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state of relativistic astrophysics and of the problems of the quasi-stellar radio sources." Biermann to Kafka, March 13, 1967, AMPG, NLB, No. 18.

[30] The large increase in the rate of publication of papers on the properties of neutron stars also included general relativistic aspects which were especially studied under the direction of John Wheeler at Princeton, who had also considered possible emission of gravitational radiation from spinning and vibrating neutron stars: "The radiations include neutrinos, X rays, long-wavelength electromagnetic waves, and gravitational waves" (Wheeler 1966, 393). For a review on neutron stars as of the end of the 1960s, see (Cameron 1970). A discussion on the emission of gravitational radiation can be found on pp. 202–203.

[31] In July 1968, Kippenhahn wrote to Biermann, referring to white dwarfs, collapsing stars, binary systems, and mentioning the problem that for the study of such complex related phenomena one needed a more powerful computing machine and that they had further perfected their program on the evolution of stars, being at the forefront compared with other groups (Kippenhahn to Biermann, July 10, 1968, AMPG, NLB, No. 18). In 1969, Biermann himself lectured on neutron stars in the United States and was invited to talk on pulsars in more than one occasion, in particular in Italy, at the Scuola Normale Superiore in Pisa and at the Accademia dei Lincei in Rome (Luigi Radicati to Biermann, March 14, 1969, AMPG, NLB, No. 31). Kafka, too, was invited to lecture on the subject in the symposium "Pulsars, and High-Energy Activity in Supernova Remnants" held in Rome in which Biermann again gave a talk on related topics (Biermann and Lüst 1970). See also (Biermann 1969). In June, Wolfgang Kundt, one of Jordan's former students, like Ehlers and Schücking, wrote a letter to Biermann about Robert H. Dicke's gravity experiment on the oblateness of the sun, that is, its departure from a spherical mass distribution, which of course was highly interesting for Biermann (Wolfgang Kundt to L. Biermann, June 28, 1969, AMPG, NLB, No. 18).

turned to be strongly magnetized neutron stars, had started a new bonanza of radio astronomy. In the same 1960s scenario, gamma- and X-ray astronomy activities were in progress at the Institute for Extraterrestrial Physics in the Munich suburb of Garching, while the Max Planck Institute for Astronomy in Heidelberg and the Max Planck Institute for Radio Astronomy in Bonn were both finally founded. New conditions for the interaction between nuclear physics, astrophysics, cosmology, and optical and new astronomies were being created, widening the scope and context of what was being relabeled as the field of "cosmic physics." Scientists in the 1960s were beginning to look at the universe with the most diverse eyes, ranging from the large mirror of the Hale Telescope at Palomar Observatory to a tank filled with thousands of liters of dry-cleaning fluid buried deep underground capturing solar neutrinos, to arrays of detectors in the desert hunting for high-energy cosmic rays or, key catalyst of subsequent developments in this chapter, a swinging aluminum bar in Maryland, waiting for gravitational waves.

According to Einstein's theory of general relativity, accelerated masses produce gravitational waves, which propagate at the speed of light through the universe. The existence and physical properties of gravitational radiation became central to various research agendas as one of the important open questions addressed by the general relativity and gravitation community emerging from the mid-1950s onward, when "the availability of appropriate notions of what a gravitational wave is allowed physicists to put forward heuristic arguments for their existence and detectability" (Blum et al. 2018, 534).

In a summary of the Chapel Hill Conference on the Role of Gravitation in Physics, held in 1957, which was published in *Reviews of Modern Physics*, Peter Bergmann had expressed the following opinion: "[. . .] the most important nonquantum problem that has been discussed at this conference is the existence of gravitational waves." He added that their existence and properties "represent an issue of preeminent physical significance" (Bergmann 1957, 352–353). However, in the concluding summary published in the proceedings of the conference, Bergmann also remarked that the detection of gravitational waves would be an experiment "which is apparently not feasible, and is not going to be feasible for a long time." He further pointed out that there was no general agreement at the time about the existence of gravitational waves, a most important question still to be settled.[32]

One of the protagonists of this revival of interest was Felix Pirani, who studied with Alfred Schild and obtained his second PhD in Physics at Cambridge University under Hermann Bondi.[33] In 1957, Pirani published what was to become an

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[32] The proceedings of this conference, which was of great historical significance in the process of the renaissance of general relativity, have been republished as an open access volume (Rickles and DeWitt 2011).

[33] Felix Pirani: Interview by Dean Rickles, June 23, 2011. Transcript, AIP, https://www.aip.org/history-programs/niels-bohr-library/oral-histories/34463, accessed 30/7/2019. Bondi himself, who is known in particular for his work on cosmology, believed his most important scientific work was that on the theory of gravitation, specifically on gravitational waves (Hermann Bondi: Interview by David DeVorkin, March 20, 1978. Transcript, AIP, https://www.aip.org/history-

influential paper on gravitational radiation (Pirani 1957),[34] which, together with later contributions by Bondi, Ivor Robinson, and himself, as well as by Andrzej Trautman, overcame theoretical obstacles concerning the existence of gravitational waves and gave the "green light" to gravitational-wave search (Hill and Nurowski 2017).[35]

After the Chapel Hill conference, gravitational radiation became a key focus of theoretical studies in general relativity. In the meantime, Joseph Weber at the University of Maryland had chosen to spend his first sabbatical year (1955–1956) at the Institute for Advanced Study in Princeton, with John Wheeler and Robert Oppenheimer as his advisors (Trimble 2017, 265). He spent the second part of that academic year with Wheeler at the Lorentz Institute for Theoretical Physics in Leiden, which resulted in an article on gravitational waves that they co-authored, in which they addressed their reality and which was presented at Chapel Hill (Weber and Wheeler 1957). Encouraged by Wheeler himself, Weber accepted the challenge and pioneered the quest for the experimental detection of gravitational waves from astronomical sources.[36] In his first article presenting his views about the detection of gravitational waves, Weber thanked Pirani, Bergmann, and Wheeler for "helpful criticism" and acknowledged discussions with Robert H. Dicke (Weber 1960). Later, he spent the year 1962–1963 at the Institute for Advanced Study in Princeton and discussed the idea of a search for gravitational radiation with Freeman Dyson and Robert Oppenheimer. Both gave him strong encouragement (Weber 1980, 454).

Weber's experimental program was thus deeply embedded in the radical transformation characterizing the process of the renaissance of general relativity in the post-World War II period and in the related reorganization of knowledge. For several years, however, his experiments remained an isolated example. As underlined by

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programs/niels-bohr-library/oral-histories/4519, accessed 30/7/2019. He wrote a considerable number of papers on this subject in the period from 1957 to 1967, see in particular an article of 1957 in which he expressed the opinion that, contrarily to his own previous belief, "true gravitational waves do in fact exist. Moreover [. . .] these waves carry energy." Bondi also devised a "primitive detector" for gravitational waves (Bondi 1957). A discussion of gravitational waves in the context of the renaissance of general relativity can be found in (Blum et al. 2016). A specific analysis is in the more recent article (Blum et al. 2018).

[34] Pirani's interest in the problem of gravitational radiation aroused during the Bern conference of 1955, marking the 50th anniversary of special relativity, also prompted Hermann Bondi to take up the problem. Pirani presented his new work on wave theory at the Chapel Hill Conference, when a lively discussion took place during the session on gravitational radiation (Kennefick 1999, 215).

[35] For a longer, more detailed version, see (Hill and Nurowski 2016).

[36] As Wheeler himself recalled, "I gave such a feeling of reality to gravitational waves that Joe Weber has devoted himself since then to trying to detect gravitational waves." John Archibald Wheeler: Interview by Kenneth W. Ford, Session XI, March 4, 1994. Transcript, AIP, https://www.aip.org/history-programs/niels-bohr-library/oral-histories/5908-9, accessed 30/7/2019. Conversely, to have Weber as colleague during his Guggenheim fellowship first at Princeton and then at Leiden "was a real stimulus" to Wheeler. Session XII, March 28, 1994. Transcript, AIP, https://www.aip.org/history-programs/niels-bohr-library/oral-histories/5908-12, accessed 30/7/2019. A discussion on views about the existence of gravitational waves can be found in (Trimble 2017).

Peter Saulson, “Weber’s very concise discussion is remarkable for the prescience with which it foreshadowed not only his own work, but that of so many others. It also marks a watershed in the history of general relativity. In a single blow, Weber wrested consideration of gravitational waves from theorists concerned about issues such as exact solutions, and appropriated the subject instead for experimentalists trained in issues of radio engineering. The boldness and brilliance of this move are remarkable” (Saulson 1998).[37] However, Pirani himself—and certainly many others shared his opinion—was skeptical about the real possibility of detecting them: “The weakness of the gravitational interaction makes it exceedingly unlikely that gravitational radiation will ever be the subject of direct observation” (Pirani 1962, 199).

In his early papers, Weber speculated about the possibility of generating detectable gravitational waves in the laboratory but recognized that the chances of success were very low. As for the expected astrophysical sources, at the end of his 1960 paper, he had only briefly remarked, “The detectors which have been proposed are sufficiently good to search for interstellar gravitational radiation” (Weber 1960, 313). He later also mentioned as possible sources “events which might be associated with supernovae, neutron stars or closely spaced binaries”—again in the concluding lines (Weber 1963, 934).

Weber’s work, in turn, inspired interest in such astrophysical objects as possible sources of gravitational waves. In his pioneering article, written in 1962, before the discovery of pulsars (and therefore of neutron stars), Freeman Dyson speculated that the usual formula, giving the gravitational-wave energy flux from a binary star, leads—in the extreme relativistic case of a close binary collapsing system formed from a pair of neutron stars—to the prediction of a huge output of radiation. The powerful burst of gravitational waves—“the death cry of a binary neutron star”—should be detectable by Weber’s existing equipment (Dyson 1963, 119).[38] This remark gave an extra stimulus to the pioneering experimental work of Weber, also prompting the physics and astrophysics communities to consider gravitational radiation—whose physical reality was becoming plausible—as a phenomenon of great potential importance in the physical world.[39]

The astrophysical scenario was thus coming up with very promising sources of gravitational waves. Back at the first Texas conference in 1963, when quasars had just been discovered, some theorists were suggesting that gravitational energy, released by a supermassive object, was responsible for the powerful radiation

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[37] See also (Levine 2004).

[38] The article was submitted as prize essay to the Gravity Foundation in April 1962.

[39] The renewal of interest in massive stars had been kindled by the already mentioned Fowler and Hoyle’s suggestion that stars with mass of order of about 10^8 solar masses might accumulate at the center of galaxies or in intergalactic space serving as the source of the “prodigious” energies involved in emission or storage in the radio galaxies and stars. After the discovery of quasars, this led to the organization of the first Texas Conference and to discussions of the energy release in collapse of the core of a massive star and the related possibility of emission of gravitational radiation (Robinson et al. 1965).

emitted (Rees 1998, 81). Similarly, gravitational waves had been proposed as a mode of energy loss by Hoyle and Fowler, and by Hoyle in a further publication (Fowler and Hoyle 1963a, 1963b; Hoyle 1963). In any case, as astrophysical theory—and computer simulations—began to reveal the characteristics of compact objects, such as neutron stars, attention was turned to the possibility of *detecting* the gravitational radiation emitted by gravitational collapse. In parallel with Freeman Dyson's speculations, the idea that close double stars with one white-dwarf component could radiate enough gravitational power to be astrophysically significant and even detectable—thus becoming "of interest as a test for the existence of gravitational waves"—was also suggested by others (Kraft et al. 1962, 314). In 1963, a mathematical expression for cosmic gravitational waves from *realistic sources* was given by Philip Peters and Jon Mathews, who worked out the gravity-wave emission from Newtonian binary star systems in bounded Keplerian motion (Peters and Mathews 1963). In 1964, when neutron stars were still a hypothesis, Hong-Yee Chiu, credited with coining the term *quasar* (Chiu 1964b), discussed a picture in which every supernova would "inevitably become a neutron star" ("the only way a neutron star may be formed") stressing that the rotational energy would be "dissipated, during the collapse phase, into gravitational waves." If perfected to their expectation, instruments designed by Weber and his associates "should be able to detect such waves many galaxies away." Chiu also mentioned the possibility that such neutron stars could be detected "by extraterrestrial x-ray telescopes" also adding that if detected, they would "pose interesting questions on our present theory of fundamental particles" (Chiu 1964a, 405). According to John Wheeler, too, the super-dense core remaining after a supernova explosion—together with neutrinos, X-rays, and electromagnetic radiation—might also emit gravitational waves (Wheeler 1966). Gravitational radiation was being considered during the 1960s also as a possible mechanism for both the dissipation and transfer of energy in the domain of relativistic astrophysics (Braginskii 1966).[40]

Spinning compact objects, too, were candidate sources of gravitational waves. In 1965, Chao-wen Chin, inspired by Chiu, discussed the gravitational radiation from a spinning body and used his calculation to estimate the energy-loss rate of a spinning collapsing neutron star (Chin 1965). In 1967, Franco Pacini pointed out that a spinning neutron star with a large magnetic field *would emit electromagnetic waves*, and might even be a source of gravitational waves (Pacini 1967), an hypothesis explicitly discussed by Wa-Yin Chau (Chau 1967). Such theoretical premises further clarify how the actual discovery of the pulsars, rapidly rotating neutron stars emitting a beam of electromagnetic radiation at very regular intervals announced in February 1968, was really instrumental in arousing considerable interest in the theory of very dense stars and gravitational collapse. As pulsars were also

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[40] During the First Texas Conference, Fowler proposed the energy transfer via gravitational radiation from the binary core of two collapsed stars as a mechanism for polar explosion of the envelope leading to quasars (Fowler 1964). His model was revisited in a more detailed analysis in (Cooperstock 1967).

quickly recognized as promising sources of detectable gravitational waves, Weber immediately estimated the expected fluxes of gravitational radiation from such objects and proposed a search on a specific band, suggesting that for this search he could modify his apparatus (Weber 1968a).[41] The possibility of the emission of gravitational radiation by such astrophysical sources attracted wider attention to Weber's ongoing efforts, and, in 1968, he was asked to write a review article for *Physics Today*. The front cover of its April issue also featured a schematic representation of his experimental set-up (Weber 1968b).

Moreover, mid-June 1969 saw the publication of Weber's famous article claiming to have observed coincidences on gravitational radiation detectors based on resonating metal bars separated by a distance of about 1000 km at Argonne National Laboratory near Chicago and at the University of Maryland: "There is good evidence that gravitational radiation has been discovered" (Weber 1969, 1324).[42] The announcement immediately spurred a wide interest: for example, the front cover of the issue of *Science News* dated 26 December 1970 was dedicated to "Black holes and gravity waves," also featuring the drawing of a vortex representing the black hole: "Because gravity-wave signals have actually been reported and because looking for them has become a good deal more popular than it used to be, theorists have been taking a close look at the kinds of objects proposed as possible sources to see which of them might actually be detectable." The article mentioned Ruffini and Wheeler and, of course, Weber, but it also reported William O. Hamilton's opinion about the efforts to detect gravitational waves at Louisiana State University, using cryogenic detectors to reduce the thermal noise and increase the sensitivity: "As

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[41] Weber also suggested the Earth as a possible detector: because of its large mass, it has a very large cross section, which would make it absorb more gravitational waves. Freeman Dyson also studied the seismic response of the Earth to gravitational waves at pulsar frequencies (Dyson 1969). In the concluding lines, Dyson commented, "[...] we should remember the history of radio astronomy, which was greatly hampered in its early stages by theoretical estimates predicting that few detectable sources should exist. The predictions were wrong because the majority of sources were objects unknown to optical astronomers at that time. Whenever a new channel of observation of the Universe is opened, we should expect to see something unexpected. For this reason above all, the seismic detection of pulsars is not as hopeless an enterprise as the calculations here reported would make it appear." Scientists looking for gravitationally induced vibrations in Earth are mentioned in (Collins 2004, 116).

[42] Weber specified, "My definition of a coincidence is that the rectified outputs of two or more detectors cross a given threshold in the positive direction within a specified time interval. For the present experiments the time interval was 0.44 seconds. The magnitudes of the outputs at a coincident crossing enable computation of the probability that the coincidence was accidental. Observation of a number of coincidences with low probability of occurring statistically establishes, with good confidence, that the detectors are being excited by a common source. We may conclude that such coincidences are due to gravitational radiation if we are certain that other effects such as seismic and electromagnetic disturbances are not exciting the detectors" (Weber 1969, 1320). In this article, Weber acknowledged discussions, among the others, with C. W. Misner, H. S. Zapolsky, R. H. Dicke, F. J. Dyson, J. A. Wheeler, and P. G. Bergmann. See also Weber's preliminary description of the new series of experiments involving two detectors spaced about two kilometers apart, and the announcement that he had already observed a number of coincident events (Weber 1968c).

long as supernovas were thought to be the only source of gravity waves whose signals were likely to be observable, it was not worth expending the money and the engineering effort to build something that might wait around 40 years before recording a signal. Now that Dr Weber's uncooled detectors are seeing gravity-wave events on the order of once a month, the need for the more sensitive detectors is evident" (Thomsen 1970, 481).

Soon, gravitational waves—as well as hard X-rays and gamma rays—would be envisaged by John Wheeler and Remo Ruffini as one of the most promising ways to detect black holes (Ruffini and Wheeler 1971).[43] In 1970, Franck Zerilli analyzed the problem of the pulse of gravitational radiation given off when a star falls into a "black hole (Zerilli 1970), and Stephen Hawking's prescient article of 1971 even discussed gravitational radiation resulting from the collision of two black holes (Hawking 1971).

5 The Impact of Weber's Announcements at the Institute for Astrophysics

Joseph Weber's announcement caused a sensation in the physics community. The Max Planck Institute for Astrophysics quickly reacted to the new exciting perspective opened by his claims. His article was published in the June 16 issue of *Physical Review Letters*, and by July there was a telephone conversation between Weber and Biermann, who was at the time in the United States, where he was a regular visitor every year.[44] During the call, Biermann expressed the keen interest of his group in Weber's experiments, which were most probably discussed with his collaborators immediately before he left Munich.[45]

Shortly afterward, Peter Kafka reacted to Weber's article with a detailed analysis of the possible sources of gravitational waves, also discussing related difficulties of interpretation of his data, concluding that "All possibilities to explain the large number of events observed by Weber seem rather unlikely and demand more or less 'accidental' sources" (Kafka 1969, 138; see also Kafka 1970a, 1970b). Signals of the magnitude and rate observed by Weber were not easily explained on the basis of known astrophysical phenomena. If they originated from a source near the center of our Galaxy, as he suggested, it appeared rather hard to reconcile the

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[43] Gravitational waveforms emitted by test bodies falling radially into a Schwarzschild black hole were given for the first time in (Davis et al. 1972).

[44] This is very clear from the bulk of Biermann's correspondence. In general, he traveled very often and thus, when he was far from Munich, he exchanged letters with his collaborators, which are a precious source of information on the activity and movements of the group.

[45] Biermann to Weber, March 19, 1970, AMPG, NLB, No. 48. A telephone call was made between Aspen, Colorado, where Weber often spent time (as acknowledged in his articles) and Boulder, where Biermann had spent the months of July and August in 1969 giving lectures (Biermann and Lüst 1970, 79).

energy fluxes implied with other estimates of rate of energy loss by the Galaxy, as discussed for example by Dennis Sciama (Sciama 1969) or by Kafka himself in the essay “Are Weber’s Pulses Illegal?”, submitted to the Gravity Research Foundation competition.[46]

The characteristics of Weber’s gravitational-wave antennae were immediately studied by Hermann Ulrich Schmidt, while Kafka explored in detail the possible consequences of the gravitational waves “supposedly discovered by Weber.”[47]Gerhard Börner, who had been Heisenberg’s and Hans P. Dürr’s PhD student at the Ludwig Maximilian University of Munich, with a dissertation on quantum field theory in cosmology (*Feldtheorien im de Sitter-Raum unter besonderer Berücksichtigung der nichtlinearen Spinortheorie*),[48] was now working at the Institute for Astrophysics on relativistic cosmology and models of neutron stars.[49] Börner also collaborated on theoretical models for super-dense matter with Hans A. Bethe and Katsuhiko Sato, who had been guests in Munich (Biermann and Lüst 1971, 91; Bethe et al. 1970; Börner and Sato 1971) and later spent a long period of time in Kyoto, at the Research Institute for Fundamental Physics directed by Hideki Yukawa, as well as in the United States. With Börner, cosmology later became one of the fields included in the research agenda of the Max Planck Institute for Astrophysics.[50]

By early summer of 1969, both Biermann and Heisenberg were working toward intensifying research on gravitation theory and relativistic astrophysics. In this regard, they shared the common aim to invite the relativist Jürgen Ehlers to spend a long period of time at their Max Planck Institute. Ehlers, who had a professorship

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[46] Kafka’s essay was awarded the second prize for the year 1972 of the annual award offered by the Gravity Research Foundation (Anonymous 1972).

[47] These research activities, together with Biermann’s studies on some characteristics of the density of pulsars, were announced in the new section of the Annual Report entitled “Relativistische Astrophysik, Quasare und Pulsare” (Biermann and Lüst 1970, 86–87).

[48] See related publications (Börner and Dürr 1969, 1970; Börner 1970) and Heisenberg’s parallel interests during the 1960s in cosmological problems, unified theory of elementary particles, and nonlinear spinor theory (AMPG, III. Abt., Rep. 93, No. 913, 950, 951, 953, 954, 961, 965, 966, 982, 999). Heisenberg’s nonlinear spinor theory is a main focus of a detailed historical account of his quest toward a theory of everything during the 1950s (Blum 2019).

[49] See for example (Börner 1973a, 1973b). At the end of July 1969, Börner made a long report to Biermann about the Enrico Fermi summer school in Varenna dedicated to general relativity and cosmology, directed by Rainer K. Sachs, a former student of Peter Bergmann’s at Syracuse, to which Weber, too, participated. Börner mentions discussions there about Weber’s claims that the gravitational waves he was detecting apparently originated from the center of our galaxy (G. Börner to L. Biermann, July 30, 1969, AMPG, NLB, No. 18). On August 6, Biermann confirmed that he had got a message from Weber indicating that the gravitational radiation measured using his instruments was in fact not isotropic. Biermann also asked Börner whether he had taken a look at “less high densities” and mentioned that he was handling the problem of pulsars in connection with an invitation he had received at a symposium on the subject organized by the Accademia dei Lincei in Rome.

[50] A specific section dedicated to cosmology appeared in annual reports from 1986 onward (Hillebrandt and Schmidt 1987, 205).

at the University of Texas, Austin, was now holding visiting professorships in Germany (Allen et al. 2008). It became Biermann and Heisenberg's ambition to have him back in Germany.[51] In spring 1969, Biermann had met Ehlers in Göttingen proposing him to spend a few weeks in Munich in October, at the Institute for Physics and Astrophysics, during which time Ehlers might discuss gravitation theory and relativistic astrophysics topics at different occasions at the institute.[52] At the time, Ehlers had just published a wide overview on the state of cosmology in relationship with the impact of the recent discoveries of quasars, pulsars, the cosmic microwave background, which were pushing general relativity to the forefront, together with systematic experimental efforts to understand its observable predictions (Ehlers 1969).[53]

In early October of that year, Ehlers was in Munich (Biermann and Lüst 1970, 79). The recent discovery of pulsars, as fast-rotating super-dense neutron stars, had given rise to a series of new perspectives that were of the utmost interest for exploring connections between general relativity and astrophysics, a subject Biermann had the opportunity to discuss with Ehlers.

From then on, things moved quickly. Biermann proposed that Ehlers should move to the Max Planck Institute for Astrophysics[54] and, at the end of October,

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[51] At that time, Ehlers was even considered as a possible candidate for Heisenberg's succession. See documents in AMPG, II. Abt., Rep. 62, No. 437.

[52] Agreements about Ehlers' stay in Munich were made in June of that year. Following a meeting in Göttingen a short time previously, Biermann proposed him a sojourn of a few weeks in October (L. Biermann to J. Ehlers, June 16, 1969, AMPG, NLB, No. 18). This letter is followed by a draft of a letter not sent, probably prepared before their meeting in Göttingen (see handwritten note mentioning Göttingen) that is interesting because of its more detailed content, which illustrates the kind of research questions they would have liked to address at the institute [for Physics and Astrophysics] and the related idea of having Ehlers for a long period of time: "In connection with the most recent observations in the field of relativistic astrophysics, we have asked ourselves the question of how to make the theory of gravitation and the related questions of cosmology—also in the context of the more recent observations of 3° K radiation—and the pulsars, the subject of works here at the Institute. The Institute for Astrophysics already has an old tradition in some areas of mathematical physics and applied mathematics, and the Institute for Physics has long been interested in the relationships between quantum field theory and gravitation. In this context, the question has arisen as to whether we could hire you for at least one or two years for our institute [translation by the authors]." The letter concluded with several hypotheses on different options for the two scientists to see each other in Germany or in the United States, where Biermann was going for 2 months starting from July 11.

[53] See also his contribution on gravitational waves in the lectures at the summer school on general relativity organized by the well-known Italian relativist Carlo Cattaneo (Ehlers 2011). Ehlers had also recently edited a series of three volumes containing the proceedings of a summer seminar held at Cornell University in 1965 on problems in relativity and astrophysics, which included lessons on theoretical developments in general relativity, experimental tests of general relativity, stellar structure and gravitational collapse, gravitational radiation, observational cosmology, and cosmic rays (Ehlers 1967).

[54] Minutes of the 15th meeting of the board of trustees (Kuratorium) of the Max Planck Institute for Physics and Astrophysics, 17.03.1970, AMPG, II. Abt., Rep. 66, No. 3069. In the Kuratorium meeting of April 7, 1971, Biermann explained the underlying reasons for the decision to enter the

Heisenberg and Biermann sent a joint letter to Adolf Butenandt, then President of the Max Planck Society, in which they emphasized how during the last year general relativity and gravitation question had become relevant both at the Institute for Astrophysics and for Physics, especially in relationship with gravitational waves and neutron stars. For this reason, the Munich institutes would strongly benefit from the presence of a renowned relativist like Jürgen Ehlers.[55] In documents related to Ehlers' call to Munich, it is clearly stated how *both* Heisenberg's and Biermann's scientific interests would benefit from having Ehlers at the institute. A main aim was also to build a bridge between unified field theory and gravitation theory, in connection with new related interests in astrophysics and the idea of creating a group working on gravitational-wave experiments. This would thus also create a deeper relationship between theory and experiment.[56] A first step in this direction would be to call Jürgen Ehlers and open new perspectives at the institute in interdisciplinary studies encompassing astrophysics and theoretical work on the unified field theory. On November 7, a commission to appoint Ehlers as a scientific member of the Institute for Astrophysics was formed.[57] On March 3, the Senate confirmed the appointment, remarking that Ehlers' visit to Munich had shown that his presence would be of the greatest importance for both the Institute for Physics (Hans P. Dürr's theoretical group) and Biermann's Institute for Astrophysics, as well as for the Institute for Extraterrestrial Physics. Together with the Max Planck Institute for Plasma Physics led by Schlüter, all these had been born from Heisenberg's Institute for Physics established in Göttingen after the war, as a continuation of the Kaiser Wilhelm Institute for Physics founded in 1917 in Berlin.[58]

Connections between gravitation theory and topics such as gravitational waves, neutron stars, quasi-stellar systems, and also quantum field theory and future attempts to detect gravitational waves had come into focus as key research questions. Ehlers' arrival would not change the overall organization: Research on elementary

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field of gravitational wave experiments, which also clarify that the idea of having Ehlers at the institute had been around since some time.

[55] L. Biermann and W. Heisenberg to Adolf Butenandt, October 31, 1969, AMPG, III. Abt., Rep. 93, No. 1667.

[56] Minutes of the 15th meeting of the board of trustees (Kuratorium) of the Max Planck Institute for Physics and Astrophysics, 17.03.1970, AMPG, II. Abt., Rep. 66, No. 3069.

[57] The same committee was also involved in Heisenberg's succession. Such appointment proceedings are described in reports of the committee to the chemical-physical-technological section of the scientific council of the Max Planck Society, which are part of Rep. 62. From now on, these are shortened to "CPTS minutes" + dates/numbers. (CPTS meeting minutes of 07.11.1969, AMPG, II. Abt., Rep. 62, No. 1757).

[58] A department for Astrophysics led by Biermann existed since 1948 within the Max Planck Institute for Physics, becoming a sub-institute in 1958. The Institute for Extraterrestrial Physics led by Reimar Lüst was founded in 1963 as a new sub-institute, while the Institute for Plasma physics was founded in 1960 as an autonomous entity, from which the Institute of Quantum Optics—having a relevant role in this story—was born in 1979. As we will see in the following pages, the family will further enlarge with the foundation of two new Max Planck Institutes related to general relativity and gravitational-wave research.

particle physics would continue as in the past, but general relativity and gravitation would create a new link with astrophysics. With the incursion into gravitational-wave experiments (see below), the Institute for Astrophysics would also move into experimental astrophysics based on a strong theoretical standpoint, a process that was characteristic of the Munich family of institutes.

Ehlers became a scientific member of the Institute for Astrophysics as of June 1, 1971.[59] At that time, general relativity was becoming a major branch of physics, also boosted by the newly established fields of relativistic astrophysics and observational cosmology (Blum et al. 2018).

6 Heinz Billing's Resonant-Bar Experiments and the Promise of Gravitational-Wave Astronomy in Munich

In the meantime, in late November 1970, the possibility of starting a gravitational-wave experiment was being seriously considered by Biermann's group.[60] The section headed "Relativistische Astrophysik, Quasare und Pulsare" of the 1970 Annual Report was now clearly showing the establishment of a new research line in which, for the first time, the "preparation of a gravitational-wave experiment" is mentioned, together with studies on matter at supranuclear density and neutron star models.[61] The extreme physics of these stars could allow astronomers to probe physics in very strong gravitational fields—and general relativity would be the tool to understand the structure of such highly energetic, compact astrophysical objects.

In parallel with intense theoretical work on general relativity and relativistic astrophysics, plans for the gravitational waves experimental activity at the Max Planck Institute for Astrophysics continued, immediately involving on the experimental side Heinz Billing, who was leading the computing group since the early

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[59] On February 9, 1971, during the meeting of the CPT Section of the Scientific Council, it was communicated that Ehlers had accepted, and that he would take up his position on June 1, 1971 (AMPG, II. Abt., Rep. 62, No. 1761).

[60] See Biermann, to Börner, November 26, 1970, AMPG, NLB, No. 20. On November 26, 1970, Hermann Ulrich Schmidt, who was spending some time at the National Solar Observatory at Sacramento Peak in New Mexico, was writing to Biermann about discussion he was having with Ehlers, Weber, and Remo Ruffini about beginning a gravitational wave experiment in Munich. See also answer from Biermann on December 8 (Schmidt to Biermann, November 26, 1970, and Biermann to Schmidt, December 8, 1970, AMPG, NLB, No. 21).

[61] The group included Kafka, Friedrich Meyer, and Gerhard Börner (Biermann and Lüst 1971, 91). See also works published during the initial phase, as (Kafka and Wills 1972). Kafka stressed that the most powerful emission would come from "the collapse of a rotating star towards a black hole" or the "fusion of two black holes in a dense cluster" (Kafka 1970b, 436).

1950s, now successfully returned to physics.[62] Wheels were put in motion and work began in earnest in 1971, when the gravitational-wave experiment had its own specific section in the Annual Report.[63] The aim was "to confirm or disprove the existence of gravitational pulses suggested by Weber as an explanation of his results" (Billing et al. 1975, 111). With the arrival of Ehlers in June 1971, the new Department for Gravitation Theory and Relativistic Astrophysics was established.[64]

As the Munich setup was planned to be as close as possible to Weber's experiment, in January–February, both Billing and his new assistant, Walter Winkler, visited Weber at the University of Maryland in order to become familiar with his antenna and obtain all the information that would be useful for their future work.

But for a coincidence experiment, they needed a second antenna, far from Munich. They were lucky, because, independently from them, a German colleague, the electronics engineer Karl Maischberger, and the physicist Donato Bramanti had also begun to work on a Weber-type gravitational-wave antenna at the European Space Research Institute (ESRIN) in Frascati, near Rome, with which the institute

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[62] As recalled by Billing: "In 1970, I was surprisingly visited in my study by two Biermann's senior staff members, H.U. Schmidt and Friedrich Meyer, and asked if I would be willing to repeat Weber's gravitational wave experiment [. . .] This task appealed to me immensely. A whole new field of research, working at the limit of the measurable and finally again practicing real experimental physics! But there was one difficulty. The only one who was free to do a new job in my department was me. All my employees were deep into important ongoing projects [our translation]." And so, having at disposal a couple of free positions, Billing hired a new assistant, Walter Winkler, through an announcement in *Die Zeit*, a most renowned weekly newspaper in Germany (Billing 1994, 156–161). Winkler began to work on January 1, 1971, and they soon went to visit Weber in the United States for a whole month.

[63] They specified that the decision to repeat Weber's gravitational-wave experiment had been taken because of both its great astrophysical significance and the still pending difficulties in evaluating Weber's findings. The prerequisites for this were particularly favorable at the institute, as the necessary engineering and electronic experiences were available at the Numerical Calculators Division, while the local astrophysicists would be able to handle the theory and the statistical problems, and the addition of Ehlers would guarantee the close connection with the general theory of relativity. It was further emphasized how Weber's detector could be improved (Biermann and Lüst 1972, 326).

[64] Ehlers carried on his fundamental work on the exact solutions of Einstein's theory, and Börner continued his theoretical work on pulsars and neutron stars, also as supernova remnants. Kafka and Meyer analyzed Weber's evaluation methods in view of their own planned experiment. Martin Walker studied problems connected to the theory of black holes. Activities included the organization of a 2-day workshop on pulsars at the institute and participation in conferences related to relativistic astrophysics and general relativity and gravitation (Biermann and Lüst 1972). During the course of 1971, new members had joined Billing in the gravitational wave project: W. Winkler (from January 1), and John M. Stewart and Martin Walker (both from October 1). John Stewart was a former student of Dennis Sciama and George Ellis in Cambridge at the Department of Applied Mathematics and Theoretical Physics and at the Centre for Theoretical Cosmology http://www.ctc.cam.ac.uk/news/161121_newsitem.php, accessed 1/9/2019. Both Stewart and Walker were meant to work as assistants to Ehlers (see related correspondence between Ehlers and Biermann between late 1970 and spring 1971, AMPG, NLB, No. 20).

had already interacted in the past years (Bramanti and Maischberger 1972).[65] A Conference on Cosmic Plasma Physics was organized by ESRIN in September 1971. Biermann participated in this conference and it was certainly an opportunity for him to become acquainted with local plans on the gravitational-wave experiment and to establish a collaboration.[66] In the early 1970s, a second experimental activity for gravitational-wave detection had also begun in Italy, at the University of Rome, led by Edoardo Amaldi and Guido Pizzella, which evolved along a different research line.[67]

The Munich resonant bar—a long aluminum cylinder reproducing Weber's setup that should ring at a certain frequency in response to a gravitational wave—began operating as of October 1972.[68] The aim was to test whether the pulses of gravitational radiation reported by Weber were detectable in coincidence between Munich and Frascati.[69] The first negative results, in conflict with Weber's, were

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[65] While intending to be as close as possible to the original experiment, they still made several improvements, which made their detector—together with the similar one built in Frascati—"the most sensitive room-temperature bar experiment at that time" (Winkler 2018, 15).

[66] For several years, Biermann had been in contact with the astrophysicists Livio Gratton and Franco Pacini, working at the Laboratory for Space Physics in Frascati. In December 1970, Biermann wrote to Pacini: "I read your paper on 'Neutron stars, Pulsar Radiation and Supernova Remnants' which you had sent me earlier, with great interest." Biermann to F. Pacini, December 10, 1970, AMPG, NLB, No. 30. In February of the following year, Biermann mentioned his participation in the Conference on Cosmic Plasma Physics. Biermann to F. Pacini, February 3, 1971, AMPG, NLB, No. 30. The same folder also contains correspondence with Livio Gratton, who had initiated research on relativistic astrophysics at Sapienza University in Rome and was called from abroad by Edoardo Amaldi to the chair of astrophysics (Bonolis et al. 2017).

[67] The relationship between Biermann and Amaldi dated back to the end of the 1950s, when Enrico Persico and Amaldi were setting up at the Physics Department of Sapienza University in Rome the Laboratorio Gas Ionizzati, where research on plasma and thermonuclear fusion was performed (see correspondence in Edoardo Amaldi Archives, Sapienza University of Rome, Enrico Persico papers, Box 16). See, also, for example, Edoardo Amaldi's letter to L. Biermann, June 17, 1959, thanking him for lessons held in Rome on plasma and cosmic rays. Biermann had offered that Rudolf Kippenhahn might stay in Rome for a few months, but Amaldi wanted someone who could remain for at least 2 years to give a strong support to initial activities on plasma research (AMPG, NLB, No. 28) (Bonolis 2012). On the other hand, Amaldi's interest toward general relativity and gravitational waves had developed since the end of the 1950s, leading him to encourage his postdoc student Remo Ruffini to visit Pascual Jordan in Hamburg and later supporting his application for an European Space Research Organization (ESRO) 2-year fellowship to be spent in the United States, at Princeton with John Wheeler and at Maryland University with Weber. In July 1967, in a letter written to Reimar Lüst (who would soon become Vice President of ESRO) Amaldi mentioned his intention of setting up an experimental group working in the field of gravitational waves "at the return of Ruffini" (E. Amaldi to R. Lüst, July 7, 1967, Edoardo Amaldi Archives, Box 375, Folder 3). On the Roman activities, see (Bonolis and La Rana 2017; La Rana and Milano 2017; Pizzella 2008). A more complete historical overview emphasizing experiments made by the Rome group can be found in (Pizzella 2016).

[68] Both Munich and Frascati built detectors as close to Weber's as possible, including a close match with his resonant frequency of 1660 Hz (Bramanti et al. 1973).

[69] As ESRIN was going to be closed, the Frascati experiment would become part of the Munich program, but run in Frascati (Pinkau 1973, 101). See also (Kafka and Meyer 1972).

presented in June 1973, in Paris, at the International Colloquium on Gravitational Waves and Radiation (Kafka 1974a).[70]

As a sign of the growing importance of its status, the Munich-Frascati gravitational-wave experiment became the first research activity presented in the Annual Report 1973. No signal had been detected, but investigations were underway that aimed at understanding whether improvement of antennae could further extend the sensitivity.[71] In 1974, the experiment was still the first item presented in the Annual Report: It was considered to have reached the highest performance possible for that type of antennae, with the highest sensitivity among the coincidence detectors used (Pinkau 1975, 103).

Triggered by Weber's announcement, other groups had also started experiments to analyze and test Weber's results: in the United Kingdom,[72] in the United States at IBM and Bell Laboratories (Levine and Garwin 1973; Tyson 1973; Douglass et al. 1975), in Japan (Hirakawa and Narihara 1975), and in the Soviet Union, where discussions on gravitational-wave detection began already around 1960.

One of the main promoters of gravitation research in USSR was Dmitri Ivanenko (University of Moscow), who had just edited the volume *The Newest Problems of Gravitation*, where the Russian translation of papers on the problem of gravitational waves, notably by Bondi and Weber, were included.[73] Ivanenko was a member of

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[70] At the same conference, Silvano Bonazzola and his group presented the results of the Weber-type detector built at the Meudon observatory, mentioning double and triple coincidence research with the Munich and Frascati detectors (Bonazzola et al. 1974). Kafka gave a talk with the same title at the Symposium "Gravitational Radiation and Gravitational Collapse" of the International Astronomical Union held in Warsaw in early September 1973 (Kafka 1974b). See also discussions during the Sixth Texas Symposium of 1972 (Weber et al. 1973).

[71] In 1973, the group included Billing, Kafka, Meyer, Lise Schnupp, and Winkler. Maischberger, too, was a member of the scientific staff at the institute (Pinkau 1974).

[72] The group in Glasgow was then led by Ron W. P. Drever, who later became a member of the team initially running the LIGO project. Interestingly, Drever's interests in those days went from the search for gamma rays from pulsars to radio signals associated with gravitational waves, searching specifically for pulses from the galactic center, which might be correlated with the events reported by Weber (Charman et al. 1970, 1971). The Glasgow group (J. Hough, J. R. Pugh, R. Bland and R. W. P. Drever) used a slightly different type of detector, and instead of a single bar, they had a system made by two separate aluminum bars with piezoelectric transducers cemented between them to monitor changes in their separation. This arrangement would give higher coupling between the mechanical and electrical systems, and so a much larger fraction of mechanical energy would be communicated to the transducers, obtaining a larger electrical output than in a Weber detector. For their observations, two of these detectors were set up 50 m apart and results from data recorded between fall 1972 and April 1973 were submitted in early September: "In this time," they claimed, "we have observed one distinctive signal which fulfils the requirements expected of an event due to a short pulse of gravitational radiation." But they finally concluded that it was unlikely that the signals reported by Weber in 1970 were due to pulses of gravitational radiation of duration less than a few milliseconds, even if, based on their observations, they did not exclude "the possibility that Weber may have detected bursts of gravitational radiation of much longer duration" (Drever et al. 1973).

[73] For an overview of experimental research on the detection of extraterrestrial gravitational radiation performed in Soviet Union since the late 1960s, see (Rudenko 2017).

the International Committee on General Relativity and Gravitation established after the Royaumont conference of 1959 near Paris, which brought together scientists working on both sides of the Iron Curtain with the task of coordinating research activities and organization of international conferences (Lalli et al. 2020, 51–53). At that time, Zeldovich was shifting his research interests from nuclear and particle physics to astrophysics, general relativity, cosmology, and the new astronomies. He was one of the first to think in terms of the Big Bang Universe as a natural laboratory for particle physics and to call the universe "a giant accelerator." For the last 25 years of his life, astrophysics and cosmology had a central place in his thinking, as well as in the thinking of his collaborators (Sakharov 1988). Zeldovich immediately recognized the importance of Weber's gravitational-wave experiments and their possible role as probes to explore fundamental physics and cosmology and as a tool in astronomy and astrophysics to study compact objects, and organized a meeting of his theoretical group with Vladimir B. Braginsky's experimental group, at Moscow State University.[74] Braginsky assembled a scientific group and was the first, after Weber, to build a resonant bar—of the same size as those of Weber—and while active theoretical studies continued, experimental efforts were performed from the late 1960s, repeating the search for coincident signals on separated Weber-type antennae (Braginskii et al. 1969). Results obtained were negative (Braginskii et al. 1972).[75]

In December 1974, also the Glasgow group reported a negative result providing evidence "against the hypothesis that the signals reported by Weber are caused by a large flux of very small pulses" (Hough et al. 1975, 501).

Other searches were carried out by various groups, all with predominantly negative results. The Munich-Frascati experiment reported results of the first 150 days of coincident data in 1975 (Billing et al. 1975)[76] and, in March 1976, after 580 days of total useful observation time, the detectors were dismantled and the experiment stopped, with the conclusion that "The most interesting aim of gravitational wave astronomy will be the observation of stellar collapse" and the hope that "the many new antennae, being developed now and during the next decade, will be able to detect a few gravitational collapse events per year, and thus provide most valuable information on extreme states of matter and final stages of stellar

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[74] Since the end of the 1950s, Braginsky had discussed the possibility of measuring the speed of propagation of gravitational waves (Braginskii et al. 1960), and in his review of 1966 (the Russian version was published in 1965), he also presented a list of the most noteworthy potential sources to be detected (Braginskii 1966).

[75] The authors reported that no statistically significant excess coincidences had been found in their experiment. See also (Braginskii et al. 1974).

[76] The Frascati-Munich group claimed to have "set the lowest limits so far obtained for the rate of incoming short gravitational pulses stronger than a few times 10^5 erg/cm^2 Hz at frequencies around 1660 Hz." The same frequency band had been used by Weber. See (Billing and Winkler 1976, 665).

evolution which will otherwise remain hidden."[77] Billing himself thought that the negative outcome was "not tragic," and that they had built what for some time had been the most sensitive antennae worldwide. As Kippenhahn commented, "Billing and his group are those who up to now have been the best in *not* finding gravitational waves" (Billing 1994, 163).[78]

In the early 1970s, the Max Planck Institute for Astrophysics considerably expanded its research activities, and the growing number of national and international visitors corresponded to a similar flux of internal members visiting scientific centers abroad or invited to give talks, as evident from the annual reports at the time.[79] This went in parallel with the explosive developments of astrophysics and cosmology, strongly supported by the rapidly evolving field of the new astronomies whose birth had been fueled by the advent of the space age. These new technological windows also promised to allow studies on astrophysical processes that only seemed possible within the framework of general relativity. For instance, black holes and the search for their observational evidence, theories of quasars, neutron stars, compact X-ray sources, the physics of high density and nuclear matter, and the distribution of quasars in the universe were becoming popular subjects addressed at conferences. In 1973, the 16th Solvay Conference in Physics, entitled "Astrophysics and Gravitation," was promoted by Edoardo Amaldi (then President of the Scientific Committee), strongly supported by Biermann and Heisenberg.[80] At that time, when gamma and X-ray astronomy were already becoming a key tool for understanding the high-energy universe, the quest for the detection of gravitational waves using the new technique of laser interferometry was still in its infancy (see below), but under the term of "Gravoastronomie," the field was already contributing to set the stage for the emergence of multi-wave multi-messenger expansion within a cluster of institutes of the Max Planck Society (Kafka and Meyer 1972).[81]

"The future of gravitational-wave astronomy looks bright whether or not Weber is actually detecting gravitational radiation," remarked Press and Thorne as early as 1972. Such hopes were embedded in the wider awareness that the windows of

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[77] It seemed "appropriate" to publish the final negative results because—they claimed—"our experiment was as similar to Weber's as possible, whereas all other coincidence experiments deviated in one way or the other [...] Moreover, we think we have set the lowest limits obtained by Weber-type experiments over a reasonably long period of observation" (Kafka and Schnupp 1978, 97).

[78] For a discussion on the negative results from the different groups and the response to Weber's claims, see (Saulson 1998).

[79] The guests visiting each year the Institute for Physics and Astrophysics, also including the Institute for Extraterrestrial physics, grew from around 30 at the beginning of the 1960s up to around 100 in the early 1970s.

[80] See W. Heisenberg to E. Amaldi, April 16, 1973 and other correspondence related to the 1973 Solvay conference (AMPG, NLB, No. 30).

[81] Significant levels of gravity waves are produced by bulk motion of huge amounts of mass and transmitted almost undisturbed through all forms and amounts of intervening matter. Information carried by gravitational waves is thus complementary to the information carried by electromagnetic radiation.

observational astronomy had become broader, now including "along with photons from many decades of the electromagnetic spectrum, extraterrestrial 'artifacts' of other sorts: cosmic rays, meteorites, particles from the solar wind, samples of the lunar surface, and neutrinos" (Press and Thorne 1972, 335). After a few years, despite the failure of all experiments searching for gravitational waves, and despite the "apparent disagreement between the results of Weber's experiments and those of other workers" leading to a great amount of controversy, the status of the field appeared so well established that Ronald Drever could point out that "the consensus view that Weber's results are not due to gravitational radiation seems to me so likely to be correct that it is more profitable to concentrate now on development of detectors of very much greater sensitivity" (Drever 1977, 16).

By the completion of the "first-generation" detectors around 1975, the design and early development work for a "second generation" was already underway.

7 The Transition from Resonant Bars to Laser Interferometry: An "Original Sin"

The first wave of experiments—as well as the discovery of pulsars and the longstanding aim to detect gravitational radiation pulses produced in catastrophic collapse of stars resulting in supernovas or black holes—had prompted other researchers to propose alternative detectors designed to search for such short pulses, claiming higher sensitivities than those of Weber's original experiments, but within about an order of magnitude of them. A first obvious approach involved using larger bars of aluminum—or new types of material—and cooling them down to very low temperature (2 K or less, near the absolute zero) to reduce thermal noise, measuring their oscillations by totally new types of mechanical/electrical transducers. Developments in this direction had been proceeding for several years at Stanford University, at Louisiana State University, and at Sapienza University in Rome.[82] Another very challenging proposal from a technical point of view came from Braginsky's group at Moscow University: Instead of bars, they were experimenting the possibility of building relatively small gravitational-wave detectors using single sapphire crystals

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[82] Since early 1971, a long-term project for a second-generation detector of this type, a cryogenic resonant-bar detector, had been developed in Rome, at the Sapienza University, by Edoardo Amaldi and Guido Pizzella (Pizzella 2016; La Rana and Milano 2017; Bassan and La Rana 2017). William Fairbanks and William Hamilton at Stanford and Louisiana State University, respectively, and David Blair's team at the University of Western Australia in Perth were also developing cryogenic resonant bars cooled to liquid-helium temperatures, that is, about 270 degrees below zero on the Celsius scale. In 1975, studies on an interferometric detector were also initiated by Amaldi and Pizzella's group in two dissertations by Massimo Bassan and Livio Narici, a project that was abandoned after the graduation of the two students (Bassan and La Rana 2017).

weighing only a few kilograms, which should be very efficient in discriminating between thermal noise and gravitational-wave pulses.[83]

While these groups were concentrating on the problem of reduction of the background effects, an alternative approach to improving sensitivity could be obtained increasing the displacement caused by the wave. As the variation of the distance between the test masses induced by the passage of a gravitational wave is proportional to the distance between the masses, one must increase the separation between the test masses as much as possible. It is this extremely small change in separation, which has to be experimentally detected against a background of perturbing influences such as thermal and seismic vibrations.[84] The basic idea behind this new approach was to continually compare the lengths of the arms of optical interferometers by bouncing laser beams between pairs of mirrors at the ends of each arm, and then making the two beams converge on a point and overlap. In the absence of gravitational waves, the beams' electromagnetic oscillations cancel out. If there is a space-time disturbance caused by gravitational waves, the arms change length, and the laser beams no longer cancel each other out: light is detected.[85] Any relative distance changes in two optical paths at right angles to one another would

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[83] For a discussion on the different techniques and prospects for developing detectors of very much higher sensitivity, see especially section 4 and references therein in (Drever 1977).

[84] Weber's early resonant-mass antennae consisted of cylinders having the dimension of one to two meters, which should vibrate at a specific resonance frequency when put in motion by a transit of a gravitational wave pulse. These devices were supposed to allow detection of a change in the cylinders' length by about 10^{-16} meters.

[85] It is much more practicable to measure the distance between test bodies along one arm with respect to the distance between similar masses along a perpendicular arm, a layout particularly appropriate since the effect of a gravitational wave tends to cause the opposite sign of length change in the two arms. In a Michelson laser interferometer, a laser beam will be split into two identical beams by a partially reflecting mirror, with one beam reflected at 90 degrees from the first, but preserving the original frequency. Each beam travels down an arm of the interferometer and both are reflected back and merged into a single beam before arriving at the photodetector. As long as the arms do not change length while the beams are traveling, light waves will keep perfectly aligned canceling out in the recombined beam (totally destructive interference). Gravitational waves cause space to stretch in one direction and squeeze in a perpendicular direction simultaneously. For this reason, one arm of an interferometer will lengthen, while the other one shrinks and constructive interference pattern will be observed in the photodetector. If one arm gets longer than the other, one laser beam will take longer to return back creating a phase difference between the two beams, which will affect the interference pattern, showing that something happened to change the distance traveled by one or both laser beams. The interference pattern can be used to measure precisely how much change in length occurred and to extract information. The longer the arms of an interferometer, the smaller the measurements they can make. But this is an incredibly tiny effect, as gravitational waves, for example, can just change the length of a 4-km arm interferometer by 10^{-18} m, that is, 1/1000th the width of the classical proton radius. The trick is thus to create a longer light path that amplifies the gravitational-wave input to detectable amplitude: as long as the wave is passing, laser light in each arm bounces back and forth between the two mirrors hundreds of times before being recombined after such multiple passes. Nevertheless, detection of such small effect also implies that filtering out all possible sources of noise is one of the most challenging tasks for this investigative technique.

detect displacements due to a gravitational wave propagating in a direction normal to the plane of the system. The advantage of this method is that the mirrors, acting as test masses, can be placed kilometers apart, so that a gravitational wave induces larger relative motions. The changes in optical path might be further increased by reflecting each beam back and forward many times between each pair of masses to enhance displacement sensitivity. But a most important feature of interferometer antennae—which are potentially more sensitive than resonant-bar detectors—is that they are *inherently broadband*, being also sensitive over a much wider range of frequencies than had been practicable with bar detectors, and can detect and measure the wave forms of all classes of sources. However, laser systems of course had also the disadvantage of being technologically more complex and, in particular, *more expensive* than bars.

The seeds of this idea, as an alternative to Weber's antennae, can be found in a paper by the Soviet scientists Gertsenshtein and Pustovoit published at the beginning of the 1960s (Gertsenshtein and Pustovoit 1963), but the pioneer of this technique was Robert L. Forward, who got his PhD in Physics from the University of Maryland in 1965, collaborating in the building and operation of Weber's bar antenna. Apparently the concept had been discussed within Weber's group around 1964 (Collins 2004, 265–266), but Forward was the first to build a small size interferometer in the late 1960s at Hughes Aircraft Company Research Laboratories in Malibu, and put in operation the first prototype detector in 1971 (Moss et al. 1971), improving it until 1978. He used a Michelson interferometer to look for changes in separation of masses about 3 meters apart. The sensitivity achieved was of order 10^{-13} cm in this baseline, which although inferior to that obtained with bar detectors designed for millisecond pulses, was encouraging in such a relatively small and simple system. He demonstrated that this idea could really work but did not obtain funds to move to a more sophisticated instrument.

Simultaneously, Rainer Weiss from MIT had been actively exploring the idea of laser interferometry as a better chance of detecting gravitational waves since the end of the 1960s, starting a very detailed theoretical analysis of the ultimate sensitivity and of the noise sources of an interferometer. Interestingly, Weiss's project was not specifically connected with Weber's experiments: "My intent was never to check on Weber. The thing that excited me the most was the pulsars [. . .]" (Collins 2004, 247).

After the failure of a first attempt in 1972, Weiss sent another funding application to the National Science Foundation (NSF) in August 1974, proposing the construction of a prototype interferometer with arms 9 meters in length.[86] Because

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[86] Weiss' 1974 proposal, as suggested by Collins, was most probably "heavily based" on material appearing in the 1972 Quarterly Report of the Research Laboratory of Electronics at MIT (Weiss 1972). When mentioning attempts throughout the world aiming to confirm Weber's results with resonant gravitational antennae similar to those of Weber, Weiss specified, "A broadband antenna of the type proposed in this report would give independent confirmation of the existence of these events, as well as furnish new information about the pulse shapes. The discovery of the pulsars may have uncovered sources of gravitational radiation which have extremely well-known frequencies

of Kafka's deep involvement in the analysis and evaluation of Weber's experiment, he was asked to be one of the reviewers of Weiss' project. Controversially—and he later admitted this was an unfortunate breach of trust in his role as a reviewer—he circulated the proposal among the experimental groups in Munich (Collins 2004, 276–277).[87] At that particular moment in time, the group was actually investigating the possibility of designing an antenna that was to be kept at very low temperatures—near absolute zero—to reduce thermal noise, in parallel with other technical improvements to improve sensitivity (Biermann and Pinkau 1974, 105). For people who had worked on resonant detectors, the natural thing to do would have been to cool the detectors with liquid helium.[88] Billing, Kafka, Maischberger, and Winkler were involved in long discussions on how to proceed further. They looked closely to cooled resonance detectors as well as to interferometers.[89] They were aware of the low-temperature resonance experiments, as well as of Gerstenstein and Pustovoit's early suggestion of using interferometers as a means of detecting gravitational waves and of course knew about Forward's pioneering

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and angular positions." He calculated that the flux incident on the earth from the Crab Nebula pulsar was much smaller than the intensity of the events measured by Weber, but the detection of pulsar signals could be benefited by use of the new techniques he was proposing in his antenna design, which could "serve as a pulsar antenna." At that time, Weiss was also leading a balloon experiment to study the microwave background radiation, which is described in the first part of the Progress Report.

[87] See also Peter Kafka: Interview by Harry Collins available at http://sites.cardiff.ac.uk/harrycollins/webquote/, accessed 6/10/2018. Kafka felt somehow as an outsider in that field and "didn't understand much about the experimental possibilities" and so he "had to talk to the experimentalists anyhow," and then it was unavoidable that they discussed all these things in detail. According to Collins's interview with Robert L. Forward (also available at the same URL), Maischberger was involved as a reviewer, too, and he immediately thought of carrying out the interferometric experiment himself. It is interesting to observe how previous publications show that Maischberger was familiar with laser technology because he had worked at measurements with optical radars, radiating pulses of monochromatic laser light for atmospheric research, before beginning his work in gravitational waves. See for example (Bramanti and Maischberger 1972).

[88] On June 1, 1974, Biermann visited Pizzella and his group at their laboratories near Rome: "We showed Biermann the EXPLORER cryostat we were assembling and he was impressed and concluded that we were too much ahead, thus the German group would have done better to continue the search for GWs with a different technique, say with interferometers. It is worth to notice how much the premises of such a decision were wrong, since it took thirty years to the Rome group for reaching a sensitivity which was halfway with respect to the initial goal" (Pizzella 2016, 297).

[89] "Peter Kafka and myself thought at that time about the main limits for the sensitivity of large-scale interferometers for strain measurements. We worked out the standard quantum limit for such interferometers (to my knowledge nobody had done that before), when the sensitivity is limited equally by photon statistics and radiation pressure fluctuations. With reasonable assumptions—km size armlength, 10 kg mirror mass and gigawatt light-power—we ended up with strain sensitivities of 10^{-24} for short pulses. I described our considerations later on in more detail in my talk at the conference on Experimental Gravitation, in Pavia, September 17–20, 1976 (*Gravitazione Sperimentale*, Atti dei Convegni Lincei 34, Pavia, 1977)." Walter Winkler, personal communication to the authors, March 23, 2019.

tabletop experiments (Gertsenshtein and Pustovoit 1963; Forward 1978). Walter Winkler well remembered the situation at the time[90] :

> We had decided to stop the Weber-bar experiment, when it was clear that nothing could be found—despite of highly improved sensitivity compared to Weber's experiments. We were about discussing how to proceed, when Peter Kafka told us very reluctantly that he had got Rai Weiss' proposal to review, that he voted very positively to the American funding agencies. We had not yet made a decision about how to proceed experimentally and we were talking to each other every day. It would not have made much sense just to keep quiet. We therefore decided to inform Rai that we knew about his proposal, which indeed was for us a strong push into the direction of interferometry. Peter talked with Rai about our situation, and they understood each other quite well. In addition, Billing phoned Rai and asked him whether he would mind if we start to work on interferometers. Rai answered: No, why should I?

Actually, David Shoemaker, at that time working in Weiss' group, was later a member of the Munich/Garching project for 2 years.

The interferometric technique was more complex than the Weber bar. However, the Munich/Garching group was so enthusiastic about Weiss's plans that they immediately determined it would be possible to replicate the interferometric experiment using in-house resources. Certainly, it was a courageous decision for researchers who had already given much effort in building Weber bars to start from zero again and explore a whole new technology.

A first indication of their intention of concretely moving toward a brand-new project can be found in the Annual Report of 1974: The development of more sensitive gravitational antennae was to use the principle of laser interferometry on which pioneering work started in Munich (Pinkau 1975, 103).[91] In March 1975, Kafka gave a talk at the International School of Cosmology and Gravitation in Erice, Sicily. He was very critical of Weber's results, showing that the current state of Weber bars (including the Munich-Frascati experiment), was a long way from achieving the optimal sensitivity required for detection. In mentioning the potential of extremely low-temperature detectors, Kafka pointed out that "laser interferometry with long 'free-mass antennas'" would be another avenue that seemed "worth exploring" (Kafka 1977, 239).

Kafka had been very positive in reviewing Weiss' proposal. However, much to their regret, again because of the failures of the American funding system to deal with research in controversial interdisciplinary fields, Weiss did not get the money from the National Science Foundation.[92] And so the original American project was delayed, while the Munich group quickly moved forward with the

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[90] Walter Winkler, personal communication to the authors, March 18, 2019.

[91] See also (Kafka 1974c) and (Billing 1977).

[92] Weiss's opinion was that "The proposal to the N.S.F. was unfavorably reviewed at the time most likely because it was too big a step from acoustic gravitational wave detectors ..." (Collins 2004, 275).

new project.[93] Later, according to Kafka, the Americans themselves would use the Germans' success (and the fact that the project proposal had been inspired by Weiss' leaked proposal) to receive funding, and over the next decades, to an ever-increasing extent, they eventually took back control over the largest effort in gravitational-wave detection experiments.[94] Walter Winkler recalled, "Rai Weiss stated in this respect: LIGO would not have been funded without the results from the Munich/Garching group."[95]

The Munich-Frascati coincidence experiment had achieved the highest sensitivities with room-temperature bars. The work on laser interferometry that began between 1974 and 1975 was a possibility to improve the sensitivity by several orders of magnitude.[96] If cosmic gravitational waves could be detected, it was likely that they would reveal properties of their sources, which could not be learned from electromagnetic, cosmic ray, or cosmic neutrino observations. By that time, all the different groups active in gravitational-wave detections around the world (at Moscow State University, Yorktown Heights, New York, Rochester, Bell Labs, and Glasgow), failed to confirm Weber's detections, and thus it became general belief that his data were not to be ascribed to gravitational-wave signals.[97]

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[93] As Weiss later recalled, "They had been working on the bar detectors—the same method Weber used—and things had come to the end of the line with that. Virtually everybody in the world who had built the bar detector was seeing nothing [. . .] And they were fetching around looking for the next step, and they were really turned on by this idea. So, they asked if there were people in my group who were working on it who would like to come to Germany. At the time, we hadn't gotten all the way, to where it was functioning. What happened, however, is that they started working on it. I mean, you can't stop people; you can't do that. And the Max Planck group in fact did most of the early development, because they had the money. I was always very jealous of that. They had the money, and they had a large group of very experienced professionals who had been working on Weber's kind of detector. And they went immediately into interferometers—this was about 1974, probably—and I couldn't go forward. So, I kept working more and more on the cosmic background radiation, because that's where I got money—having lost the military support." Rainer Weiss: Interview by Shirley K. Cohen, May 10, 2000. Transcript, California Institute of Technology Archives, Oral History Project, http://oralhistories.library.caltech.edu/183/, accessed 30/7/2019.

[94] Peter Kafka: Interview by Harry Collins available at http://sites.cardiff.ac.uk/harrycollins/webquote/, accessed 5/5/2019.

[95] Walter Winkler, personal communication to the authors, March 23, 2019.

[96] See the announcement about preliminary work on a laser antenna with improved sensitivity ("Vorarbeiten für Laser-Antenne mit wesentlich erhöhter Empfindlichkeit") in the section "Experimental Work" of the 1975 Annual Report (Kippenhahn and Pinkau 1976, 118). The group working on this project was formed by H. Billing, P. Kafka, K. Maischberger, L. Schnupp, W. Winkler.

[97] For an alternative view to Collins on this topic, see (Franklin 1994).

8 Parallel Astronomical Developments: A Violent Universe and the Indirect Observation of Gravitational Waves

In 1973, news about a new kind of signal, of unknown astrophysical origin, quickly spread through the scientific community (Klebesadel et al. 1973). The serendipitous discovery of gamma-ray bursts, intense fluxes of radiation clearly emitted in connection with catastrophic astrophysical events, not correlated either in time or position with any other known astrophysical phenomenon or object, generated a flurry of theoretical speculations. Several models were proposed, including shock waves in supernovae, stellar flares, collapsing neutron stars, comets captured by neutron stars, and many others.[98] The sources were likely to be associated with dwarf stars, neutron stars, or black holes. As with other transient astronomical observations, these could give clues to help understand the mechanism of the highest-energy processes in astrophysics—such as those associated with the final cataclysmic stages in stellar evolution. It was not until much later that this new message from the high-energy universe, not yet correlated to any optical event, would be connected to the simultaneous emission of gravitational radiation from cataclysmic events. At that point in time, it provided evidence of the possibility of radiation emitted from compact astrophysical objects, similarly to what was being hypothesized for gravitational waves.

Another astronomical discovery strongly encouraged the opening of a new hunting season. In January 1975, Russell A. Hulse and Joseph H. Taylor working with the radio telescope at Arecibo, Puerto Rico, announced the discovery of the first pulsar in a close binary system (Hulse and Taylor 1975). In opening up new possibilities for the study of relativistic gravity, it made it immediately clear that this system, now the most promising astrophysical source of gravitational waves, could provide the first test bed for strong field effects of general relativity.[99] Among the potential physical and astrophysical consequences, Einstein's theory predicts that over time such a system's orbital energy will be converted to gravitational radiation and the two stars will gradually spiral closer to one another as gravitational waves carry energy away.[100] The decrease of the orbital period (obtainable from the observed time variation of the pulsar period) would thus constitute "a test for the existence of gravitational radiation" (Wagoner 1975).[101] Such frontier astronomical phenomena

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[98] Börner proposed as an explanation the bremsstrahlung (or braking radiation) of a beam of relativistic electrons hitting a region of high proton density (Anzer and Börner 1975).

[99] Since 1962, it had been suggested that a double system with one white-dwarf component could radiate enough gravitational power to be detectable and even become a test for the existence of gravitational waves (Kraft et al. 1962).

[100] See previously mentioned article by Dyson written in 1963, when pulsars—and consequently neutron stars—had not yet been discovered, suggesting that a close binary system of neutron stars could emit a powerful burst of gravitational waves after coalescing of the two components (Dyson 1963).

[101] At the time, Ehlers stressed that "the state of the theory of gravitational radiation itself was by no means satisfactory; relativity could not properly be tested against the observations until relativists

were pushing Einstein's solar-system-tested general theory of relativity to explore a much wider environment. As a result, it was expected at the time that gravitational radiation might become a powerful tool for observational astronomy.[102] But its impact went well beyond. With the discovery of pulsars, quasars, and galactic X-ray sources, and the coincident expansion in the search for gravitational waves, relativistic gravity—which had always played a central role in cosmology—was assuming an important place also in the astrophysics of localized objects.

In March 1976, while observations with the Weber-type resonant antennae ended, a 3-meter interferometer was being built by the Munich group.[103] Their Weber-type coincidence experiment had been run between July 1973 and February 1976. Reporting the total result up to the dismantling of the detectors, they compared these with "future aims of gravitational pulse astronomy," the most interesting of which was expected to be the observation of stellar collapse:

> While the observation of 'weak' radiation, e.g. from close binaries, will be helpful for a confirmation of Einstein's theory (and a check of the approximation methods to solve its equations), the 'strong' radiation from final collapse will contain fascinating additional information about the behavior of matter at extreme densities, unobtainable in any other way (Kafka and Schnupp 1978, 103).

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sorted out the theory" (Directors of the Albert Einstein Institute 2008). See also (Ehlers 1974). Hulse and Taylor were awarded the 1993 Nobel Prize in Physics for the discovery of the binary pulsar.

[102] For a review of the history of the discovery of the first binary pulsar and a description of its immediate impact and its longer-term effect on theoretical and experimental studies of relativistic gravity, see (Damour 2015). Kennefick has given a full account of how the measurement of orbital decay in the binary pulsar PSR 1913 + 16 interacted with an ongoing debate among theorists about whether the quadrupole formula, expressing the rate of emission of gravitational wave energy by a system of accelerating masses, could give a reasonable approximation of the source strength of possible astrophysical sources of gravitational waves, especially binary stars. References to previous work are in (Kennefick 2017). Kennefick has remarked how by the mid-1970s most theorists accepted that binary star systems did generate gravitational waves, but still some experts doubted whether the quadrupole formula could be correctly applied to them. Ehlers and other theorists were in fact among the skeptics objecting that "a formula for the energy loss due to gravitational radiation of bound systems such as binaries had not yet been derived either exactly or by means of a consistent approximation method within general relativity, a view which contradicts some widely accepted claims in the literature [. . .] derivations presented so far either contain inconsistencies or are incomplete" (Ehlers et al. 1976, L77).

[103] The working group comprised Billing, Kafka, Maischberger, Schnupp, and Winkler (Kippenhahn and Pinkau 1977, 142). The new project was presented at the international meeting on experimental gravitation held in Pavia in 1976 (Winkler 1977). By 1977, the section reporting research on "Gravitationstheorie" had expanded into several diverse research lines, and the members of the gravitational wave group were dividing their attention between studying various different technical problems (Kippenhahn and Pinkau 1978).

The authors had compared the expected strengths and rates of gravitational-wave signals from core collapse in supernovae with sensitivities of the then current detectors, whose performance—according to their evaluation—could not be improved in order to be able "to detect events at the rate of several per year or greater." Consequently, they had decided "not to continue with (low-temperature/high-quality) Weber-type experiments, but rather with a Weiss-Forward type experiment," that is, a laser-lighted Michelson interferometer:

> It is hoped that the many new antennae, being developed now and during the next decade, will be able to detect a few gravitational collapse events per year, and thus provide most valuable information on extreme states of matter and final stages of stellar evolution which will otherwise remain hidden (Kafka and Schnupp 1978, 103).

In December 1978, the Ninth Texas Symposium on Relativistic Astrophysics, which had become the principal international meeting where relativists and astrophysicists met and discussed recent research, was held in Munich (Ehlers et al. 1980).[104] For the first time in the history of these series of meetings, a Texas Symposium was held not just outside Texas but also outside the continental United States.

An outline of research fields studied at the Institute for Astrophysics at that time included the old classic "battle horses" such as atomic and molecular physics, solar physics, comets, star formation, and end phases of star development, as well as the new relativistic sector: gravitational theory and relativistic astrophysics, quasars, supernovae and collapse, and gravitational waves (Kippenhahn and Lüst 1977).[105] Since the 1960s, much effort had been made concerning the modeling of collapsing stars and the evaluation of their gravitational radiation emission, as they had been considered the best candidates for the production of frequent and intense gravitational pulses. These topics were discussed at the meeting, with the program also including microwave background radiation related to the dense, hot initial stage of the universe, a survey of the gamma-ray sources, as well as various theoretical and observational aspects of X-ray astronomy linked to late stages of stellar evolution, particularly in binary star systems. One of the workshops organized within the different sections was dedicated to gravitational radiation. In a wide overview of the then current status of relativistic astrophysics and of the latest scientific developments discussed during the conference, Börner and Kafka remarked that the previous 15 years had also brought two Nobel Prizes, which had never before been received in astronomy.[106] They duly commented how "Relativistic astrophysics has

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[104] In Reimar Lüst's papers, see a proposal for the conference written by Jürgen Ehlers to Jürgen Buntfuss, German Research Foundation (DFG), on January 14, 1976 (AMPG, III. Abt., Rep. 145, Folder 230 "Korrespondenz Klaus Pinkau", Fol. 253). See also (Börner and Kafka 1980). The event received financial support from the Max Planck Society (through its President, Reimar Lüst), the German Research Foundation, and the Institute for Astrophysics directed by Kippenhahn. Support was also provided by the Bavarian government and the city of Munich.

[105] See also the previously mentioned annual reports from the 1970s/early 1980s.

[106] They were referring to the Nobel Prize in Physics 1974, awarded to Martin Ryle and Antony Hewish, for their research in radio astronomy and the discovery of pulsars. Jocelyn Bell, who had in fact been the first to observe and analyze the pulsating radio signal, was excluded from the prize.

no sharp boundaries. Its best definition is probably still the program of the Texas symposia" (Börner and Kafka 1980, 181).[107]

The Texas Symposium held in Munich was also the occasion of the first public announcement of the experimental evidence for the reality of gravitational radiation damping in the binary pulsar discovered by Hulse and Taylor, which was published shortly afterward (Taylor et al. 1979).

9 Scaling Up Interferometry: An Itinerant Gravitational-Wave Group in the 1980s

Ludwig Biermann officially retired in March 1975 but continued to be active at the institute. In promoting the gravitational-wave experiment, he had added a last fruitful item to his rich long-lasting legacy.[108] Starting around 1974, the Munich/Garching group had built prototypes of interferometric gravitational-wave detectors to find solutions for occurring problems and develop improvements for existing techniques and instruments—a new field of research in many respects.[109]

The second Nobel Prize for an astronomical discovery was awarded to Arno Penzias and Robert Wilson in 1978 for the discovery of the cosmic microwave background radiation.

[107] See also Kafka's two review articles (Kafka 1979a, 1979b).

[108] Biermann became emeritus on March 31, 1975. In the Annual Report, signed by his successor Kippenhahn, his instrumental role during almost 30 years at the Institute for Astrophysics in opening and promoting new research fields—ultimately leading to the foundation of new Max Planck institutes—was emphasized also recalling the impulse given with the establishment of the department for relativity theory and the promotion of the Munich gravitational experiment (Kippenhahn and Pinkau 1976, 112).

[109] Winkler has remarked, "For the first time the optical components have been individually suspended. A properly shaped laser beam is injected into the interferometer, the beams in the two arms have to be properly adjusted and are eventually brought to interference. Servo-systems have been developed and implemented to operate the interferometer at optimal interference—the dark output—and keep it there. The first interferometer was a rigid block, containing beam-splitter, two mirrors and Pockels cells to adjust the lightpath in the arms for destructive interference at the output port. With this rigid interferometer 4 noise sources could be identified: 1. Laser intensity noise, 2. Laser frequency noise, 3. Laser motions of the laser beam, 4. The so-called parasitic interferometer – a reflex superimposing with the original laser beam. In October 1978, 3-m arm length was started and delay lines were added to the arms to increase the optical pathlength, as proposed by Rainer Weiss." Walter Winkler, personal communication to the authors, April 27, 2019. He also emphasized that the operation and results obtained with the prototypes are described in his talk at the Ninth Texas Conference held in Munich in 1978, and in (Billing et al. 1979b). Described in detail are local and global control of mirror position and orientation, optical feedback for path difference stabilization, frequency stabilization of the laser light, influence of scattered light and resulting requirements for the setup. Specifications for the delay-line mirrors had been set up. Fluctuations in beam-geometry (lateral displacement, orientation, beam-width) and resulting spurious signals had been investigated for the first time. They made approaches to stabilize the beam in this respect by introducing a beam-symmetrizer, which led eventually to their invention of a mode-selector to stabilize the geometry of a laser beam.

Since the previous years, discussions on continuing research on the gravitational-wave experiment with laser interferometry were ongoing, also in view of Heinz Billing's retirement in 1982.[110] The committee on the future of Billing's group expressed the opinion that the prototype 3-meter gravitational-wave antenna was a project of fundamental importance and proposed continuing with the preliminary phase, during which the small interferometer would be tested in view of the more ambitious project for a 30-meter antenna (Rüdiger et al. 1987). The group worked hard to reduce several unwanted effects and developed innovative technologies that then had an impact on future gravitational interferometers.[111] They eliminated the excess noise reaching the shot-noise level in 1982 (the lower limit of detection set by the quantum noise effect originated from the discrete nature of photons and electrons), which meant they had "found out *all relevant noise sources*, understood them well enough, found means and ways to reduce them sufficiently for the shot-noise level at that time and compatible with the rest of the setup."[112]

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[110] The fate of Billing's group, still named "Numerische Rechenmaschinen," was discussed starting from March 1977 (CPTS meeting minutes of 08.03.1977, 22.06.1977, 01.02.1978, AMPG, II. Abt., Rep. 62, No. 1780, 1781, 1783).

[111] Some construction details of the prototype of 3-meter arm length used for early studies of noise and other disturbances, such as laser frequency instabilities, might restrict signal perceptibility (Maischberger et al. 1979; Billing et al. 1979b). The new project was presented in the internal report (Billing et al. 1979a). See also Heinz Billing et al., "The Present State of the Munich Gravitational Wave Experiment," in (Schmutzer 1983, 401). In the Annual Report for 1980, the 30-m device was presented as an intermediate stage to a 300-m arm-length interferometer (Kippenhahn and Fink 1981, 229). Ongoing research was presented in internal reports before being published (Rüdiger et al. 1980; Schilling et al. 1980, 1981; Maischberger et al. 1981).

[112] Walter Winkler, personal communication to the authors, March 23, 2019. An example in this sense, added Winkler, was the suspension of the mirrors: "When we started around 1974/1975 to suspend the optical components like mirrors or the beam-splitter as pendulums (in order to isolate them from mechanical noise), it was immediately clear that further isolation stages have been necessary to avoid the excitation of the different degrees of freedom well above the thermal excitation. Therefore we used right from the beginning several mass-spring components in series in addition to the pendulum mode. Later on we have used triple pendula. In the beginning we damped by so called local damping via coil/magnet systems several degrees of freedom, also that of the suspension point of the lowest pendulum. In addition we used coil/magnet systems and Pockels cells in the light path to keep the interferometer at its point of operation. Somewhat later we invented the shadow-meter: a small plate interrupting a part of a light-beam, which falls onto a photodiode. A motion of the plate changes the amount of light, falling onto the photodiode. Thus the photocurrent changes correspondingly to the relative motion. This shadow-meter was subsequently used by the other groups. As usual, the mirrors themselves had been fixed on some kind of mirror holders. When looking at the interferometer signal, we found huge noise contributions coming from the resonances between mirrors and their holders. Whatever we tried out – nothing really helped. One day Karl Maischberger said to me: why not suspend the bare mirrors in a wire sling, and thus avoid these ugly resonances? We did so, and immediately the noise level was much better. An ingenious idea, which nobody had thought of before. Then we had to invent means to adjust the mirrors properly and keep them there. Later on this arrangement was improved by the Glasgow group, suspending the fused silica mirrors on fused silica wires, and fix them together by silicate bonding, thus making up a monolithic component with high mechanical Q. At that time, we had also solved all the relevant problems such as stabilization of the laser beam

This first 3-meter prototype "was the best in the world for many years" (Collins 2004, 277).

With Billing's retirement, the heroic era of gravitational-wave experiments at the Institute for Astrophysics was coming to an end and at the same time, the development of laser interferometers was changing globally the scale of gravitational-wave experiments.[113] Moreover, the challenge to detect gravitational waves was creating a new chapter in the field of quantum electronics. By October 1980, a decision had been taken to move the gravitational-wave experiment group to the Max Planck Institute of Quantum Optics, which was founded on January 1, 1981.[114] In August of that year, an international meeting on quantum optics and experimental gravity was organized by the new institute and promoted by the

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in frequency (in the end relative to the light-path inside the interferometer) and geometry, servo systems, scattered light contributions, vacuum requirements, data acquisition etc. Otherwise we would not have got the shot-noise level as set by the laser-power at that time!"

[113] During the meeting of the CPT section on October 29, 1980, it was reported that the committee on the future of Billing's *Rechengruppe* was of the opinion "The gravitational wave experiment is of fundamental importance and therefore recommends that the preliminary experiment be continued. In the event that it proves to be promising at the time of Mr. Billing's retirement, the Commission asks the President to ensure that the main experiment is continued inside or outside the Max Planck Society." It was also noted, "This assessment was confirmed by foreign experts. Scientists from Caltech and MIT had advised the president in talks to continue the experiment. Based on the positive statements, the project should be continued in the Max Planck Society. Mr. Walther agreed to take the group into the Institute of Quantum Optics. Finally, the Chairman noted that the Group currently holds the top international position with its work" [translations by the authors]. CPTS meeting minutes of 29.10.1980, AMPG, II. Abt., Rep. 62, No. 1791. On March 10, 1980 Winkler gave a talk at the advisory board meeting at the Institute for Astrophysics about the status of the gravitational wave experiment. They had just reached the shot-noise limit of 50 mW laser-power and had found the fundamental importance of scattered light. Reimar Lüst, at that time president of the Max-Planck Society, was present and "was obviously ready to support the research after Billing's retirement in 1982." Walter Winkler, personal communication to the authors, April 4, 2019.

[114] In fact, the roots of the Max Planck Institute of Quantum Optics dated back to the establishment on January 1, 1976, of a Laser Research Group set up at the Max Planck Institute for Plasma Physics (IPP) as a result of an agreement between the German Federal Ministry for Research and Technology, as it was called at the time, and the Max Planck Society. The aim of such a group was to work on the development of high-power lasers and their application to plasma physics, chemistry, spectroscopy, and other fields. This issue was discussed at the meetings of the Max Planck Society's "Senatsausschuss für Forschungspolitik und Forschungsplanung" (Senate committee on research policy and research planning) in 1975 (see copies of the minutes in AMPG, III. Abt., Rep. 68 A, No. 151). The committee discussing the future of this group and its transformation into the Institute of Quantum Optics with Karl-Ludwig Kompa, Herbert Walther, and Siegbert Witkowski as Directors was formed on June 14, 1978 and during the CPT Section meeting of May 5, 1979, the final formal decision was unanimously taken (CPTS meeting minutes of 14.06.1978, 30.01.1979, 09.05.1979, AMPG, II. Abt., Rep. 62, No. 1784, 1786, 1787). In 1981, the research group was given separate status as the Institute of Quantum Optics, and the Research Group on Gravitational Waves became involved with the development of laser interferometers. The group at IPP initially had 46 members and quickly grew to 105, so that the space made available by IPP, including additional barracks, soon became too small. In 1986, when the institute moved to a dedicated new building, there were 184 staff members. See preface and Section 3.2.10,

NATO Advanced Study Institute on Quantum Optics and Experimental General Relativity. The meeting aimed at establishing links between physicists working in fields traditionally separated as quantum optics, experimental gravitation, and the quantum theory of measurement. Efforts to develop gravitational-wave detectors already underway in several laboratories around the world—and their quantum mechanical nature coming into play because of the weakness of the signals they were attempting to measure—were unifying these previously far removed areas. Joint discussions during the meeting offered the opportunity to "close the gap" (Meystre and Scully 1983).

In May 1982, when the gravitational-wave group became officially part of the Max Planck Institute of Quantum Optics in Garching, construction of a new prototype interferometer, which would have a 30-meter path, had already started. It was completed in mid-1983, but improvements continued to be made over the years (Max-Planck-Gesellschaft zur Förderung der Wissenschaften 1983, 701).[115] The scaling from the 3-m to the 30-m device worked exactly as expected, giving the confidence for building larger detectors. Weiss himself expressed the valuable efforts made by the group: "So the Max Planck group actually did most of the very early interesting development. They came up with a lot of what I would call the practical ideas to make this thing better and better."[116] He further remarked, "The Germans have found and solved problems which I had not even thought of!"[117]

In the meantime, the observation between 1974 and 1981 of the first binary pulsar system discovered in 1974 had clearly demonstrated that the orbit was slowly shrinking, following the curve predicted by general relativity for the loss of energy and momentum due to gravitational-wave emission (Taylor and Weisberg

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entitled "Messung von Gravitationswellen – eine Revolution in der Astronomie?" in (Max-Planck-Gesellschaft 1986).

[115] A description of the laser-interferometric project related to that stage of activities can be found in Billing et al. (1983) and Rüdiger et al. (1983). See also the later internal reports: (Schilling et al. 1984; Shoemaker et al. 1985; Schilling et al. 1984; Shoemaker et al. 1985, 1988). David Shoemaker, who joined the Garching group developing the laser interferometer, had worked with Rainer Weiss at MIT on the early 1.5-meter prototype (Livas et al. 1986). For a short history of the institute, see the preface of (Max-Planck-Gesellschaft 1986).

[116] Reiner Weiss: Interview by Shirley K. Cohen, May 10, 2000. Transcript, California Institute of Technology Archives, Oral History Project, http://oralhistories.library.caltech.edu/183/, accessed 19/1/2019.

[117] "When I was member of STAC [Scientific and Technical Advisory Committee] for Virgo—remembers Winkler—I recommended the people to build a prototype interferometer. The answer at that time was: We do not need one. We simulate everything on the computer. But: You will not find all relevant problems and a solution for them just by working at a computer! You never will think of the influence of scattered light (we have found it in connection with a reflex from an antireflectively coated area superposing with the main beam. I calculated the problem through and found that a few photons are in principle sufficient to produce spurious signals). Another example: geometrical motions of the laser beam relative to the interferometer cause signals via slight asymmetries. Nobody had thought of that before. We invented the mode-cleaner. There are many of those experiences we had, and which nobody had thought of before." Walter Winkler, personal communication to the authors, March 23, 2019.

1982). This progress, together with tremendous advances in experimental tests of relativity, contributed to stringently constrain or even rule out alternative theories, increasing the confidence of gravitation theorists that general relativity was the correct classical theory of gravity. Despite the fact that detection of gravitational radiation remained to be demonstrated, this was the first indirect proof of the existence of gravitational waves, providing strong support for decisions to start launch more ambitious projects.

10 Munich's Initiative (and Failure) to Build a Full-Size Interferometer

A new group in Italy, led by Adalberto Giazotto, was working at the University of Pisa from 1982 onward on a seismic noise attenuation system for very low frequencies—which were supposed to be emitted by several pulsars—in view of a future gravitational-wave interferometer.[118] The Italians began to discuss a joint project with the French group headed by Alain Brillet during the Fourth Marcel Grossmann Meeting on General Relativity held in Rome in 1985 (La Rana and Milano 2017, 191). The French had started in the early 1980s a prototype project in Orsay, near Paris, investigating lasers and the interferometric approach with the goal to operate a 5–10 m prototype within 5 or 6 years (Brillet 1984). Their complementary expertise led to an Italian-French collaboration and to the definition of a project for an interferometric antenna led by Brillet and Giazotto in 1989 (Bradaschia et al. 1990).

The group at Glasgow University, too, had moved toward the development of techniques for the detection of gravitational radiation using optical interferometry since 1975. Like in Garching, the strategy had been based on the hope that, once sophisticated prototypes of modest length had been operated successfully, the sensitivity to gravity waves could be improved fairly rapidly by scaling up the length of the arms, without making major changes in the instrumentation by which the length difference was monitored. For this reason, all effort focused on developing the monitoring instrumentation on prototype detectors of small arm length. At Glasgow, they had built and were developing a system with an arm length of 10 m. A special attention was paid to identifying all noise sources, understanding them thoroughly, and devising ways to remove them which would work not just

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[118] See Adalberto Giazotto: Interview by L. Bonolis, Pisa, December 18, 2006. Transcript in Bemporad and Bonolis (2012). Published online at http://static.sif.it/SIF/resources/public/files/uomini-quarks/giazotto.pdf, accessed 18/1/2019.

on the prototypes, but also on much larger future detectors (Robertson et al. 1982; Drever et al. 1983; Hough et al. 1983, 1984, 1986).[119]

In 1987, the detection of neutrinos from the supernova 1987A appeared to be a failed opportunity for gravitational-wave detections.[120] As Kafka commented, "None of the more sensitive of the presently-existing GW antennae was working on February 23rd 1987 when the supernova in the Large Magellanic Cloud went off—not to mention the more sensitive antennae planned for the near future. Otherwise the birth of GW Astronomy might have been registered" (Kafka 1989, 55).[121]

But the gravitational-wave community was laying the premise to get the proper sensitivity requirements for the future observation of such catastrophic events, since the two main candidate sources for detection of gravitational radiation were supernovae and coalescing of close binary systems composed of highly condensed partners, that is, neutron stars or—even more efficient—black holes. In Glasgow, they were considering the possibility of building a larger detector of arm length

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[119] In 1979, Ronald Drever took up a part-time appointment to Caltech, and full-time later, in 1983, leaving James Hough as the Glasgow leader. At Caltech, Drever started a project that was eventually funded.

[120] This was the closest observed supernova since the seventeenth century. It became the first instance of detection of neutrinos in such an event, reported by four underground laboratories around the world, all within 24 hours of the visual sighting. See (Woosley and Phillips 1988) and references therein. As Wheeler had already stressed during a conference on underground science in 1982 (Wheeler and Wheeler 1983, abstract): "At least one kind of supernova is expected to emit a large flux of neutrinos and gravitational radiation because of the collapse of a core to form a neutron star [...] The corresponding neutrino bursts can be detected via Cerenkov events in the same water used in proton decay experiments. Dedicated equipment is under construction to detect the gravitational radiation. Events throughout the Galaxy could be detectable, but are expected only at intervals exceeding a decade. Nevertheless, the next event could come tomorrow, so every attempt should be made to make the monitoring for such events routine."

[121] Kafka's contribution was included in the proceedings of a NATO Advanced Research Workshop that was held from July 6 to 9, 1987, in Cardiff, representing a snapshot of the state of the gravitational wave community's thinking and understanding in the summer 1987. Bernard Schutz, who had been the promoter of the workshop, following discussion he had in 1985 and 1986 with many of the principal members of the various groups building prototype laser-interferometric detectors, emphasized in the preface that even if most of the effort had been concentrated on the detector system, proposals being planned by the different groups would have to address also questions related to computer hardware required to sift through data coming in at rates of several gigabytes per day and what software would be required for this task. Moreover, given that every group had accepted that "a worldwide network of detectors operating in coincidence with one another was required in order to provide both convincing evidence of detections of gravitational waves and sufficient information to determine the amplitude and direction of the waves that had been detected, what sort of problems would the necessary data exchanges raise?" Schutz further remarked in the last lines of the preface: "None of us knows when the *first* gravitational wave will be observed in our detectors, but as the book shows, we are already looking beyond that momentous event to the establishment of *gravitational wave astronomy*, the regular detection and identification of gravitational waves from a great variety of different sources scattered throughout the universe" (Schutz 1989, preface). See also Schutz' discussion about possible strategies for maximizing coincidences between detectors in the United States and Europe in (Schutz and Tinto 1987).

approximately 1 km (Hough et al. 1984), and in Garching, after encouraging progress with the 30-meter prototype, the group was stepping up efforts in order to prepare for a big jump in size: a full-sized 3-km arm-length interferometer.[122] Preliminary investigations for this ambitious project ("Voruntersuchungen für den Bau eines großen Laserinterferometers zur Messung von Gravitationswellen") led by Gerd Leuchs at MPI for Quantum Optics, were financed by the German Federal Ministry for Research and Technology (BMFT) during the period 1987–1989.[123]

Both groups had gained considerable experience in the design and operation of prototype versions of interferometric detectors since the early 1970s. The experimental group at Glasgow had benefited from collaboration with a theoretical group led by Bernard F. Schutz at the University of Wales at Cardiff, interested in analysis of signals from such detectors, and in Garching, right from the start, the experimental group had been in close contact with colleagues at the Max Planck Institute for Astrophysics—where the gravitational-wave project was born—especially with the Department of General Relativity led by Jürgen Ehlers.

But during 1988 it became clear that the British proposal for a 1-km antenna would not be financed by the Science and Engineering Research Council (SERC).[124] Because of serious financial problems, the funding of such an expensive enterprise was in fierce competition with projects put forward by the astronomy/astrophysics community.[125] But still around 1989, in Germany, "the idea

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[122] The concept of the large antenna was described in (Maischberger et al. 1985). Albrecht Rüdinger presented the project at the Fourth Marcel Grossmann Meeting on General Relativity (Winkler et al. 1986). Plans for the large detector were described also in (Rüdiger et al. 1987; Maischberger et al. 1988). A definition phase, with an expected duration of between 1 and 2 years, was beginning. During this period, various questions would be clarified, including different technical issues as well as the choice of the site and a reliable estimate of the main cost items (Winkler et al. 1985). The report MPQ 96 was the precursor of a later report, the updated study MPQ 129, organized in three main parts preceded by a summary providing information about the content (Leuchs et al. 1987a). The English translation of these chapter summaries was included in a further report, MPQ 131 (Leuchs et al. 1987b). The authors thanked Peter Kafka for writing the first introductory part reviewing the physics and astrophysics of gravitational waves in the context of the proposed big antennae, which was also published in two articles in *Die Naturwissenschaften* discussing the general properties of the waves and the planned antenna sensitivity, in connection with the expected sources (Kafka 1986a, 1986b).

[123] As part of a series of high-energy physics projects, DESY (Deutsches Electronen-Synchrotron), the German national research center operating particle accelerators in Hamburg, was supporting the execution of the project as lead partner on behalf of BMFT (AMPG, II. Dpt., Rep. 66, No. 3122, 3853, 3868).

[124] For a general overview of ongoing projects and state of art of the field at the time, see (Hough et al. 1987).

[125] At the end of the 1980s, many conventional astronomers were still very suspicious and did not consider gravitational waves as something worth funding, a circumstance that influenced such decisions. The cosmic-ray physicist Alan Watson was one exception, and his support proved crucial in the 1990s, when the GEO collaboration joined LIGO. Bernard Schutz: Interview by Adele La Rana, CERN, August 28, 2017, and personal communication to the authors, 24 November 2019. This was also the case in Germany and was reflected in the Denkschrift (white paper), which should guide astronomical investments in the coming decades. Heinrich Völk's recollections of

of building a large interferometer crystallized to be physically highly interesting, technically viable [. . .] financially within range of becoming reality."[126] It could be envisaged a cost sharing between the regular budget of the Max Planck Society, a grant from the BMFT and a support by the state.[127] As the British group had given up plans for its own large project, a fourth partner was now in sight. And so, the Garching project for a 3-kilometer interferometric gravitational wave detector resurfaced in 1989 as a joint German-British proposal (Hough et al. 1989), strongly encouraged by the two funding bodies BMFT and SERC. In Appendix A of the proposal the two groups presented the results of a 100-hour period of coincident observation using the two prototypes at Garching and Glasgow (the 30-m and the 10-m arm length), which had been solicited by BMFT and SERC to show that such detector could be operated in the production fashion by the two teams working together. It was the first time that two detectors had been run continuously in a data-taking mode, demonstrating the potential for long-term operation of laser-interferometric detectors. But the beginning of a long period of economic recession in the United Kingdom, starting during 1990 and going on until spring 1993, would strongly influence the destiny of the German-British project. By mid-1991, it would become clear that the United Kingdom would not be able to contribute with funds at least for a couple of years. As we see in the following pages, this lack of British commitment had the effect of making the Max Planck initiative vulnerable during these crucial years: While the proportion of SERC financial contribution was relatively small (20 million as opposed to 100 million of the BMFT), the Ministry had been insisting since the 1980s that any large scientific project should be done

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the time he was editing the *Denkschrift Astronomie* (Völk et al. 1987) in collaboration with his good friend Peter Biermann, son of Ludwig Biermann, are very explicit about this point: "We also tried a little bit to get things which we found interesting, like Gravitational Wave astronomy. We had to fight hard to get that in! You cannot imagine what kind of... opposition you encounter in such a case. Anyway we put gravitational waves as one of the—not the most expensive—but the most important projects in this. I'm still proud of it, that we did that at the time!" Heinrich Völk: Interview by Luisa Bonolis and Juan-Andrés Leon, Heidelberg, October 9–10, 2017. About US astronomers and astrophysicists animosity toward LIGO and how they felt that the project was competing for their resources, see (Collins 2004, 500–504).

[126] Hermann Schunck: Written interview by Adele La Rana, May 14, 2019.

[127] Funds for the preliminary investigations of the German project were provided by BMFT and searches for a suitable site went on during the second half of the 1980s. See documents on the financing of a grant for "Voruntersuchungen für den Bau eines großen Laserinterferometers zur Messung von Gravitationswellen" starting in November 1987 and ending in December 1990 and for a second tranche covering the period from 1.1.1990 to 31.12.1992 (AMPG, II Abt., Rep. 66, No. 3853, 3868 and Rep. 68, No. 65). Collaboration with other European groups, in particular with the Glasgow team, is also mentioned in the proposal by the MPI for Quantum Optics to BMFT for the period 1990–1992, related to a requested sum of 4.184.500,00 DM (about 2 million Euros of today). See letter sent on December 13, 1989, from the Max Planck General Administration to DESY (addressed to Dr. Prünster), the research institution acting as project-executing agency on behalf of BMFT, in which it was specified: "We [MPS] are unable to put at disposal of the Institute [of Quantum Optics] additional funds for this project. We support the application and would be very grateful to you for promoting this project" (AMPG, II Abt., Rep. 66, No. 3853).

as an international collaboration; this financial impasse could be mobilized by skeptics of the gravitational waves enterprise in Germany to slow down its advance, diminishing the need for direct scientific confrontation.[128]

The gravitational-wave communities around the world in the early 1990s navigated a political-rhetorical minefield as they simultaneously presented their cases to the respective funding bodies: The argument was made by all of them for the need of several detectors around the world, which gave support to each other's projects; but at the same time, this was difficult to reconcile diplomatically with ambitions of global leadership. The Germans during these years were at a particular disadvantage to the French-Italians, while at the same time aiming to negotiate as equals with them based on the premise of having their own detector.

As stated in the preface of the joint proposal (Hough et al. 1989), it was expected that "all the long baseline detectors to be built [the LIGO project and the Italian/French Virgo project] will operate as part of a coordinated worldwide network." At that time, future prospects for the realization of a big interferometer still looked excellent. From 1990, the gravitational-wave project at the Max Planck Institute of Quantum Optics in Garching was led by Karsten Danzmann, who had come back from Stanford University, where he had moved in 1982 after his PhD at the Technical University in Hannover.[129] Gerd Leuchs, who had led the Garching group from 1985 to 1989, moved to work into industry and later became director at the Max Planck Institute for the Science of Light.

In September 1991, the German-British project, now named GEO, was presented at a meeting organized in Bad Honnef by Ehlers and Gerhard Schäfer as a 3-km arm-length interferometer to be built near Hannover, in the German state of Lower Saxony (Danzmann et al. 1992).[130] In March 1992, the French CNRS, the Italian INFN, and the German MPG signed an "Expression of common interest," an agreement to promote "an effective collaboration between the European teams in view of building and operating two antennas in Europe: the French-Italian project VIRGO and the German-British project GEO."[131] In Summer 1992, a 3-km GEO

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[128] See folder on the gravitational-wave experiment in the Archives of the Max Planck Society in Munich: Akten der Registratur und des Archivs der Max-Planck-Gesellschaft, MPI für Quantenoptik, Gravitationswellenexperiment III, 1991–1997, Aktenzeichen 18140907, Barcode 233163 (from now on ARMPG), Fol. 373–380.

[129] After working in plasma physics and astrophysics, Danzmann had turned to laser spectroscopy. Immediately after having listened to Danzmann's talk at a conference on laser spectroscopy in the United States, Herbert Walther, director of the Max Planck Institute of Quantum Optics, told him "Mr. Danzmann, you will come to Munich and work on gravitational waves!" Interview with Karsten Danzmann, March 29, 2018, Deutsche Physikalische Gesellschaft e. V., Stern-Gerlach-Medaille 2018, available at https://www.youtube.com/watch?v=tNTB74bFGuc, accessed 23/2/2020.

[130] See also (Lück and the Geo600 Team 1997; Völter 2016).

[131] ARMPG, Fol. 322–323. And while Heinz Riesenhuber, the Minister of Scientific Research, was favoring a pan-European solution, the proposed German (GEO) project "seemed not feasible in the current form," as from a memorandum dated October 19, 1992, prepared for a meeting to be held in Paris to discuss the European gravitational-wave project "EUROGRAV" with the participation of

interferometer was still part of a list of the detectors at that scale being planned in the world: the French-Italian 3-km Virgo (comprising 9 groups from both countries) to be built near Pisa, the American 4-km LIGO (Laser Interferometer Gravitational-Wave Observatory) project (approved in fall 1991) with scientists at MIT and Caltech, and a more recent Australian collaboration proposing a 3-km detector near Perth (AIGO, not yet approved at the time). Plans for these full-scale astrophysical observatories required the evolving of the prototypes from laboratory setups used to test new optical measurement techniques into stable astrophysical instruments. They were meant not to be "in competition with each other"; on the contrary, each of them was considered crucially dependent on the others, since it had been evaluated that "to fully unravel the information contained in the signals with respect to the source direction, time structure and polarization" required "a world-wide network of four detectors." The hope was that the network could be in place by the end of the 1990s, and that at the beginning of the next millennium they might be able "to mark the beginning of the age of Gravitational Astronomy" (Danzmann 1993, 19).

At a time of enormous expansion of interest in, and importance of, Einstein's theory of gravitation, the next major step should be the construction of a number of long-baseline detectors around the world. An array of detectors of this type was expected "to allow the observation of gravitational waves from a range of astrophysical sources, leading to improved insight in many areas including stellar collapse, binary coalescence and the expansion of the Universe."[132]

By 1992, the existence of gravitational waves had been revealed with high precision. Arrival-time measurements of the radio signals from the binary pulsar PSR 1913 + 16, running since 1974, showed an orbital-motion decay consistent with the gravity-wave emission according to general relativity with an accuracy better than 0.5% (Taylor et al. 1992). This timely result would provide further impulse to ongoing plans to build large-scale ground-based laser interferometers. Their large bandwidth would allow detection of gravitational waves from a very wide range of potential sources. The following year, the 1993 Nobel Prize in Physics was awarded to Russell A. Hulse and Joseph H. Taylor for the discovery of the binary pulsar PSR 1913 + 16. Subsequent observations and interpretations of the evolution of the orbit had opened up "new possibilities for the study of gravitation."[133]

However, more menacing clouds were gathering on the horizon for the planned British-German 3-km gravitational-wave antenna. In November 1989, the Berlin Wall had been opened after nearly three decades, marking the falling of the Iron Curtain, and in August 1990, the Reunification Treaty between the two German states

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France, Italy, Germany, Great Britain (and Niedersachsen), and which should "without any doubt" preview the building of two detectors (ARMPG, Fol. 391–392). See in the same folder several documents testifying high-level interactions aiming at consolidating such agreements.

[132] See preface in (Hough et al. 1989).

[133] The Nobel Prize in Physics 1993, NobelPrize.org. Nobel Media AB 2020, https://www.nobelprize.org/prizes/physics/1993/summary/, accessed 1/02/2020.

was signed. In mid-1990, BMFT formed a multidisciplinary advisory commission led by the theoretical physicist Siegfried Großmann, which was supposed to make recommendations about fundamental research in Germany. The commission worked from August 1990 to November 1991, and a 124-page long report was officially released in April 1992. Notwithstanding the *special sympathy* with which the Commission regarded the large-scale experiment of a gravitational-wave detector "because of its novel scientific objectives," also acknowledging its "*special charm*" due to its innovative approach to gravitation, "the smallness, possibly the still-undetectability, of the effect," was also highlighted in the report.[134] The search for gravitational waves was not the only field affected by this report, and there were hints of disciplinary rivalries in the outcome: On one side, BESSY II, the upgraded new electron storage ring producing synchrotron radiation for materials research purposes to be built in Berlin (more on this later), was considered a "high priority" initiative that already in July of that year the project got the "green light." On the contrary, fundamental particle physics did not receive a favorable treatment, but instead a financial horizon that "should not enlarge, nor back off in the next few years." It was easy to imagine "what a hard standing the three solid-state physicists in the commission had, to enforce this formulation." As for gravitational waves, the commission had not recommended "immediate implementation, but swift prosecution with intensive scientific discussion" (Dreisigacker 1992, 374).

Thus, also endorsed by the Großmann commission's report, BMFT took a position fully justified by the critical situation due to German reunification and the challenging responsibility in the process of restructuring East German science (Sabel 1993): The ambitious dream of a 3-km interferometer was definitely not to be considered a priority in respect to other planned physics projects, on which BMFT had started huge investments programs since the mid-1980s.[135]

The Max Planck Society had officially asked for support for the big project by end 1989/early 1990.[136] However, it became evident as time passed, that there

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[134] The Commission remarked that the "big requirements for extreme stabilization of lasers, mirror technology are promising high technical spin-off." It was also reported that the 1987 DFG *Denkschrift* on Astronomy (Völk et al. 1987) had recommended the gravitational-wave detector. A copy of the pages in the commission's report related to the project of building a detector for gravitational wave astronomy (pp. 76–78) can be found in the Archives of the Max Planck Society in Munich (ARMPG, Fol. 373–380).

[135] As recalled by Hermann Schunck (at the time director at BMFT and responsible for fundamental research especially for Physics), the Ministry neither got any budget hike to take up that new responsibility nor any extra personnel. He further emphasized: "But there was another reason of psychological importance. BMFT had started a huge investment program in physics in the middle of the 1980s, including building a huge HEP-accelerator (HERA) in Hamburg (with prominent participation of Italy), an enlargement of experimental possibilities for nuclear physics in Darmstadt, a new nuclear research reactor in Berlin, an X-ray satellite for the Max Planck Society and some more. All these projects proved to be highly successful, by the way. The political leadership of BMFT had the clear feeling that all this was enough for physics." Hermann Schunck: Written interview by Adele La Rana, May 14, 2019.

[136] See related documents in AMPG, II Abt., Rep. 66, No. 3853, 65.

would be no action on the side of the BMFT, notwithstanding many years of financial support for in-depth and outstanding preliminary investigations. Initially, during 1991, it had become already clear that BMFT would promote the project *only* in connection with a strong European cooperation.[137] However, BMFT was "still very undecided" and once the British were putting everything on hold for financial reasons, Max Planck people hoped to counteract such hesitating attitude with a multinational initiative involving the French and the Italians.[138] The project had actually prominent and constant support within the Society by its Vice-President Herbert Walther, Director of the MPI of Quantum Optics, who had asked Danzmann to come back from Stanford University and lead the gravitational-wave group. There was even an attempt to get support from Walter E. Massey, director of the National Science Foundation. In April 1992, at the time of the official release of the Großmann commission's report, Massey sent a letter to the Federal Minister Heinz Riesenhuber, describing the crucial importance of such a network of detectors, as "one of the outstanding opportunities in experimental science today," especially emphasizing how one of the important criteria used in their evaluation had been "optimal performance with a possible European detector site, assumed to be in Hannover, Germany." The answer came only next September, after several months: "As a number of proposals had been submitted regarding investment in basic research, a decision required detailed evaluation. Careful consideration of the priorities of basic scientific research has revealed that BMFT funding of the gravitational wave detector will not be possible in the foreseeable future. I regret that this project, like other projects concerned with interesting scientific topics, cannot be supported by BMFT."[139] This answer left little room for doubt on BMFT's intentions. However, it looked like the uncertainty in BMFT was arising from different interpretations of the Minister's directive: Not to start any new "big project."[140] Soon after, in a letter to Edmund Marsch, deputy Secretary General of the Max Planck Society, Danzmann, leader of the project, stressed that "The

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[137] See, for example, memorandum dated December 6, 1991 written by Daniel Cribier, head of the French project, following a meeting in Munich with Wolfgang Hasenclever, General Secretary of the Max Planck Society, Walther, Director of the Institute for Quantum Optics, and Danzmann, now leading the GEO project (ARMPG, Fol. 382).

[138] Hasenclever to Dieter Kind, president of the Physikalisch-Technisch Bundesanstalt, August 14, 1991, ARMPG, Fol. 387.

[139] Walter E. Massey to Heinz Riesenhuber, April 28, 1992; Riesenhuber to Massey, September 2, 1992, ARMPG, Fol. 311 and 301.

[140] Since the late 1980s, there had been a shift in German Federal research policy regarding the responsibilities of the Max Planck Society. In previous decades, it had often been the case that the MPG had taken over the stewardship of large infrastructural projects, including ground-based astronomical observatories and satellites. This had put the MPS at an advantage with respect to other national institutions, which increasingly protested their dominant position. New initiatives such as the Denkschrift of 1987 called for a more horizontal distribution of tasks leading to what was called "Verbundforschung," a form of organization that coordinated all German partners participating in large international projects. The gravitational wave interferometer had already been assigned funds under this scheme, making it very difficult to deny that a "large project" was on

Department [for Basic Research] is interpreting this decision in such a way that it is no longer possible to promote basic research in these areas within the framework of 'Verbundforschung'." For this reason, their new application was not allocated federal funds, "despite the opinion of the evaluation committee."[141] After a few days, Danzmann wrote again to Marsch: "We understand that in the current financial situation, the BMFT is unable to make a capital cost contribution to the construction of a gravitational wave detector."[142]

What looked like the last word on the question was written on November 17, when the state secretary Gebhard Ziller answered to Wolfgang Hasenclever, general secretary of the MPG. The latter had underlined how such big device was only aiming at fundamental research, but Ziller sharply answered that there would be priorities in the coming years and he very much regretted that he could not currently envisage further funding for the GEO project, but he hoped that the money spent so far had created the base on which MPG could and would continue to work to some extent on the issue of gravitational waves using its own funds for fundamental research.[143]

After the detection of gravitational waves in 2015, it became clear in retrospect that this was a missed opportunity, as described, for instance, in a public interview by Danzmann himself. He stated that in Germany the wrong decision was made: While the Germans developed much of the high technology for the detection, the Americans by investing in the buildings and vacuum chambers of the large detectors claimed the largest recognition (Hilbig et al. 2017).

The most dramatic account and interpretation of this retreat from a full-sized interferometer is given by Hermann Schunck, at the time ministerial director at BMFT and responsible for fundamental research especially for Physics: "This situation was a catastrophe for pledging for a new project, what my unit did after the letter had arrived. While discussing the project with our superiors I was bluntly told to take my hands off that subject altogether. This was a situation quite new and exceptional for me. I was used to be able to do my job basically on my own responsibility and judgment, within certain constraints, like budget and general policy, and with the general hierarchal decision process of a political bureaucracy. My immediate judgment what this drawback meant to GW research was: not just a usual missed opportunity like any other, but a missed star hour of the history of Physics in Germany. I understood the importance of GW as the last great prognosis stemming from General Relativity that had not been proven experimentally. And I had the clear vision that the first group to do this would travel to Stockholm.

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the horizon. For more on details on Verbundforschung, see our upcoming book on the history of astronomy and astrophysics in the Max Planck Society.

[141] Danzmann to Marsch, October 14, 1992, ARMPG, Fol. 298.

[142] Danzmann to Marsch, October 19, 1992, ARMPG, Fol. 299. Our translation.

[143] Hasenclever to Ziller, November 4, 1992, ARMPG, Fol. 281; Ziller to Hasenclever, November 17, 1992, ARMPG, Fol. 279–280.

Working as a research administrator you do not have many chances like that, if any."[144]

As historians, however, we must also understand that German Unification had changed circumstances in a truly dramatic fashion: By early 1993, it had become definitely clear that the British Science and Engineering Research Council would not fund the joint project "for financial reasons." Even in the United States, which inevitably served as a reference for investment in large research projects, the early 1990s was a particularly difficult period for physics, and one which for the first time featured an open conflict between different branches (particle-, solid state-, and astrophysics) for a reducing pot of resources: The Superconducting Super Collider was canceled in 1993 (Riordan et al. 2015; Martin 2018). Within astrophysics itself, gravitational-wave detection was perceived as competing with other projects in the field: The 1990s decadal survey, which guides American investment in astronomy and astrophysics (National Research Council 1991), was negative to gravitational waves, and the community was particularly allergic to the claim of efforts in the field aiming toward an "observatory" rather than a high-risk detection experiment. With conflicting interests on a "knife-edge," what ultimately saved LIGO was "pork-barrel" federal politics in the United States, which favored investment in the geographical sites in Washington State and Louisiana (Collins 2004, 489–511).[145] While in Germany the scientific communities were similarly split, the geographical-political circumstances following unification were much more disfavorable to gravitational waves: While there was strong regional interest in Bavaria and especially Lower Saxony, German federal research priorities were completely oriented toward areas formerly within East Germany. BESSY II (mentioned earlier) was ultimately pursued because it would be located in East Berlin; during the same period, the flagship institute of the Garching area, the Max Planck Institute for Plasma Physics, was forced to focus its further expansion in Greifswald on the East German Baltic Sea coast.

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[144] Hermann Schunck: Written interview by Adele La Rana, May 14, 2019.

[145] After listing some potential causes for the final decision ("skill at lobbying of the various parties," Washington's need of big science because of its prestige, or because "a congressional staffer explained that the cost of LIGO was only an accounting error in the size of budget they were dealing with") Collins remarks, "So why was LIGO funded? [...] The sociological interest is that the forces of all kinds were roughly evenly balanced, so the funding decision could have gone either way [...] The funding of LIGO was an immensely important issue to the scientific community, which works with the NSF, but it was not an immensely important issue to those who were actually providing the money. None of the energy and hard work that both the pro-LIGO side and the anti-LIGO side put into presenting their cases was wasted, but the net result was an even balance [...] So, there is no big story about the funding of LIGO except the story that put the funding on a knife edge" (Collins 2004, 509–510).

11 GEO600: A Retreat from Full-Scale Experimentation to Focus on Instrumental Developments

After all those efforts, with ongoing plans for similar large-scale antennas both in Europe and the United States, the German and British teams who had pioneered research in the field since the beginning of the 1970s, and had a longstanding collaboration being both active building prototype interferometers, were deeply disappointed and felt they could not renounce. They struggled to find an alternative strategy, as pursued by Karsten Danzmann, who by the time of the failure had already been appointed as professor in Hannover with the perspective of the full-scale experimental enterprise, before it was known that even the funding from the state of Lower Saxony had been reallocated.[146]

The arm length, a most important design parameter, turns out to be the major cost factor: the cost of civil engineering and of the vacuum system being approximately proportional to the length, making up close to 70% of the total cost. Thus, a reduction in arm length would cut down the detector cost considerably, making the plan to build a much smaller facility a realistic aim for the British-German teams. Max Planck scientists thus joined forces with British researchers to build the smaller GEO600 "experiment," a 600-m arm-length antenna (Danzmann et al. 1994; Lück and the Geo600 Team 1997).[147] GEO600, whose construction began in September 1995, was designed on the basis of experience with two prototypes (in view of an interferometric detector with arms of a length close to 3 kilometers): the 10-meter interferometer at the University of Glasgow and the 30-meter interferometer at the Max Planck Institute of Quantum Optics. It had been decided that it should be something built "with their own hands," which could give them a chance to develop new technology, "piece by piece," and get money for each innovative "standalone project" from different sources, even from the BMFT. Crucially, the raised money was never explicitly for the *detector* itself, it was always for specific technology developments.[148]

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[146] Walther to Hasenclever, April 24 1992, ARMPG Fol. 271–2.

[147] In the meantime the whole funding structure had changed in the United Kingdom: the new Council was in favor of the gravitational wave project (Bernard Schutz: Interview by Adele La Rana, CERN, August 28, 2017), and so, GEO600 was funded by the Max Planck Society, the Volkswagen Foundation, the State of Lower Saxony, and by the Particle Physics and Astronomy Research Council on the British side. The project was soon presented at the first Edoardo Amaldi Conference held in Frascati, Italy, June 14 to 17, 1994 (Danzmann et al. 1995). On GEO600, see also (Völter 2016).

[148] During the year 1993–2000, this strategy was supported by Hermann Schunck at BMFT, who was able to channel "leftover" money from other German projects that had not been able to spend it for financing specific GEO600 needs justifiable as "standalone projects" such as vibration isolation, data acquisition, novel optics, laser stabilization, and novel vacuum system design (Hermann Schunck: Written interview by Adele La Rana, May 14, 2019, and personal communication to the authors, November 20, 2019).

In 1993, Danzmann had become professor at the University of Hannover and Director of the Institute for Atomic and Molecular physics, and at the same time, since 1994 he was also made leader of the new branch of the Max Planck Institute for Quantum Optics in Hannover.[149] This consolidated the leadership of the German side of the collaboration under the same person.

Despite the drawbacks, gravitational waves were now becoming a most important new field of research and one that spanned across several branches of the Munich "family" of institutes including Astrophysics, Quantum Optics, and, later, the two sites of Gravitational Physics in Golm and Hannover. In March 1991, the Max Planck Institute for Physics and Astrophysics had been split up into three independent institutes: the MPI for Physics in Munich (Werner-Heisenberg-Institut), the MPI for Astrophysics, and the MPI for Extraterrestrial Physics, the last two in Garching.[150] In 1995, a Max Planck Institute for Gravitational Physics named after Albert Einstein, the physicist who developed the theory of general relativity (Albert-Einstein-Institut, AEI), was founded in Golm, near Potsdam, with Directors Jürgen Ehlers and Bernard F. Schutz, who also remained part time in Cardiff.[151] Immediately after the foundation of the institute, Hermann Nicolai was appointed as third director at AEI.[152] The result of this further "cell division" (Trümper 2004) in the Munich area was the creation of a new astrophysics-oriented Max Planck Institute in the new *Bundesländer* following German unification.

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[149] ARMPG, Fol. 246, 212–213.

[150] Minutes of the 127th senate meeting in Frankfurt am Main, 08.03.1991, AMPG, II. Abt., Rep 60, No. 127.SP, pp. 23–24.

[151] The foundation of the Max Planck Institute for Gravitational Physics in the context of German unification was discussed in a dedicated committee (*Beratung über Aufnahme von Forschungsaktivitäten der MPG in den neuen Bundesländer nach der Vereinigung*) as of October 1990 (see CPTS meeting minutes of 2.10.1990, including a long report by the MPG President Hans F. Zacher, and minutes of 07.02.1991, 05.06.1991, 23.10.1991, 07.02.1992, 19.10.1993, AMPG, II. Abt., Rep. 62, No. 1821, 1822, 1823, 1824, 1825, 1830). In October 1991, Ehlers had in fact prepared a memorandum about his plans for such an institute that he had sent to Zacher. A special committee for the foundation of a Max Planck Institute for Gravitational Physics was formed on February 7, 1992. Works of the committee were reported during several meetings of the CPT Section (CPTS meeting minutes of 07.02.1992, 03.06.1992, 16.10.1992, 03.02.1993, 16.06.1993, 19.10.1993, 03.02.1994, 08.06.1994, 09.02.1995, AMPG, II. Abt., Rep. 62, No. 1825, 1826, 1827, 1828, 1829, 1830, 1831, 1832, 1834). Details about Ehlers' role and other aspects related to the phase preceding the actual decision to found the new institute—also connected to the abovementioned difficult period of the Institute for Astrophysics following Kippenhahn's anticipated retirement—can be found in (Hillebrandt 2013). For a related discussion on the German unification phase, see (Dreisigacker 1991). On the foundation of AEI, see also (Goenner and Hehl 1991; Goenner 2016). For the evolution of research on general relativity in Germany and its eventual institutionalization in the form of a dedicated research institute, see (Goenner 2017a).

[152] CPTS meeting minutes of 09.02.1995, 21.06.1995, 19/20.10.1995, 8/9.02.1996, AMPG, II. Abt., Rep. 62, No. 1834, 1835, 1836, 1837. Ehlers, Schutz and Nicolai led research activities, respectively, in general relativity, relativistic astrophysics, and quantum gravity/unified theories. Numerical relativity and computer simulations, also related to collapsing relativistic binaries and their associated gravitational waves, were an active part of the research activity since the very beginning of AEI, see for example (Schutz 1999).

The research program had its roots in several activities already ongoing during the previous 25 years at the Institute for Physics and Astrophysics: the foundations of general relativity, the quest for unifying general relativity and quantum mechanics, the study of neutron stars and black holes, and, of course, gravitational-wave antennae. In 1995, in parallel with the founding of the Albert Einstein Institute, the construction of the 600-meter arm-length detector was starting in Ruthe, a site 20 km south of Hannover. Methods for data analysis and simulations of possible sources were developed both at the University of Wales in Cardiff and at the Albert Einstein Institute in Potsdam. Soon after, this activity became one of the main research focuses of the new Institute with the decision to transform the already existing research center at the Max Planck Institute of Quantum Optics, based in Hannover and led by Karsten Danzmann, into a branch of the Albert Einstein Institute. The founding of a "center of excellence" for gravitational-wave research thus unified both experimental and theoretical activities under the same roof.[153]

In 2001, Danzmann was promoted to Director of the Laser Interferometry and Gravitational Wave Astronomy Division, the first of two divisions that were planned when the Quantum Optics branch in Hannover became officially part of the Albert Einstein Institute, which has since then sites in both Potsdam and Hannover.[154]

The move to Hannover itself, where Danzmann spent his early career, reflected the politics of increasingly large projects in gravitational-wave detection. While researchers at Garching had attained worldwide recognition for their development of multiple ingenious experimental methods for improving the sensitivity of the detection of the space disturbance, two other crucial factors were outside Bavarian control: the laser source itself and the site for the large interferometer. The laser development efforts were being concentrated in Hannover, in the group of Danzmann's scientific mentor Herbert Welling, and through his worldwide prominence in the field, Welling obtained the siting of the large interferometer for the state of Lower Saxony,[155] also facilitated by the promise of co-funding from the nearby Volkswagen Foundation (Grote 2018, 72–77).

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[153] In June 2000, the founding of a center for gravitational wave research was discussed during a meeting of the CPT Section. As stressed by Bernard Schutz, the whole operation would assure participation of the Max Planck Society with a cutting-edge role in the outstanding projects EURO and the laser-interferometric detectors LIGO and LISA, the Laser Interferometer Space Antenna mission, a giant interferometer to be placed in space. A committee was formed to examine the whole plan (CPTS meeting minutes of 07.06.2000, 19/20.10.2000, 15/16.02.2001, AMPG, II. Abt., Rep. 62, No. 1851, 1852, 1853).

[154] CPTS meeting minutes of 15/16.02.2001, 20.06.2001, 18/19.10.2001, AMPG, II. Abt., Rep. 62, No. 1853, 1854, 1855.

[155] Interview with Karsten Danzmann, March 29, 2018, Deutsche Physikalische Gesellschaft e. V., Stern-Gerlach-Medaille 2018, available at https://www.youtube.com/watch?v=tNTB74bFGuc, accessed 23/2/2020. Danzmann is considered part of the so-called Welling Laser Family (*Laserfamilie Welling*), and just a few years before, Welling had consolidated the region's footprint in this field with the establishment of the Laser Zentrum Hannover (Liftin and Mlynek 2009). For the latest account of his career, see: "Grosses Verdienstkreuz für Professor Herbert Welling", Presseinformation des Niedersächsischen Ministeriums für Wissenschaft, 31.8.2019,

Interestingly, this geographical move did not imply the geographical relocation of personnel, as it coincided with a most radical stage of generational renewal occurring during the 1990s: The experts from Bavaria from the founder years gradually transferred their technologies and practical know-how to younger members hired directly in Hannover, starting with Danzmann himself, who did not know much about the subject when he was first hired to lead the project. Over the course of the decade, as Bavarians reached the end of their careers, the positions freed by them were used to hire a new generation in Lower Saxony.

While the German 3-kilometer interferometer project had to be put aside in favor of the smaller GEO600, the American proposal for the Laser Interferometer Gravitational-Wave Observatory (LIGO), consisting in two widely separated long-based installations (4-km arms) within the United States, was funded, like the Italian Virgo.[156] The Virgo project for a 3-km interferometer was approved between 1992 and 1994 by the Centre National de la Recherche Scientifique (CNRS) and the Istituto Nazionale di Fisica Nucleare (INFN), eventually leading to the building of the Virgo interferometer at Cascina, near Pisa, beginning in the second half of the 1990s.

In 1997, the British-German collaboration finally entered in partnership with LIGO, becoming part of the worldwide network of gravitational-wave detectors and contributing to the next generation of US detectors with new advanced technologies (Abbott et al. 2004; Dooley et al. 2016).[157] A collaboration linking the LIGO detectors in the United States with its partners GEO600 in Germany and the Virgo detector in Italy was established in early 2007. Many of the technologies developed at GEO600 became thus instrumental in enabling the unprecedented sensitivity of LIGO and Virgo.[158]

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available online at https://www.mwk.niedersachsen.de/startseite/aktuelles/presseinformationen/grosses-verdienstkreuz-fur-professor-dr-herbert-welling-180202.html, accessed 23/2/2020.

[156] The LIGO construction proposal was approved by the National Science Board in 1990, and in 1992 the LIGO cooperative agreement for the management of LIGO was signed by NSF and Caltech, while construction at the chosen sites Hanford and Livingston began between 1994 and 1995.

[157] Since 2001, when the Hannover branch of the Max Planck Institute of Quantum Optics merged with the Albert Einstein Institute, GEO600 has been operated by AEI within the international collaboration with the Leibniz University of Hannover (which had been actively involved in the program through Karsten Danzmann) and the University of Glasgow and University of Wales at Cardiff and is now part of the worldwide network of gravitational wave detectors, including LIGO in the United States, Virgo in Italy, and KAGRA (Kamioka Gravitational Wave Detector) in Japan, which has been completed in October 2019. With two 3-km baseline arms stretching through tunnels under a mountain, it is the world's first interferometer of its size to be built underground. For a list of institutions and members of the LIGO Scientific Collaboration, see (LIGO Scientific Collaboration 2018).

[158] We cite here a series of articles related to the first realization by the German group of innovative detector technologies that made Advanced LIGO and Virgo so sensitive, such as the resonant sideband extraction, the automatic alignment as well as the power and dual recycling in suspended interferometer, the thermally adaptive optics, the detuned dual recycling, the contactless mirror refocusing, the automatic beam alignment, the radiation pressure interferometer calibration, the

At the same time, the European gravitational-wave community joined in the ESA-NASA space project LISA, the Laser Interferometer Space Antenna, consisting of three spacecrafts in heliocentric orbits, forming an equilateral triangle with 2.5-million-km sides, which would be complementary to ground-based detectors making it possible to detect the extremely low frequency ranges—and thus also different kind of sources—which are limited by noise of different origins (mainly seismic and Newtonian) affecting interferometric gravitational-wave detectors located on the Earth's surface (Danzmann et al. 1993; Rüdiger et al. 2001). LISA is a very long-term project, which at the time was expected to measure gravitational waves only several decades after its inception.

12 Open Questions: A Munich "Family Affair"' of Theory and Instrumental Specialization in a Global Scientific Race?

On September 14, 2015, at 09:50:45 UTC, 100 years after Einstein formulated the field equations of general relativity, the two detectors of the Laser Interferometer Gravitational-Wave Observatory (LIGO) simultaneously observed a transient gravitational-wave signal matching the waveform predicted by general relativity for the inspiral and merger of a pair of black holes of about 30 solar masses each. The signal caused the mirrors at the ends of each interferometer's 4-km arms to oscillate with an amplitude of about 10^{-18} m, roughly a factor of a thousand smaller than the classical proton radius. It was the first direct detection of gravitational waves after decades of experimental efforts and the first ever observation of a binary black hole merger (LIGO Scientific Collaboration & Virgo Collaboration et al. 2016), a culminating achievement in the long process of the renaissance of general relativity.

Events such as GW150914 are invisible for traditional astronomical instruments, as any signal other than gravitational waves is emitted near the merging black holes. But then, on August 17, 2017, four decades after Hulse and Taylor discovered the first neutron star binary, the Advanced LIGO and Advanced Virgo observatories made their first direct detection of a swell of gravitational waves from the coalescence of a neutron star binary system, which was followed after 1.7 seconds by a burst of gamma rays detected by the orbiting Fermi Gamma-Ray Space Telescope and INTEGRAL observatory (LIGO Scientific Collaboration & Virgo Collaboration 2017). The detection of this new gravitational-wave signal

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stable high-power lasers, the DC readout of signal recycled interferometer, the contactless mirror refiguring and the first realization, and routine operation of squeezed light in a large gravitational wave detector (Mizuno et al. 1993; Heinzel et al. 1996; Schnier et al. 1997; Heinzel et al. 1998; Heinzel et al. 1999; Lück et al. 2000; Freise et al. 2000; Heinzel et al. 2002; Lück et al. 2004; Grote et al. 2004; Mossavi et al. 2006; Seifert et al. 2006; Hild et al. 2009; Grote 2013; Wittel et al. 2014).

(GW170817) offered a novel opportunity to directly probe the properties of matter at the extreme conditions found in the interior of these stars, while the unprecedented joint gravitational and electromagnetic observation of this astronomical cataclysm was marking the beginning of a new era in multi-messenger astrophysics (Abbott et al. 2017a, 2017b). Violent astrophysical events where large masses undergo large accelerations (such as gravitational collapses or mergings of compact objects) can be very powerful sources of gravitational waves, a messenger of the high-energy universe together with cosmic rays, gamma rays, and neutrinos (Lipari 2017). The next step might be the associated detection of high-energy neutrinos from binary neutron star or black hole mergers, which are primary sources of gravitational waves. Given the sensitivity of the gravitational-wave signal to the neutron star structure, the new era of multi-messenger astronomy, with its growing synergy between astrophysics, gravitational physics, and nuclear physics, will also provide new insights into the nature of dense matter and into the properties of new states of matter at exceedingly high density and temperature.

Many of the instrumental innovations that eventually led to the first 2015 detection of gravitational waves using the LIGO detectors had been pioneered by Max Planck Institute researchers,[159] and they also played a key role in the computational tasks related to the detection efforts.[160] The LIGO Scientific Collaboration also includes two Russian groups from Lomonosov Moscow State University and from the Institute of Applied Physics, Russian Academy of Sciences.[161]

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[159] The principal Hannover/GEO contributions to LIGO include the following: the laser system; the demonstration of squeezed light at GEO600 and development of novel ideas to control squeezed light in GW detectors; techniques for lock acquisition and for alignment control of mirrors such as beam centering on wavefront sensors; and the demonstration of several technologies in GEO600, which lead to their adaptation in Advanced LIGO: signal recycling, electrostatic actuators, multi-stage mirror suspensions with monolithic last stage, squeezing application, and thermal compensation systems to shape mirror geometries. The Hannover/GEO contributions to Virgo include the following: parts of the laser technology; beam centering technology for the automatic alignment system of mirrors; and the squeezed light source and corresponding support in operation at Virgo today.

[160] The development of highly accurate analytical and numerical models of gravitational-wave sources—in particular of gravitational waves that neutron stars or black holes generate in the final process of orbiting and colliding with each other—have allowed extraction of astrophysical and cosmological information from the observed waveforms. These waveform models are then implemented and employed in the continuing search for binary coalescences. To significantly increase the probability of identifying gravitational waves in LIGO and Virgo data, the search for burst-like events in turn requires detailed knowledge of the expected signals from different sources and such search tools are sensitive because of systematic development in the algorithm and methods. Numerical relativity simulations with supercomputers not only play an important role in predicting gravitational waveforms that are used for gravitational wave detection, but allow in general exploration of general relativistic phenomena and other high-energy phenomena, such as gamma-ray bursts and stellar core collapse, or mass ejection with related nucleosynthesis processes.

[161] Both groups have been responsible for separate functional units of the LIGO detectors. Research performed by the Braginsky group at the Physics Department of Moscow State University since the early 1990s also made a significant contribution to the development and fabrication of LIGO

After such breakthrough events, an open question already addressed by the gravitational-wave community became more pressing: What were the reasons for the failure of early attempts at an extended European collaboration aiming at a ground-based twin interferometer project including the French-Italian-born Virgo? (La Rana and Milano 2017, 194).[162]

This very preliminary outline of some main aspects of the story behind gravitational waves has helped shed some light on to what extent further wider and in-depth historical studies are needed to reconstruct the dynamics of such a missed opportunity to realize a European network of gravitational-wave telescopes that might have followed and matched the successful example of effective cooperation in the CERN enterprise.

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gravitational wave detectors. After first starting research in the field of gravitational physics in the early 1970s, Braginsky's group began to work on gravitational wave laser antennae in the early 1990s. The main aspects of this research are described in (Braginsky et al. 2016). On the contribution of the Applied Physics Institute, see (Khazanov and Sergeev 2017). For a summary of experimental research on the detection of gravitational radiation performed in the Soviet Union, see (Rudenko 2017).

[162] See especially La Rana's Chap. 10 in this volume.

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The Origins of Virgo and the Emergence of the International Gravitational Wave Community

Adele La Rana

1 Introduction

On September 14, 2015, almost 60 years after Joseph Weber's first proposal of resonant bar detectors for picking up gravitational waves (Weber 1960), the two American interferometric antennas comprising LIGO (Laser Interferometer Gravitational Observatory) recorded the first detection of a gravitational wave signal. A chirping waveform lasting about two-tenths of a second with a frequency spanning rapidly from 35 Hz to 250 Hz hit the detector in Livingston, Louisiana, and about 7 ms later, the one in Hanford, Washington, 3000 km away (Abbott et al. 2016).

The tiny signal, corresponding to a time-dependent variation of the distance between the mirrors in the interferometers of about 10^{-18} m, matched perfectly the waveforms predicted by general relativity (GR) for the gravitational radiation emitted in the merger of two massive black holes, in the very final stage of their life as a binary system: a quasiperiodic sinusoid with rapidly growing amplitude and frequency, called a *chirp*, like the sound produced by a sparrow. The gravitational radiation identified and analyzed by the LIGO-Virgo collaboration was generated 1.3 billion years ago by the collapse of two black holes of about 30 solar masses each, inspiralling one toward the other at about half the speed of light.

52 years before this first detection, P. C. Peters and J. Mathews calculated for the first time the gravitational signal radiated by a system of two point masses, moving around each other under their mutual gravitational influence (Peters and Mathews

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1963). Those were the lively years when relativistic astrophysics was born and when Weber's first experiments were bringing discussions about the physical reality of gravitational waves (GWs) from the theoretical to the empirical domain (Weber 1960, 1962; Collins 2004). In the same year, 1963, Freeman Dyson pointed out for the first time that if neutron stars were proved to exist, a binary system of neutron stars would emit, in the last seconds before plunging into one another, a violent pulse of gravitational radiation, "which should be detectable using Weber's equipment or some suitable modification of it". He added that "it would seem worthwhile to maintain a watch for events of this kind" (Dyson 1963).

The astronomical evidence for the existence of neutron stars came 4 years later, from the observations of the first pulsar (Hewish et al. 1968). The detection of gravitational radiation from a binary neutron star merger was accomplished for the first time on August 17, 2017, by LIGO and their European companion Virgo located near Pisa, Italy, while about 2 seconds later, a short gamma-ray burst was observed by the gamma-ray space telescopes Fermi (NASA) and Integral (ESA). The triple-coincidence GW detection allowed the source to be located in a limited patch of the sky and thus to select and observe the remnant kilonova a few hours later by optical telescopes; the discovery officially inaugurated multimessenger astronomy (Abbott et al. 2017).

Hypothetical astrophysical sources such as black holes were one of the fields of theoretical investigation gradually growing in the fecund years of the so-called Renaissance of GR (Will 1989; Eisenstaedt 1989) and especially through the following period, thanks to the pioneering work of many theoretical physicists such as David Finkelstein, Roy Kerr, John Wheeler, Roger Penrose, Stephen Hawking, Kip Thorne, Remo Ruffini (Thorne 1994). Interferometric antennas, the third-generation of GW detectors after Weber's resonant bars and their advanced cryogenic successors, have now allowed for the first direct detection of these astrophysical monsters, identified so far only indirectly by the effects of their strong gravity on surrounding matter (Pizzella 2016). In some sense, a significant phase started during the Renaissance of GR has come to an end; GWs have been detected, allowing for the first time the observation of black hole mergers and collapsing binary neutron star systems.

Among the scientific investigations started during the Renaissance period of GR, the field of GW research in particular demonstrates the process of how Einstein's theory was established as a branch of physics in its own right from the 1950s onward. On the theoretical side, the formation of a consensus around the reality of GWs immediately after the Chapel Hill conference is one of the clearest signs that physicists started trusting the physical implications of the theory in that period (Kennefick 2007). On the experimental side, the research activity for GW detection constituted a fundamental part of the search for GR's identity as a physical theory, aiming at testing its range of validity and its modeling and predictive power.

Stimulated by Weber's claims of having detected the first gravitational signals in 1969 (Weber 1969) and by the observations of the first pulsars, in the early 1970s, the search for GWs spread to different labs in the USA and in Europe. Furthermore, the discovery of the first binary pulsar made by Russell Hulse and Joseph Taylor in

1974 provided an extraordinary laboratory for testing GR theories and proving the physical reality of GWs (Damour 2015).

Over a period ranging from the mid-1960s to the mid-1970s, named by Kip Thorne the *Golden Age* of GR, several events contributed to stabilize the processes activated in the Renaissance period, including the birth and rapid growth of relativistic astrophysics and the formation of the International Society on General Relativity and Gravitation in 1974. The gradual affirmation of the GW research field can be seen as part of this process of stabilization, although the establishment of a specific international organization devoted to experimental GW research took many more years, rising with the development of the long-baseline interferometric detectors and the need for a coordinated worldwide network to build and operate them. The GWIC (GW International Committee) was born only in 1997 and the first regular and dedicated conference, the Edoardo Amaldi Conference on GWs, began well into the 1990s. The cutting-edge science and technology needed to build the new generation of detectors required a change of scale in the investment of money and people. This mobilization of scientists motivated the establishment of a well-identified and organized community, with its own specialist gatherings, inside the wider scientific circle of GR researchers.

The advent of long-baseline interferometric detectors has projected the field of gravitational waves into the cosmos of Big Science. The transformation took place at the turn of the 1980s and 1990s, with the gradual breakthrough of the LIGO and Virgo projects and proposals from other countries such as Australia, Japan, Germany and the United Kingdom. Analyzing this gradual shift is particularly interesting from the point of view of the contents of this book, because it allows looking at a long-term evolution of the Renaissance of GR, offering a new and original historical perspective. Particularly engaging is the European context of this transformation, whose history is still unexplored.

This chapter looks at the history of GW research as a case study to better understand and define the stabilization process of GR, only alluded to in the historiographical framework put forward by Blum, Lalli, and Renn (Blum et al. 2015, 2016). The gradual emergence of GWs as a true physical phenomenon deserving experimental exploration during the Renaissance period and, in the following years, the spreading of this new experimental activity around the world are examples of how the GW research trajectory symbolizes the evolution of Einstein's theory, from its rebirth to its effective rooting in the mainstream of physics. The development of the big interferometric projects in the early 1990s and the contemporaneous birth of a specific international GW community can be seen as the distant offspring of the Renaissance period. The entrance of GW experiments into Big Science, with the launch of the projects LIGO and Virgo, testifies to the stabilization of the research field that one can interpret as part of a wider long-term process of stabilization of GR. This chapter therefore assumes a long-term horizon and critically analyzes, in particular, the origins of the Virgo endeavor, dating back to the 1980s, in its European and international contexts. In those years, the first proposals for long-baseline interferometric detectors were being discussed and presented in Europe and in the USA and were strongly linked to the need to create a coordinated network of antennas—including the optimistic plan of building an array of three interferometers in Europe alone.

2 Some Internal Reasons for a Historical Perspective on the Origins of Virgo

Despite being an extraordinary scientific achievement signed by both parties to the LIGO-Virgo scientific collaboration, the first GW detection in September 2015 has not been perceived by the Virgo scientists as a complete success, because the Virgo interferometer was at that time turned off during its upgrading phase to Virgo Advanced. The detection was accomplished only by the LIGO facilities, while the complex data analysis was a joint effort of the entire collaboration, made by about 1000 scientists from 133 scientific institutions belonging to 18 different countries worldwide.[1] The final paper of the first detection was signed by LIGO and Virgo collaborators, on the basis of a 2007 agreement on the full sharing of data, joint data analysis, and common publications established among the scientists behind the two interferometric detectors.[2] However, during the 20 years required to build, operate, and upgrade the interferometers in the USA and Italy, scientific cooperation occurred at many different levels and often people working for one experiment would later join the other, sharing expertise, data analysis formats, and technologies. For example, the high-purity quartz glass mirrors of LIGO and Virgo with ultra-low absorption are fabricated by the German company Heraeus, polished to a precision within a tenth of an angstrom by the American Company Wave Precision, and the ultra-performing coating to render them reflective, assuring high homogeneity and low loss, is delivered by the Laboratoire Matériaux Avancés in Lyon, France. The three interferometers have grown up together, as well as the GW community around them. However, an obvious and fundamental asymmetry exists between the US and European gravitational wave research. The USA has two long-baseline interferometers, which opens the possibility of claiming detection on their own, while Europe has only one comparable detector and cannot accomplish an independent detection because at least two interferometers are needed in order to claim detection of a transient signal such as the one emitted by coalescing binary systems.

After the first detections were announced in 2016 and 2017, many questions arose among the scientific community about the missed opportunity of having an array of

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[1] The about 1000 scientists who signed the discovery paper (Abbott et al. 2016) work for different scientific institutions located in the following countries: Australia, Belgium, Brazil, Canada, China, France, Germany, Hungary, India, Italy, Japan, Korea, the Netherlands, Poland, Russia, Spain, UK, and USA.

[2] As described by Harry Collins in his book *Gravity's Kiss*, a daily account of the months following the first detection and preceding the announcement in February 2016, the signing of this agreement was not equally supported by all the scientists in LIGO and Virgo (Collins 2017).

two or more long-based GW interferometers in Europe. Such an opportunity was indeed discussed among the European groups during the 1980s and early 1990s in various ad hoc meetings, workshops, and conferences and several steps were made in order to establish a European collaboration of some sort, as we will see. However, these attempts to promote a European network of GW interferometers were not successful.

Virgo was born in the early 1990s as a French–Italian project, funded by the French Conseil Nationale de la Recherche Scientifique (CNRS) and by the Italian Istituto Nazionale di Fisica Nucleare (INFN). The construction of the 3-km arm length interferometer started in 1997 in the fields of Cascina, near Pisa, Italy, and was completed in 2003. Other European countries joined the Virgo experiment only afterward: National Institute for Subatomic Physics (Nikhef) in the Netherlands entered the Virgo collaboration in 2006, while the Polgraw group in Warsaw and the KFKI Research Institute for Particle and Nuclear Physics (RMKI) in Budapest joined in 2010. The first Virgo scientific run was accomplished in 2007, starting on May 18 and ending 4 months later, during which period the memorandum of understanding between Virgo and LIGO was signed.

This gives rise to several questions. Why was Virgo not born as a European project instead of a French-Italian endeavor? Why would Europe only build one long-baseline interferometric detector if detection needs at least two? These issues have been strongly questioned by the scientists and stakeholders of the GW community, especially after the first detections made by the LIGO interferometers.

3 The Need for an Array of Detectors

International collaboration is a fundamental feature of the research field of GW detection because of the specific nature of the investigated phenomenon. The expected signals are so weak compared to the different disturbances acting on the detector that they are almost concealed. However, the extremely low interaction of gravitational radiation with matter also implies that the same GW will pass through different detectors located worldwide basically unmodified, i. e., without losing any energy or information about its source. The experimental problem is to extract this tiny signal, which travels at the speed of light and thus hits all detectors almost at the same time, from the detector noise.

In order to identify a transient GW signal in a stream of experimental data, coincidence analysis of data streams coming from different detectors is needed. If two detectors are far from each other, their noises are mostly uncorrelated and thus coincidence analysis can cancel out the local noise from the data stream. In this way, the probability of a random coincidence among the detectors is reduced.

Micro-creeps in the mirror-suspending systems, sudden external mechanical or electromagnetic disturbances, and the so-called non-Gaussian noise mimic very well the signals that would be expected from GW bursts. The external disturbance can be identified only in some cases, allowing the corresponding event to be discarded. In most cases, however, the identification is impossible: the only possibility is to match coincidences between different detectors, in order to cancel out local noise.

The need for an array of detectors, however, is not motivated solely by the aim of accomplishing the detection of a GW signal; coincidence analysis is necessary to extract from the data precious information about its astrophysical source. The dimensions of the antennas are small compared with the wavelength of the expected radiation, so coincidence analysis techniques involving widely spaced antennas must be used to localize the sources in the sky and to obtain useful astrophysical information. This strategy is similarly used in radio astronomy, which faces analogous problems, and is referred to as VLBI (very-long-base-line interferometer). In order to identify the arrival direction of a GW, the signal must be detected by at least three detectors, which can reduce incertitude on the position to a reasonably small patch in the sky. The time delay between the arrivals of the signal at any two detectors determines a circle in the sky, on a plane perpendicular to the line joining the detectors, in which the source must lie. With three detectors, there are two independent time delays, giving two circles on the sky, which will intersect in general at two different points. With four or more detectors, there would be a unique intersection region.

As pointed out in a note drafted in September 1987 by Bernard Schutz, professor at Cardiff University, Wales, and a major contributor in the British and German-British projects for interferometric detection, “More detectors mean capturing more gravitational wave events. This is partly because of greater sky coverage, but mainly because the decreasing risk of ‘random’ coincidences caused by noise allows one to operate with lower thresholds and therefore to see a greater volume of space. More detectors mean also a better reconstruction of the GW, therefore more physics and astrophysics.”[3]

The need of an array of GW interferometric detectors to make gravitational astronomy possible was a strong scientific argument supporting the 1980s proposals for long-based GW interferometers in Europe and in the USA. The huge effort required to build an array of antennas would be justified by the opening of a new field of astronomy by the possibility of accomplishing the first detection of gravitational radiation. The acronym LIGO, containing the word “observatory,” speaks to this story.

In his speech as a referee of LIGO in front of the members of the House of Representatives of the United States of America, on March 13, 1991, the physicist of Bell Laboratories J. Anthony Tyson argued (Tyson 1991, 6):

> In no sense could a single LIGO be called an astronomical “observatory”. This is because several high sensitivity LIGOs, spaced around the Earth, are required to unravel the information in the complex gravitational wave signature. [. . .] A single LIGO cannot tell what direction in the sky an event came from, and also has great difficulty in discriminating against local interference. This is therefore a natural project for international cooperation.

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[3] Note by Bernard Schutz, *Outline case for building 3 laser interferometric detectors in Europe*, September 1987. Personal papers of Alain Brillet, in the archives of the European Gravitational Observatory, Cascina (Pisa), Italy (PAB).

Tyson's speech was aimed at persuading the members of the House of Representatives that it was not worthwhile to invest so much money in such an ambitious project, which could not be assured of success and which was heavily depending on international cooperation to truly open a new branch of astrophysics.

Thus, the search for GWs is necessarily an international endeavor, which highlights the antinomic process of scientific competition/collaboration. The need for scientific cooperation drove the interactions among the teams in Europe and in the USA aimed at building GW interferometric antennas and also influenced the political strategies adopted to negotiate with the funding agencies. Therefore, looking at the history of gravitational wave detection is particularly interesting from the point of view of the international collaborations: both the ones that have been established and the ones that have been attempted.

4 The European Teams Working on Interferometric GW Detectors in the 1980s

During the 1980s, three small European teams were working on the development of laser interferometry for GW detection: the group at the Max Planck Institute for Quantum Optics, in Garching, directed by Heinz Billing and, after his retirement in 1982, by Gerd Leuchs from 1985 to 1990[4]; the team founded in Glasgow by Ron Drever and, after his leaving for Caltech in 1981, led by Jim Hough; and the team of Alain Brillet in Orsay.[5] These small European teams were pioneering in the field of laser interferometry and providing fundamental R&D results, which later

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[4] Gerd Leuchs had been called by Herbert Walther to lead the group, but his position was weak inside, as he was a newcomer in the field of GWs and also the youngest of the group. In 1990, Leuchs left the field of gravitational waves and Karsten Danzmann was appointed as the new project leader for gravitational wave research at the Max Planck Institute for Quantum Optics in Garching.

[5] The team in Garching was composed of Heinz Billing (retired in 1982), Walter Winkler, Karl Maischberger, Albrecht Rüdiger, Rolland Schilling, Lise Schnupp, David Shoemaker (1984–1986), and Gerd Leuchs (who joined in 1985 and left in 1990). For a historical perspective on the Garching team, see Chap. 9 by Bonolis and Leon in this volume and a book in preparation by the same authors, with the provisional title *History of Astronomy, Astrophysics and Space Science in the Max Planck Society*, recently submitted to Brill Publishers. The Glasgow group was composed of Ron Drever (who went half time to Caltech in 1979 and full time in 1984), Jim Hough, S. Hoggan, G. A. Kerr, J. B. Mangan, B. J. Meers, G. P. Newton, N. A. Robertson, and H. Ward. The group in Orsay was formed by Alain Brillet, Catherine Nary Man, Jean-Yves Vinet, David Shoemaker (since 1986 up to 1989, when he goes back to MIT), and Dan Dewey, a post-doc coming from MIT as Shoemaker. David Shoemaker met Alain Brillet at the Marcel Grossman Meeting in Rome in 1985 and at a conference on quantum optics of the Max Planck Institute held at Schloss Ringberg in the south of Germany. Afterward, he was invited to go to Orsay by Alain Brillet, to work at his PhD. So, in 1986 Shoemaker moved from Garching, where the group was facing many problems about the new leadership and organization after Billing's retirement, and joined the group in Orsay. Interview by the author with David Shoemaker, Geneva, August 27, 2017.

were critical in supporting the approval of the LIGO and Virgo projects in the early 1990s.[6]

After initial experiments with Weber-type room temperature bar resonators (1971–1975), the Garching group started to work in 1975 at developing Rainer Weiss's ideas on interferometric detection, building a 3-m prototype and being the first to reach the shot noise limit in 1982 (Weiss 1972).[7] This was a very important achievement, as it meant that they had found out "all relevant noise sources, understood them well enough, found means and ways to reduce them sufficiently for the shot noise level at that time and compatible with the rest of the setup".[8] By 1985, they were operating a 30 m delay line interferometer, again achieving the shot noise limit—now corresponding to a 10 times lower strain noise, consistent with the 10 times longer arm length (Shoemaker et al. 1986).

The Glasgow team had also begun research activity on bar detectors in the early 1970s and migrated to interferometric techniques at about the same time as Garching, developing during the 1980s a 10 m prototype with comparable performance, but testing the use of Fabry-Perot cavities instead of delay lines to enhance the optical length of the interferometer arms.[9] Ron Drever proposed

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[6] Written e-mail interview with Walter Winkler, Winkler's reply to the author on May 26, 2016; interview by the author with Jim Hough and Bernard Schutz, CERN (Geneva), August 28, 2017; interviews by the author with Alain Brillet, Nice, April 27–28, 2017, and Cascina, July 17–19, 2017.

[7] Written e-mail interview with Walter Winkler, Winkler's reply to the author on May 26, 2016; interview by Peter Collins with Peter Kafka from the Garching group, http://sites.cardiff.ac.uk/harrycollins/webquote/. Accessed 14 September 2020. Photon shot noise is due to the fluctuation of the number of photons detected by the photodiode at the output of the interferometer. Therefore, if the shot noise is the limiting factor, the sensitivity of the interferometric detector increases with the squareroot of the laser power: four times more laser power means twice as much sensitivity. The topic was described by Winkler in a milestone conference devoted to Experimental Gravitation and held in Pavia in September 1976 (Winkler 1977).

[8] Written comment by Walter Winkler, e-mail to the author on March 23, 2019. Winkler also pointed out: "At that time, we had also solved all the relevant problems such as the stabilisation of the laser beam in frequency and geometry, servo systems, scattered light contributions, vacuum requirements, data acqusition etc. Otherwise we would not have got to the shot-noise level as set by the laser-power!"

[9] In order to obtain maximum signal response from an interferometric antenna, the distance between the test masses (i.e., the suspended mirrors) should be of the order of ¼ of the wavelength of the GW. This means that for signals of kHz frequency, the armlength of the interferometer should be a multiple of 100 km. For a ground-based antenna, one can obtain effective lengths of the right order by folding the light paths in the interferometer arms, using either delay lines or resonant cavities such as the Fabry-Perot cavity (Hough 1986). In an optical delay line, the light bounces back and forward between the mirrors, which have a slight curvature, so that the beams do not fall on top of each other. After a well-determined number of bounces, the light returns to where it started and exits through a hole in one of the mirrors. The major disadvantage of the delay line is that mirrors have to be large enough to allow sufficient bounces to take place without interference among the beams. A Fabry-Perot cavity is composed of two special mirrors, which are positioned in parallel: the light entering the cavity is reflected back and forth between the mirrors, with the beams falling on top of each other. At every reflection, a small part of the light comes out from one of the mirrors (which is not fully reflecting: semi-transparent mirror) and the outcoming rays interfere with each other, producing interferometric rings.

Fabry-Perot cavities as an alternative to delay lines in 1980 and such systems were developed in Glasgow (10 m prototype) and at Caltech (40 m prototype) (Drever et al. 1981, 1983; Newton et al. 1986; Spero 1986). The experimental group in Glasgow had been strengthened during the 1980s by collaboration with a theoretical group interested in many aspects of GWs including sources, data analysis of GW signals and networks, led by Bernard Schutz at the University of Wales in Cardiff. It is important to notice that in 1986, Schutz discovered that signals from binaries carry information about their distance, allowing for the measurement of the Hubble Constant, whose value then was yet very uncertain (Schutz 1986). The task needed a minimum of three detectors and provided a strong argument motivating the funding of an array of interferometers: it was a concrete astronomical goal, especially in view of the likely occurrence rate of coalescing neutron-star binaries.[10]

The French group started research on GW detection in 1982, working mainly on laser technology and interferometric techniques, finding the solution for a powerful and stable source of coherent light in Nd-Yag laser. By 1987, both the Orsay and the Garching groups had successfully demonstrated light recycling, an idea first suggested by Ron Drever in Glasgow and Roland Schilling in Garching, which gives increased light power in the interferometer and hence an improved sensitivity (Drever 1983).[11]

The research activities of the three European optical groups were partly supported by a stimulus grant from the European Economic Community, which they applied for in 1985 under the impulse of Philippe Tourrenc, theoretical physicist of the Insitut Henri Poincaré in Paris and one of the main promoters of GW research in France. The joint application for this European grant was the first formal action toward a European collaboration for GW research.

Besides the specialists in optics, there was a fourth experimental group working independently from 1983 on a completely different and complementary topic led by Adalberto Giazotto in Pisa.[12] They were developing special seismic isolators in order to reduce dramatically disturbances acting on a test mass at low frequencies down to 10 Hz, the region where GWs produced by a significant number of pulsars were supposed to be detectable. In interferometric antennas, the test masses are the mirrors of the interferometer, which must be isolated as much as possible from external noise, and particularly seismic noise in the frequency range 10 Hz to a few kilohertz, in order to observe the tiny signal from the passage of a GW. This goal can be achieved by hanging the mirrors with special multi-pendular suspensions, in which Giazotto was a pioneer.

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[10] Written comment by Bernard Schutz, e-mail to the author on April 7, 2019.

[11] The Fabry-Perot cavities, the Nd-Yag laser, and light recycling are all features that have been adopted both by Virgo and by LIGO.

[12] The group was originally composed of Adalberto Giazotto, Diego Passuello, E. Campani, G. Finzi Contini, and A. Stefanini, shortly later joined by H. Kautzky, V. Montelatici, Angela Di Virgilio, and R. Del Fabbro. They were working in a small laboratory built up in San Piero a Grado, close to Pisa.

Following this brief overview of the European groups, we will focus on interactions between the teams in Orsay and in Pisa which gave life to Virgo.

5 A Close-Up on the Groups in Orsay and in Pisa

A CNRS Research Engineer at the Laboratoire de l'horloge atomique, Orsay, since 1970, Brillet became interested in interferometric detectors while spending 2 years as a post-doc at the Joint Institute for Laboratory Astrophysics (JILA) University of Colorado in Boulder (1977–1978) in the group led by John L. Hall. Here Brillet had the chance to meet Peter L. Bender, who was conceiving a project to build a space interferometer for GWs, the future Laser Interferometer Space Antenna (LISA) (Faller et al. 1985). Back in Orsay, Brillet participated in the 1979 Marcel Grossmann Meeting in Trieste, where he presented the results of a relativistic test on the isotropy of space using laser techniques accomplished at JILA (Brillet and Hall 1982).[13] Here he met Ron Drever for the first time and the French theoretician of general relativity and gravitational radiation Thibault Damour, and also Rüdiger and Schilling, who were presenting the first results of Garching's 3-m interferometric prototype and their first mode cleaner (Maischberger et al. 1982).[14]

In 1981, the main promoters of GW detectors based on laser interferometry met in a conference promoted by the NATO Advanced Study Institute and held in Bad Windsheim, Germany, which was devoted to quantum optics and experimental general relativity.[15] Among the participants were Eugene Wigner, John Wheeler, Gerd Leuchs, Kip Thorne, Jim Hough, Ron Drever, Karl Maischberger, Vladimir Braginsky, Carlton Caves, Alain Brillet, and the Frenchman Christian Bordé, co-

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[13] The first two Marcel Grossmann Meetings on Recent Developments in Theoretical and Experimental General Relativity, Gravitation, and Relativistic Field Theories were held in Trieste in 1975 and 1979. The meetings were founded by Remo Ruffini and Abdus Salam and up to the 1990s represented, together with the GRn conferences, the main periodical gatherings where the experts of GW detection had the opportunity to meet. It is interesting to note that at the 1979 conference in Trieste (MG2), a specific section devoted to GWs was organized for the first time and chaired by G. Pizzella and R. Drever. Indeed, between the first and the second MG meetings, the observation of the Hulse-Taylor binary pulsar over about 5 years had given a strong impulse to the field of GW research. Significantly, two of the four plenary invited lectures were about gravitational radiation: one by J. H. Taylor, "Gravitational radiation and the binary pulsar," and the other by E. Amaldi, "Recent progress in gravitational wave detection."

[14] Already at this stage, the group from Garching expressed in their paper a clear idea for the future: "The long range goal of the Munich project is a Michelson interferometer of very long path length (100 km), illuminated with high laser power (100 W), sensitive enough to detect Virgo cluster events" (Maischberger et al. 1982, 1).

[15] At the meeting in Bad Windsheim, all physicists from the Garching group reported on the status of their work. In particular, Winkler described the problem of scattered light, which they had recently encountered.

founder of the Laboratory of laser physics at the University of Paris North.[16] The Bad Winsheim conference was a milestone for the small community working on interferometric antennas. In the same year, Brillet visited Rainer Weiss at MIT, receiving help, encouragement, and "clear support for collaboration rather than competition."[17]

Apart from some brief experimental activity carried out by the group of Silvano Bonazzola at the Meudon Observatory in the early 1970s reproducing a Weber-type experiment in coincidence with the ones in Munich and in Frascati (Bonazzola et al. 1973), there had been no other experimental work in France in the field of GW detection. Instead, several theoretical studies had been accomplished, such as the ones by Thibault Damour, Nathalie Deruelle, and Silvano Bonazzola on the sources of GWs and by Jean Yves Vinet on the performance achievable by different types of resonant, electromagnetic, or elasto-optical detectors (Deruelle and Tourrenc 1984; Vinet 1979).

Eager to capitalize on his expertise in metrology and laser technology to develop this brand new field of fundamental physics, Brillet began discussing with the specialist in laser physics and interferometry Christian Bordé and the theorists Philippe Tourrenc, Jean Yves Vinet, and Thibault Damour the idea of investigating the interferometric approach to GW detection in France. Between 1981 and 1983, Bordé organized regular meetings at the Direction Générale de l'Armement (Directorate General of Armaments), where he was scientific advisor, in order to compare the theoretical sensitivity and technical difficulties of various types of interferometric detectors.[18]

Around 1982, Brillet began experiments in Orsay, setting up a small laboratory hosted by the Center for Nuclear Spectroscopy and Mass Spectrometry. He was joined by the young student Catherine Nary Man and later by other young researchers, such as the American David Shoemaker, who came from 2 years in Garching and a previous working period at MIT. Tourrenc, who was at that time director of the Laboratory of Cosmology and Relativistic Gravitation at the Institut Henri Poincaré, provided the experimental group with all his institutional support. The Groupe de Recherche sur les Ondes Gravitationnelles (GROG) was recognized by the CNRS as a "young team" of the Laboratoire de Cosmologie et Gravitation Relativiste, while the experimental activity, as we said, was hosted in Orsay.

During the years 1982–1985, the team did not receive regular funding but had many small contracts for research coming from the physics department of CNRS, the French military, and private enterprises. The activity was also partially European funded. With the encouragement of Philippe Tourrenc, the groups of Glasgow,

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[16] The Laboratoire de Physique des Lasers de l'Université Paris-Nord was born in 1972. In 1981–1982, Christian Bordé was its director.

[17] Interviews with Alain Brillet, Nice, April 27–28, 2017, and Cascina, July 17–19, 2017.

[18] As their American allies, the French military played a role in the research on general relativity. They were particularly interested in developing new powerful lasers and investigating the possibility of submarine communications through GWs (Deruelle and Lasota 2018).

Garching and Orsay, made a first joint application in 1985 for a European Twinning grant in the EEC stimulus program. The European Commission twinning award supported the development of high-energy lasers (YAG lasers) in Orsay, the study of high reflectivity mirrors in Glasgow, and the organization of European workshops, such as the ones held in 1985 at Schloss Ringberg, Germany, and in 1986 in Chantilly, France.[19]

The main activities in Orsay concerned optical metrology, reduction of shot noise, and enhancement of power laser stability. Particularly relevant was finding an appropriate solution for the laser source. In the frequency range of the most promising astrophysical candidates considered at the time—supernovae, emitting GWs around 1 kHz—the main noise sources to address were shot noise and laser noise. As Brillet later described, "The main challenge was to convince the community that it could be possible to split the dark fringe into more than ten billion, which required an incredibly stable and powerful laser" (Brillet 2009). The path to follow was power recycling, a technique proposed by Drever at the École des Houches in June 1982, aimed at reducing the power required from the laser by recycling the light coming back from the interferometer.[20] However, at that time, this technique had not yet been proven by an experiment or even by calculation (Brillet 2009).

While the teams in Glasgow and Garching were building the 10 m and 30 m prototypes, the group in Orsay "decided, instead of starting another prototype, to focus on lasers and interferometry, while Tourrenc and Bordé were studying the interaction of the gravitational wave with the interferometer and Vinet was developing the theory of recycling".[21]

Using argon lasers, the only high-power single-frequency laser available at the time, the French team showed that the sensitivity of a Michelson-Fabry-Perot interferometer up to 2 Watts is effectively limited by shot noise and demonstrated the efficiency of power recycling.[22] In addition, the choice and the study of infrared

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[19] Letter from Alain Brillet to Adalberto Giazotto, May 12, 1986, PAB. A second European grant was obtained shortly later and benefited also the Italian team. The grant supported the organization of a third workshop, held in Sorrento in 1988, and "allowed some collaborative work (and common publications)" (Bradaschia et al. 1989, 90).

[20] Held in a location close to Mont Blanc and devoted to gravitational waves, the École des Houches was organized in 1982 by Nathalie Deruelle and Tsvi Piran. The school brought together about sixty experimentalists, theoreticians, and astrophysicists for about three weeks. On this occasion, Ron Drever presented the idea of power recycling, a proposal made at the same time by Roland Schilling who was also participating in the gathering (Drever 1983; Deruelle and Lasota 2018). Drever's and Schilling's proposal was basically to recycle the light coming out from the interferometer to increase the effective power of the laser.

[21] Brillet A. Personal notes on the history of Virgo. February 20, 2013, PAB. Brillet is refererring to Vinet (1986).

[22] A single-frequency laser is a laser that operates on a single resonator mode, so that it emits quasimonochromatic radiation with a very small linewidth and low-phase noise. Because any-mode distribution noise is eliminated, single-frequency lasers also have the potential to have very low intensity noise.

light Nd-YAG lasers (wavelength 1064 nm) to replace the noisy and unreliable argon lasers was a major contribution of the physicists in Orsay to interferometric detectors (Shoemaker et al. 1989).[23]

In the meantime, astrophysicists had observed a growing number of pulsars detected with radio telescopes. Many of them were seen to slow down over time, although with a very low spin-down rate, increasing their rotation period by less than a few microseconds per year. Spin-down is related to energy loss due to various mechanisms, for example magnetic dipole radiation and eventually GW emission. In order to generate GWs, a spinning neutron star must have some non-symmetric distortion in its shape, i. e., a "mountain." In the 1980s, many fast pulsars had been observed, showing rotation slowdown to be possibly attributed to GW emission in the range of frequencies just above 10 Hz—pulsars with a rotation rate of more than 5 cycles per second.[24] These fast rotating pulsars were an important stimulus for Adalberto Giazotto's work on seismic noise insulation (Fig. 1).[25]

As he recounted many years later, "We realized that a relatively big number of pulsars could have GW frequency greater than 10Hz. [. . .] Given that one wanted to go to low frequency, the problem was how to cut seismic noise by 12 orders of magnitude " (Passuello and Fidecaro 2018, 416).

Adalberto Giazotto had been working in the field of particle physics since his graduation from the University of Rome in 1964. He participated in the electro-production experiments and in the study of the form factors of mesons,

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[23] In the 1989 LIGO proposal and in the 1989 British-German project, the privileged solution for the laser source was the use of green light, produced by doubling the frequency of a Nd-Yag laser and thus obtaining a 532 nm wavelength (Vogt et al. 1989; Hough et al. 1989). Such a wavelength was close to that of argon lasers and was preferred, because shorter wavelengths allowed smaller beam diameters leading to smaller optical components. Furthermore, visible light was easier to work with and reduced shot-noise, which, for a fixed laser power, decreases with the square root of the optical frequency. However, in a power-recycling scheme, the optical losses due to scattered light are much larger in the visible than in the infrared frequency range. Scattered light noise was (and *is*) a crucial problem to face in a GW interferometric detector. For this reason, in the second half of the 1980s, the Orsay group was developing and testing the solution of infrared light, differently from what the other optical groups in Europe and in the USA were proposing. As a PhD student, David Shoemaker contributed to this experimental activity in the Orsay group in the years 1986–1989. Already the 1987 French-Italian proposal discussed the possibility of using the 1064 nm Nd-Yag laser source. The 1989 Virgo proposal definitely envisaged the use of infrared light, while the British-German GEO 600 and the American LIGO turned from green light to the 1064 nm wavelength only during the 1990s.

[24] Gravitational wave signals are expected at a frequency related to the rotation rate of the astronomical system, typically at twice this value. The orbital period of a binary system, as well as the rotation period of a single pulsar, changes over time, thus also the frequency of the emitted gravitational signal is time-dependent, being at every instant equal to twice the instantaneous orbital frequency—for a pulsar, it is equal to twice the instantaneous rotation rate. While the orbital period of a binary system reduces over time, the rotation period of a pulsar increases.

[25] Giazotto used to quote Richard (Dick) Norman Manchester's work on pulsar observation at the Australian Radiotelescope in Narrabri as a fundamental scientific motivation for starting his experimental activity in the field of GWs. See, for example, Manchester and Taylor (1981). Interview by the author with Adalberto Giazotto, Cascina, July 11–12, 2016.

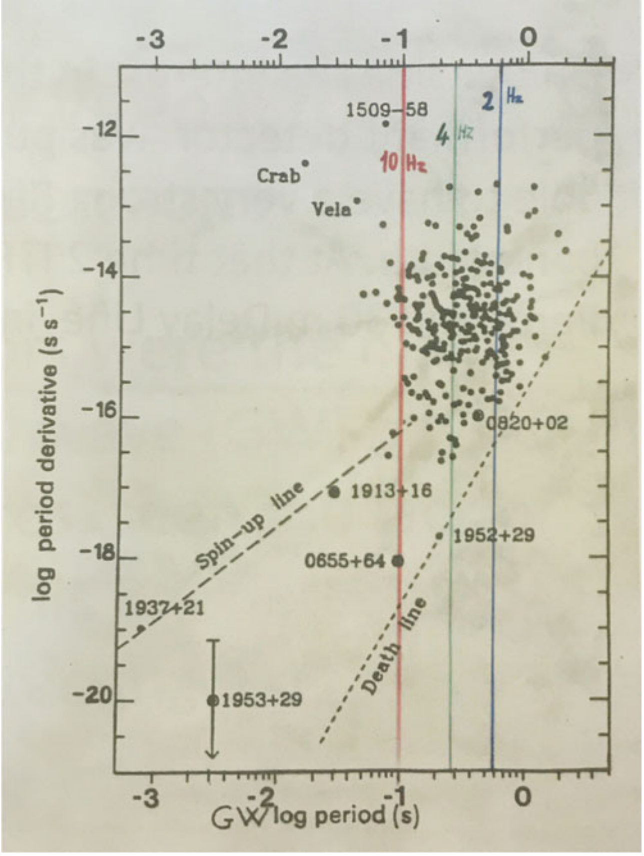

Fig. 1 The plot displays the distribution of pulsars showing rotation slowdown versus emission period. Giazotto used to show this plot in his slides during the 1980s to promote the effort toward seismic noise reduction down to 10 Hz (see Passuello and Fidecaro 2018, 415). Up to 1981, about 330 pulsars had been discovered (Manchester and Taylor 1981). Today we know that there are over 430 known pulsars spinning fast enough for their gravitational wave emission to be in the sensitive frequency band of Virgo and LIGO (∼ 20 to 2000 Hz) (See the Australia Telescope National Facility Pulsar Catalogue). The plot is included in Giazotto's personal slides, in Personal papers of Adalberto Giazotto, in the archives of the European Gravitational Observatory, Cascina (Pisa), Italy. It is also reprinted in (Passuello and Fidecaro 2018, 415)

accomplished by the group of Edoardo Amaldi and Gherardo Stoppini in Frascati, and then continued his activity in the laboratory of Daresbury in Great Britain and afterward at CERN in the experiments NA1 and NA7 led by the Pisa group (Bemporad and Bonolis 2012, 175). His interest in GWs awoke in the early 1980s, as he aimed at starting a new experimental activity in fundamental physics. Stimulated by the observation of the many new pulsars made through the Australian Radiotelescope in Narrabri (Manchester and Taylor 1981), in 1982 Giazotto submitted to the PISA INFN section a detailed internal report, which summarized the theory and noise evaluation of GW interferometric detection, analyzing in detail the low-frequency region (Giazotto 1982). In his note, Giazotto significantly argued that an important peculiarity of interferometric antennas "is that one can in principle detect not only gravitational pulses, but also periodical sources" and expressed the "hope that strong unpredicted sources of periodical GWs, which could be detected with a relative ease, exist in space." He also added that "the technological difficulties are astonishing" but "if the number of experimenters working in this field increases, the difficulties will have a chance of being overcome" (Giazotto 1982, 1–2). Low frequencies presented seemingly insuperable experimental problems due to seismic noise acting on the detectors components. In order to enlarge the bandwidth of detection to low frequencies, it was necessary not only to switch to interferometric detectors but also to study and build a new kind of seismic isolator for the mirrors of the interferometer.

The experimental activity started in San Piero a Grado, near Pisa, in 1983 with the name Interferometro per la Riduzione Attiva del Sisma (IRAS) (Campani et al. 1983).

The Garching group had already been using simple pendulum suspensions to hang the optical components in their first 3 m prototype. Winkler points out that they "used right from the beginning several mass-spring components in series in addition to the pendulum mode" and were aware that "further isolation stages were […] necessary to avoid the excitation of the different degrees of freedom well above the thermal excitation."[26]

Some preliminary studies about seismic isolation of interferometric detectors had already been accomplished in Glasgow in 1981–1982, setting up a very simple active system for isolating horizontal disturbances (Robertson et al. 1982). Fundamentally, it was a pendulum, connected to a capacitive sensor, which measured the relative horizontal displacement of the point of suspension of the test mass with respect to the mass itself. Thus, the time-dependent signal from this measurement was suitably amplified, filtered, and fed back to a transducer which controlled the position of the suspension point. The active feedback mechanism had the purpose of increasing the apparent length of the pendulum and thus its oscillation period, so that the disturbance on the hanging test mass could be partially reduced at lower frequencies.

After these preliminary studies, the British group did not continue to work specifically on low-frequency isolation but concentrated on reducing the thermal noise in the suspensions, a major problem which they became aware of by working

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[26] Written comment by Walter Winkler, e-mail to the author on March 23, 2019. Winkler recounted that at the very beginning they mounted the mirrors on holders fixed to the optical bench: "When looking at the interferometer signal, we found huge noise contributions coming from the resonances between mirrors and their holders. Whatever we tried out, nothing really helped. One day Karl Maischberger said to me: "why not suspend the bare mirrors in a wire sling, and thus avoid these ugly resonances? We did so, and immediately the noise level was much better. An ingenious idea, which nobody had thought of before. Then we had to invent means to adjust the mirrors properly and keep them there."

on their prototype in Glasgow. They developed the idea of *monolithic suspensions*: the fused silica mirrors were welded to fused silica fibers through silicate bonding, thus making up a monolithic component with high mechanical quality factor. The British solution was tested in the GEO 600 detector and then adopted by Advanced LIGO and, later, by Advanced Virgo.

Giazotto and his group took over the idea of an active system, substituting a Michelson interferometer instead of the capacitive sensor to measure the displacement of the test mass with respect to the suspension point. The IRAS apparatus reached a one-mile equivalent length for the pendulum and an attenuation of the order of 10^6. However, the experiment in Pisa also showed that it was not possible to achieve the desired attenuation working in only one degree of freedom (DOF), but that it was necessary to reduce seismic noise in all six DOFs of the rigid body (three translations and three rotations) because of the couplings between the different DOFs (Giazotto et al. 1986a, 1986b). The Pisa group thus turned to a new approach, based on passive attenuation in six DOFs: passive filters having very low vertical and relatively low rotational resonance frequencies. The idea of Giazotto was to use a multipendular suspension, in order to dissipate the vibrational energy along the chain and insulate the last pendular stage from movement of the Earth, where the mirrors of the interferometer would hang.[27] After trying different solutions and adding up new ideas step by step, this new approach proved successful and allowed the development of the *superattenuators* of Virgo.[28]

6 Widening the Observation Window: From Resonant Bars to Interferometric Antennas

The discovery of the first binary system composed of a pulsar orbiting a neutron star dates back to 1974 (Hulse and Taylor 1975; Damour 2015). In their 1975 paper,

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[27] Conceiving of a gas spring mechanical filter and building up a chain of seven filters to be put under vacuum, the group in Pisa was able to reach an attenuation of 10^8–10^9 for seismic noise down to 10 Hz (Del Fabbro et al. 1988). Many years later Giazotto recounted: “We still remember the noise in the accelerometer before the vacuum was turned on, it was relatively high with a lot of structures; when we turned on the pumping system, since no signal with some structure emerged anymore from the accelerometer, we thought that something was broken. Suddenly we realized that probably we had the most silenced object on the Earth, and the emotion was overwhelming” (Giazotto 2009, 5). This result was fundamental for supporting the 1989 Virgo proposal. However, the group soon realized that gas springs were too unstable under temperature variations to achieve the desired reduction of noise. The principle was right, but the final solution was to use tunable magnetic antisprings (Braccini et al. 1993). A further step was thus the introduction of the inverted pendulum: three long legs on flexible joints holding a disk to which the seven-filter chain would be hooked (Losurdo et al. 1999). The inverted pendulum gently displaces the suspension point to keep the alignment of the interferometer on the dark fringe. An attenuation of 10^{15} was thus obtained. For more details, see Passuello and Fidecaro (2018).

[28] The name s*uperattenuator* was given by Hans Kautsky (Giazotto 2009, 5).

Hulse and Taylor highlighted that the binary configuration provided a nearly ideal laboratory for testing general relativity "including an accurate clock in high-speed, eccentric orbit and a strong gravitational field" (L53). Observation of the system, called PSR1913 + 16, over several years showed that the orbit of the pulsar was slowly shrinking over time, following with great accuracy the prediction by general relativity for the energy loss due to GW emission (Taylor et al. 1979; Weisberg and Taylor 1981; Taylor and Weisberg 1982, 1989). These results constituted the first indirect proof of the existence of GWs.

The scientific evidence coming from the observation of the PSR1913 + 16 system provided strong support for experimental activity on GW detection. In particular, it furnished scientific motivation for the development of large bandwidth detectors—the interferometric antennas. Up to then, the privileged astrophysical candidates considered as possible sources of detectable GWs had been the supernova explosions, with expected gravitational signals peaking at around 1 kHz. Bar detectors have resonant frequencies around this value; their sensitivity was thus limited to a very narrow frequency band centered on 1 kHz: bar detectors, cryogenic ones as well, were optimized for the observation of supernova signals. Nevertheless, the discovery of the first binary system indicated the possibility of looking for gravitational waves spanning a wider band of frequencies. As predicted by GR models, a couple of massive objects inspiralling into each other under mutual gravitational action radiates an almost periodical gravitational signal, whose frequency and amplitude gradually grow over time. The transition from resonant detectors to interferometric detectors corresponds to the widening of the window of detectability to include not only gravitational pulses radiated by supernovas but also periodical or quasiperiodical signals, such as those emitted by binary systems or by pulsars. The ground-based interferometric antennas allowed to enlarge the frequency bandwidth of the detectors, extending it to frequencies lower than 1 kHz.

Since its very first proposal, Virgo's goal was to reach sensitivity down to 10 Hz.[29] It is interesting to note that Giazotto was not only thinking about transient signals, such as the ones emitted by supernovas or coalescing binary systems, but he was also pointing at continuous sources—fast rotating pulsars and binary pulsars—whose gravitational signals until recently had not been detected.

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[29] A binary system radiates the most energetic GWs during the last stage of its inspiral, the so-called coalescence: the bodies are orbiting very close to each other at extremely high speed, emitting a gravitational signal rapidly increasing in amplitude and frequency (*chirp* signal). In order to observe and follow the evolution of this kind of signal spanning over a wide range of frequencies, a detector sensitive to a large bandwidth is needed. The transition from supernovas to coalescing binary systems as the most promising sources of detectable GWs is a fundamental issue in the history of GW detectors. It is an important change of viewpoint, which contributed to the universal adoption of interferometric detectors.

7 The First Proposals for Long-Baseline GW Interferometers in Europe

The Garching group was the first European team to make an official proposal for a full-sized interferometric detector, submitting to the Max Planck Institute of Quantum Optics the document "Plans for a Large Gravitational Wave Antenna in Germany" in 1985, followed by an updated version in 1987 (Winkler et al. 1986; Leuchs et al. 1987). "After encouraging progress with the 30 m prototype, the GW group at the Max-Planck-Institut für Quantenoptik are increasing efforts towards a full-sized antenna. Arms 3 km in length are proposed, as a trade-off mainly between cost and the influence of thermal mirror motions" (Winkler et al. 1986, 621). Among the requirements, it is underlined that "the strongest sources of such radiation will have only short duration, and therefore the search must be made in a relatively wide frequency band. [. . .] To guard against spurious signals due to local noise sources, verification with at least one further interferometer is required. The interferometers used in coincidence should – if possible – be separated by distances in the range of 1000 km. Collaboration with experimental groups in the other countries is highly desirable" (Winkler et al. 1986, 622–624).

The Glasgow group, led by Jim Hough from 1983, when Drever took up a permanent post at Caltech to work on the 40 m prototype, suggested a first idea for the development of a 1 km interferometer in Scotland at a conference held in Aussois, France, in 1984, and organized by Philippe Tourrenc (Hough et al. 1984).[30] A detailed design study of the British interferometer was prepared with the engineering support of the Rutherford Appleton Laboratory, led by Ian Corbett,[31] and presented to the UK Science and Engineering Research Council (SERC) in May 1986 (Hough et al. 1986). It envisaged a detector based on a Fabry-Perot interferometer with 1 km arms or longer at either of two sites in Central Scotland. The proposal also included the possibility of installing four separate interferometers in the same vacuum system, two full length and two half length, which "should allow discrimination against the effects of random outgassing from the walls of the vacuum system and against residual seismic effects and laser fluctuations". Such a detector system was thought to possibly detect some types of gravitational signals alone: "While many experiments such as locating the direction of pulsed sources require operation with other detectors round the world, it is important that some experiments can be carried out on the instrument in a stand-alone way. These could include searches for periodic sources [. . .]" (Hough et al. 1986, 52–53).

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[30] In the same year, Caltech and MIT signed an agreement for the joint design and construction of two analogous facilities in the USA.

[31] Between the late 1980s and early 2000s, Ian Corbett held relevant positions first in the SERC and then in the Particle Physics and Astronomy Research Council (PPARC), which he contributed to create in 1994: he has been head of the SERC astronomy program, and then, with the birth of the PPARC, became Director of Science and Deputy Chief Executive, with responsibility for the UK research program in particle physics, astronomy, and space science.

Things were moving also on the French side. With the support of the Institut National d'Astrophysique et de Géophysique (INAG) in 1982–1984, Brillet's team made a cost evaluation—quite underestimated, like all proposals made in those early years—for a 1 km arm interferometer, to be built in Nançay, where some measurements of seismic noise were also accomplished. In 1986, the preliminary proposal aimed at planning a large GW antenna in France was placed on the list of the future *Très Grande Équipement* in low priority, preceded in the field of basic sciences by the Very Large Telescope.[32] However, no further steps were made toward a solely French interferometer.

The Glasgow and Garching proposals both focused on a frequency range spanning between a few hundred hertz to a few kHz, at least for the first generation of ground-based interferometric antennas. The 1985 Garching proposal argues: "A limitation of the usable frequency range is given at the low-frequency end by the steep increase of many optical and mechanical noise contributions, particularly of the seismic noise. To extend the frequency range to a lower limit of 100 Hz will already be a very difficult task" (Winkler et al. 1986, 2).

The 10 Hz goal was, instead, the special distinguishing feature outlined by the French and Italians in their first joint proposal, presented to the Italian INFN (Istituto Nazionale di Fisica Nucleare) in May 1987.

The idea of a French-Italian collaboration for GW detection was born from the encounter of Alain Brillet and Adalberto Giazotto, who had very different scientific backgrounds. They met for the first time in 1985, at the IV Marcel Grossman Meeting (MG4) in Rome and realized they had been working for several years on two complementary fundamental features of GW interferometric antennas.

> In 1985, at the meeting MG4, in Rome, Jean Yves Vinet presented his theory of recycling, which confirmed the intuitions of Ron Drever, and Adalberto Giazotto presented the first results of a high-performance seismic insulation system he was developing in Pisa, initially with the aim of developing bars capable of operating at low frequency to detect pulsars, but also capable of isolating the mirrors of an interferometer, in the frequency range from 10 Hz to 10 kHz. The idea was all the more interesting as the most important detection range for a terrestrial detector seemed to evolve towards the low frequencies: the favourite source was initially the explosion of a supernova, supposed to radiate mainly towards 1 kHz, but, on the one hand, the work of Silvano Bonazzola showed that the intensity of this radiation was uncertain, and probably low, and on the other hand, the coalescence of a binary system of Hulse-Taylor neutron stars was well modelled and may be more common, but would rather radiate around 100 Hz, or even less in the case of a binary black holes.[33]

Many years later, Giazotto described that happy encounter as "certainly planned by fate" (Giazotto 2009): "walking together around Minerva fountain at La Sapienza in Rome," the story of the project to be called Virgo had begun.

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[32] Brillet A. Personal notes on the history of Virgo, February 20, 2013, PAB.

[33] Brillet A. Personal notes on the history of Virgo, February 20, 2013, PAB.

8 The Steps Toward a French-Italian Collaboration and a European Working Party, 1986–1987

Today one can easily say that the encounter of Brillet and Giazotto in Rome was the start of Virgo, but at that time, it was not at all obvious that a collaboration would effectively begin. In the months following the Marcel Grossmann meeting, Brillet and Giazotto discussed how to possibly shape a collaboration and take advantage of a joint effort to promote the project of a large interferometric detector before INFN and CNRS. On May 9, 1986, Giazotto wrote to Brillet, including in the *cc:* field Angelo Scribano, the then Director of the INFN section for Pisa and Vincenzo Flaminio, the local INFN Coordinator of Scientific Commission II, devoted to astroparticle physics: "It should be then very nice if our two groups could collaborate together in view of the common scientific interest and of the complementary knowledge. As you also mentioned in your letter of 11/4/86 the hope is that we can in the future realize a large interferometric antenna having good sensitivity at low frequency. [...] The Pisa-INFN is ready to give due support to a common program presented and approved by the INFN National Scientific Committee"[34].

Brillet replied positively three days later, arguing about the importance of reaching a good sensitivity in the low-frequency range: "The simple devices which are being realized by the groups at Caltech, Glasgow, Munich and Orsay will provide adequate isolation only at frequencies larger than a few hundred hertz, while we know that the range of interesting frequencies extends much lower." He also commented that the MIT group was the only one who had already considered a low-frequency isolation scheme. However, Brillet pointed out that the field of GW detection called for collaborations not only for the necessity of coincidence experiments but also because of the impossibility for the research groups in the field, made up of five to six persons in average, to tackle all the problems. He added that "formal international collaborations are already appearing, to which you may wish to participate."[35]

Indeed, the three European optical groups had been already collaborating and taking advantage of a shared European grant, as we have said. They also were connected with the American teams of MIT and Caltech. A meeting had been held in Cardiff the previous February, with the purpose of shaping an international network for promoting projects in interferometric detection. On that occasion, some "notes and suggestions for an International collaboration on gravitational radiation research using laser interferometers" were produced and sent to Brillet by Rainer Weiss and Ron Drever. The cover letter from Drever and Weiss, dated March 18, 1986, reported the contents of the meeting: "[...] suggestions were made about the potential advantages of arranging some form of international collaboration between

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[34] Letter by A. Giazotto to A. Brillet, May 9, 1986, PAB.

[35] Letter by A. Brillet to A. Giazotto, May 12, 1986, PAB.

the experimental groups working in this field in Europe and the US, in part to improve the informal contacts and collaboration which are already taking place and benefitting all the groups. We are aware that possibilities for such collaboration are being discussed in Europe."[36]

The attached notes vaguely traced the aims of such a collaboration, like improving the rate of progress of gravitational radiation research using laser interferometers and facilitating provision of adequate support in each country by government or other funding agencies, promoting coincidence and cross-correlation searches for, and studies of, GWs. It is worth to note that these notes already foresaw collaboration on data acquisition, data formats and analysis, both hardware and software and collaboration in design and development of particular items of equipment or facilities, in particular the area of optical and electro-optical components.[37]

Giazotto had not been involved in the meeting in Cardiff in February 1986, nor in the previous meetings and initiatives among the European groups. At that early stage, he was not a natural interlocutor of the optical groups, as he was not working on laser physics and interferometry, which were perceived as the core of the new generation of antennas, nor was he dealing with theoretical aspects of GWs.[38] He had not participated in the milestone-quantum-optic gatherings in Bad Windsheim (1981) and at Schloss Ringberg (1985), nor was he present at the Journées Relativistes organized by Tourrenc in Aussois (1984) or at the meeting in Chantilly (1986). Not only was the group in Pisa working on a completely different topic with respect to the other experimental teams, but chronologically it was also the last to enter the field of GW detection.[39] Furthermore, Giazotto, who came from particle physics, was a kind of outsider in the field of GW research. Not being a natural interlocutor of the small world of GW interferometric detection, he was not

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[36] Letter by R.W.P. Drever and R. Weiss to A. Brillet, March 18, 1986, PAB.

[37] The group in Cardiff, in particular, focused on data analysis and on networks. Schutz and Massimo Tinto published two papers on antenna patterns and the capabilities of an interferometric network as a function of the number of detectors and their arrangement on the Earth (Schutz and Tinto 1987a, 1987b). Further work was done in Cardiff by Tinto, Sanjeev Dhurandha, and Andrzej Kròlak.

[38] Interview by the author with Angela Di Virgilio, Cascina, November 10, 2017.

[39] From reading the British and German proposals, one understands that the mechanics for suspending the mirrors was felt as a second-order problem, something to look at in the future. Interview by the author with Jim Hough and Bernard Schutz, CERN (Geneva), August 28, 2017. In the very small world of interferometric GW detection, optics was by far the most relevant field of research. At that early stage, the low-frequency strategy studied by the Pisa group appeared beyond the main and challenging problems of GW detection, which were basically related to laser technology and interferometry. Alain Brillet remembers having a conversation with Rainer Weiss, in which Weiss expressed the opinion that seismic isolation would not be the main problem at 10 Hz, and that one should face the very difficult task of low-frequency isolation later, after the first detection had been made. However, the challenge of reaching high sensitivity in the region of 10 Hz turned out to be a fundamental feature, in particular considering the frequencies of the first signals detected by LIGO and Virgo about 30 years later.

invited to participate in the “European gravitational detector working party,” which was being discussed among the other European teams in March–April 1987.

On April 24, 1987, Ian Corbett had prepared a note proposing some topics to be faced by the working party, whose aim was “to produce a report to be submitted to the three funding bodies which have so far been approached: BMFT, CNRS and SERC.” The report to be prepared should have presented “a unanimous view of a collaborative European approach to the construction and operation of interferometric GW detectors. [. . .] It should address and offer solutions to technical problems, provide realistic cost envelopes for a limited range of options, and present and evaluate models for the organization and management of the collaborative project.” From this perspective, the working party would have to “produce arguments which convince the funding agencies not only of the importance and timeliness of the science and technological feasibility of the project, but also of the ability and determination of the separate groups to work together.” This would provide “a negotiating framework for the funding agencies, and form the basis of any approach which might be made to other funding bodies, national or European.”[40]

Among the topics to be discussed by the working party, there were scientific and strategic questions as follows: “Whilst we are convinced that there should be at least two detectors in Europe, what are the cogent scientific arguments to justify this? How many antennae should we attempt to argue for?” “Is there a minimum/optimum separation between the antennae?” “What is the minimum arm length?” “What is the long-term role of the prototype detectors (Garching and Glasgow)?”

There were also organizational issues to be addressed, strongly connected to scientific arguments: “What are the arguments for building the antennas simultaneously? Do we have the resources to build and commission them simultaneously?” “How much can be done in common, how much should be done in common, and how much must be done in common?” “Should we have a common data acquisition, storage and analysis philosophy from the start?”[41]

Some very interesting questions concerned the possible collaborative scenarios. What kind of framework had to be adopted for the European collaboration? “Do we envisage a single co-ordination and management committee for the whole project comprising several antennae and many interferometers? Should this committee have financial authority delegated by the funding agencies, or should the various national groups contribute in equipment and manpower, under their control? How do we maintain the integrity of the national groups? How do we minimize bureaucracy while retaining accountability? What existing collaborative projects could provide a useful model? (or, conversely, indicate things to avoid!).”[42]

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[40] Note by Ian Corbett, *European gravitational detector working party*, pp. 1–2, April 24, 1987, PAB.

[41] Note by Ian Corbett, *European gravitational detector working party*, pp. 1–2, April 24, 1987, PAB.

[42] Note by Ian Corbett, *European gravitational detector working party*, pp. 1–2, April 24, 1987, PAB.

The optical groups planned to discuss these important matters during the first meeting of the working party to be held at the Rutherford Appleton Laboratory in Chilton, UK, in the month of June. It is interesting to notice that when Corbett wrote the text quoted above, on April 24, the Glasgow and Garching groups were not aware that Brillet and Giazotto were about to present a joint proposal to INFN in the following month of May.

9 The Difficult Balance in the Small World of European GW Interferometric Research

While the collaboration of the Orsay group with Glasgow and Garching was unavoidably characterized by some competitiveness—all of the three groups worked in the field of optics, even if on different topics—the Pisa group offered a truly complementary competence. The mutual interest of the Orsay and Pisa groups for cooperating was naturally based on this complementarity. For the Pisa group, working essentially on the mechanics of suspension to lower seismic noise, the necessity to collaborate with a team such as Brillet's was evident. It was a good chance to enter the mainstream of experimental GW research and participate in negotiations to create a European collaboration in this field. On the other hand, for the French, the alliance with the Italians would have strengthened their position in the European context and helped in reaching a critical mass to promote a joint GW project before the CNRS.

The situation of the small team of Brillet was indeed very peculiar, because their activity was not part of any research framework of the *Institut National de Physique Nucléaire et de Physique des Particules* (IN2P3), an autonomous institute of the CNRS. No nuclear or particle physicist was actually involved in the activity of the Orsay group, whereas in Italy Giazotto himself and his main supporters inside the INFN were high-energy physicists, used to big experiments and to their complex organization. One of the features distinguishing the French situation from the Italian one was indeed that in Italy, Virgo was supported from its very start by several scientists coming from Big Science.

Pierre Lehman, director of IN2P3 (1983–1992), was very interested in Brillet's experimental activity, but research on GW detection still did not have enough critical mass in France to appear as a priority in CNRS programs. There was no other experimental research going on in France concerning GWs. There was no consolidated experimental tradition in the field inside the IN2P3, nor a network of experimental physicists supporting the idea of GW detection, as was the case, instead, in the Italian INFN.[43] In Italy, the experimental research for GW detection

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[43] Interviews by the author with Alain Brillett, Nice, April 27–28, 2017, and Cascina, July 17–19, 2017. Unlike in Italy, French research activity on GW detection was supported by theoretical physicists and astrophysicists.

had started already in 1971, led by Edoardo Amaldi and Guido Pizzella. Research for their cryogenic resonant bar detectors had been supported from the early 70s by CNR and afterward by INFN. In the 1980s, Amaldi's and Pizzella's team in Rome had an international leading role in the field of cryogenic resonators for GW detection. The 2300-kg, 3-m long cryogenic bar Explorer, built at CERN, was the first antenna reaching the nominal sensitivity and stability over long periods (1990) and for several years the most sensitive GW detector in the world (Bonolis and La Rana 2016). During an interview with the author of this chapter, Brillet said that "the activity of Amaldi and Pizzella caused the National Institute for Nuclear Physics to develop a specific culture and interest in this experimental field, whereas this did not take place in France."[44]

Without a solid base in French scientific institutions, the group led by Brillet ran the risk of having a subsidiary position in the tripartite collaboration with Glasgow and Garching. Not only had the Glasgow and Garching groups started their research on GW detectors several years before Orsay (in the early 1970s) pioneered GW interferometry and possessed small-scale prototypes (30-m delay-line interferometer in Garching, 10-m Fabry-Perot interferometer in Glasgow), but they had also been collaborating more closely among themselves. They shared a similar scientific approach, testing the basic technologies on prototypes before moving to a full-scale interferometer.[45]

Brillet expressed his concern about establishing a European peer collaboration in a letter to Giazotto, written in June 1987, a few weeks after the presentation of the joint proposal to INFN and shortly before the meeting at the Rutherford Appleton Laboratory. He argued that it was very important for the French and Italian teams to appear as one single third group with its one project, in order to have a sufficient weight in the discussions for a future European collaboration.[46]

10 The First Virgo Proposal, 1987

In 1986, the small group of Giazotto had found new allies in Italy: the team led by Leopoldo Milano in Napoli was the first to join the new venture, proposing to develop the digital control system for the interferometer alignment. Also the CNR group in Frascati led by Franco Bordoni and a professor from the University of Salerno, Innocenzo Pinto, took part in the early effort to start a collaboration for a long-based GW interferometer. It is worth noting that all the Italian groups lacked

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[44] Interviews by the author with Alain Brillet, Nice, April 27–28, 2017, and Cascina, July 17–19, 2017.

[45] The special connections between Glasgow and Garching and the differences in timing and organization with respect to the Orsay group were indeed underlined several times in the 1986 British proposal.

[46] Letter from Alain Brillet to Adalberto Giazotto, June 1987, PAB.

at the time any experience in optics. The alliance with Brillet's team was thus fundamental, in order to present to INFN the first joint proposal for a long-based GW interferometric antenna in May 1987: "Proposta di Antenna interferometrica a grande base per la ricerca di Onde Gravitazionali."[47]

The use of Italian language for this first document testifies that the initiative was mainly coming from the Italian groups rather than from the French. There was pressure from the Italian side to submit a proposal to INFN for the next 5-year funding plan (*Piano quinquennale 1988–1993*) of the Italian funding agency. In particular, the circumstances must have appeared to Giazotto mature enough to make this further step, now that four different Italian groups constituted an initial critical mass and that influential members of the INFN directorate looked favorably at a project for a large GW interferometer to be built in Italy. In particular, supporting Giazotto were the President of INFN Nicola Cabibbo, the then Chairman of INFN Commission II Paolo Strolin, and Marco Napolitano, who was Director of the INFN Section of Naples and in 1989 became a member of the INFN executive committee and deputy president. On the other hand, the choice of the Italian language for this joint French-Italian proposal shows that while it was being written, negotiations among Italians and French and inside the scientific community supporting GW research in France were still taking place. Furthermore, Brillet was aware that CNRS was not ready to fund a shared project.[48] These circumstances may also explain why the French-Italian group let the British and German teams know so late about their shared project.

However, it is not surprising to read in the introduction to the proposal that a long-based interferometric antenna "would allow France and our Country, already excelling in the technology of cryogenic resonant bar detectors, to keep a high technological level also in this very complex research field."[49] The proposal was thus presented to INFN as a natural extension of an Italian research field already well established and appreciated internationally.

It must be underlined that the Pisa group was as small as Brillet's and that in 1987 Giazotto did not have any particular influential position in Italian physics nor in INFN, where he became the director of research only in 1989. Also, inside the

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[47] The "Proposal for a large based interferometric antenna for the search of gravitational waves" was signed by the following people: the French group from CNRS and the Université Pierre et Marie Curie in Orsay, Paris (A. Brillet, C. N. Mann, D. Shoemaker, P. Tourrenc, J-Y. Vinet); the team from Pisa INFN Section and Pisa University (R. Del Fabbro, A. Di Virgilio, A. Giazotto, H. Kautsky, V. Montelatici, D. Passuello, A. Stefanini); the group from Naples University Federico II (F. Barone, R. Bruzzese, A. Cutolo, M. Longo, L. Milano, S. Solimeno); and the teams from Frascati CNR (F. Bordoni, F. Fuligni, V. Iafolla) and from the University of Salerno (I. Pinto) (La Rana and Milano 2017).

[48] Interviews by the author with Alain Brillet, Nice, April 27–28, 2017, and Cascina, July 17–19, 2017.

[49] *Proposal for a large based interferometric antenna for the search of gravitational waves,* PAB.

small international community of interferometric detectors, as we have said, his position was relatively weak.[50]

Looking in this perspective, the fact that INFN already had a background in GW research was very important for the start of Virgo. However, one must not underestimate Giazotto's and Brillet's capacity for supporting the project with very persuasive scientific arguments, as pointed out by Paolo Strolin, Chairman of INFN Commission II, in the years we are interested in. As Strolin reported to the present author, "Brillet and Giazotto were not promoting, instead they were exposing their ideas, they were very convincing and the scientific challenge was extremely stimulating, impossible as it seemed."[51]

The 1987 proposal underlines the main feature distinguishing the proposed antenna from the German, British, and American projects: "we will attempt to be the first in exploring the low frequencies" as "the Italian group achieved an expertise in the low frequency strategy, which is not comparable to any other in the world."[52]

An interesting point is highlighted in the paragraph "Justification of the project in the wider international context": being sensitive to low frequencies may allow the detection of GWs emitted by periodic sources, which are non-transient and can thus be detected without ambiguity by only one antenna, because the data can be collected and integrated over long periods of time in order to enhance the signal-to-noise ratio.[53] The authors of the proposal assert that the low-frequency strategy would thus justify the building of the French-Italian antenna, even in the pessimistic hypothesis that no other interferometric detector was to be approved

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[50] Interview by the author with Angela Di Virgilio, Cascina, November 10, 2017.

[51] Interview by the author with Paolo Strolin, Naples, November 2017.

[52] *Proposal for a large based interferometric antenna for the search of gravitational waves,* PAB.

[53] At that time, the French-Italian team was not aware of the problem of the Doppler effect. Alain Brillet's comment, e-mail to the author on March 2019. Hough points out that "coherent averaging when looking for pulsars was not easy because of the Doppler shift effects etc., and this was being ignored by Adalberto and Alain, as Alain underlined." Hough had a background in pulsar research, because in 1970 he had been searching for pulsars looking for the phase fluctuations in low-frequency radio signals. Written comment by Jim Hough, e-mail to the author on April 9, 2019. As Sergio Frasca explained to the author, there are two main effects that complicate the description of the expected signal from continuous sources: the Doppler effect due to the motion of the Earth (of rotation and revolution) and the amplitude and phase modulation due to the apparent variation of the source direction. The former was well known, so "ignoring it was extremely naïve." Frasca points out that, however, "Adalberto had in mind not to reveal the individual sources, but the whole, seen as a background." Indeed, if there are many sources, "the signals would add up to form a kind of background noise, which would have been observed with a sidereal modulation due to radiation antenna pattern and the galactic asymmetry of sources." In this case, Frasca states, the Doppler effect is negligible. Due to the Doppler effect's particular signature over long periods, we can be sure that we are revealing a gravitational signal; even with only one antenna, the position of the source can be established with great precision. By studying amplitude and phase modulation, various information on the source can be deduced, for example, how it is oriented. Frasca comments that Giazotto's paper "Gravitational waves emitted by an ensemble of rotating neutron stars" (Giazotto et al. 1997) was published in 1997, but "he had been talking about this long time before." Written comment by Sergio Frasca, e-mail to the author on March 20, 2019.

in the near future. Otherwise, in the optimistic hypothesis, the approval of all five interferometric antennas (three in Europe and two in the USA) would open a field of GW astronomy and the most accurate tests of GR. This is a very important point in the story of the Virgo experiment; the low-frequency strategy appears to be a way of being independent from the destiny of the other interferometric projects and from the collaboration with other comparable experiments.

Another aspect, for which Brillet and Giazotto had battled side-by-side since the start, was to avoid making a small prototype and to work directly on a full-scale interferometer. Of course, they had to convince the funding agencies that this would be the good choice. In the 1987 proposal, they argued that "only a limited amount of data obtained from experimentation on a 30-m prototype can be extrapolated with confidence to be used for an interferometer one hundred times longer." From their point of view, a 3-km interferometer would present problems which one can solve only by facing them in a full-scale facility because the extremely high sensitivity aimed at is obtainable only with that arm length.[54] Of course, behind this motivation, there was also the desire not to lag behind the other groups, who were already far ahead with experimentation on their small-scale prototypes.

Written in Italian in a very short time, under the urgency to present it in time for a financial request to INFN, the 1987 proposal caught the other European groups by surprise, who were aware of the connections being established between Pisa and Orsay but not that a joint proposal was about to be presented to the Italian funding agency. This action was defended on the grounds of necessity and urgency, as described also in the acknowledgments at the end of the proposal: "We are very grateful to the groups who have written before us analogous proposals [...]; we are sorry that, due to our tight schedules, we could not collaborate more closely and directly with them and, in particular, we have not quoted their more recent results; we count on their help to remedy to this lack."[55]

The first occasion to render the French-Italian alliance official before the other European groups was the meeting held on June 17 at the Rutherford Appleton Laboratory: the first meeting of the European GW detectors working group.

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[54] The scaling problem, or the topic "prototype versus full scale interferometer," became in the following years one of the bones of contention between the French-Italian teams and the British-German ones, as recounted to the author by some of the main protagonists of the story (Brillet, Giazotto, Hough, Schutz, Shoemaker). Interviews by the author with Alain Brillet, Nice, April 27–28, 2017, and Cascina, July 17–19, 2017; with Adalberto Giazotto, July 11–13, 2016; with Jim Hough and Bernard Schutz, CERN (Geneva), August 28, 2017; with David Shoemaker, Geneva, August 27, 2017.

[55] *Proposal for a large based interferometric antenna for the search of gravitational waves,* PAB.

11 The First Meetings of the European GW Detectors Working Party

The people present at the meeting in Chilton were Brillet, Corbett, Hough, Leuchs, Schutz, Tourrenc, and Winkler. Giazotto, as already said, had not been invited officially, because the working group had been constituted some months before among the optical experts. Brillet and Tourrenc spoke about the project presented to INFN, from which a first decision was expected by June 19. In the minutes prepared the day after the meeting by Ian Corbett and "not for general circulation," one can read that "there was some criticism of the way the proposal to the INFN had been prepared without informing or consulting the other groups in the EC collaboration." It is also interesting to notice that the first draft of the minutes reported that the French-Italian proposal foresaw a long-baseline interferometer to be built in Nançay. Tourrenc made several corrections to the draft, also cancelling the part about Nançay.[56]

In a letter to Tourrenc of July 16, 1987, Corbett observed that "although the preparation and presentation of this proposal generated some ill-feeling," he hoped they can put all this behind them and "co-operate in helping each other's proposals in every way possible," pointing out that "open and honest dialogue is absolutely essential."[57]

It is important to understand the ideal action line envisaged up to then by the British, French, and German groups, aimed at promoting their national projects before the funding agencies. Following the successful application for a shared European grant, the three teams were forming a working party to establish a joint long-term strategy. This strategy was to be based on some form of coordination, which had to be discussed and agreed, between the different national projects. At that time, the projects which appeared more likely to be pushed forward were the British and the German. However, three well-spaced and appropriately oriented antennas would have allowed Europe to have its own independent gravitational observatory, regardless of whether the USA would have approved the two LIGOs or not. The strategy foreseen to promote the proposals was based on the premise of submitting them as part of a large-scale European project, which should have appeared as the winning card before the funding agencies.

In this perspective, the national initiatives appeared as tightly bound to each other. The unexpected presentation of the French-Italian project to INFN violated this delicate strategy, highlighting the image of a disunited and inhomogeneous community, an image which was clearly in contradiction with the aspiration to show up as a European network. Furthermore, the failure to cite the most recent works of the German and British groups in the 1987 French-Italian proposal put these

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[56] Ian Corbett, *Notes on the First Meeting of European Gravitational Wave Detector Working Group—17 June 1987 Rutherford Appleton Laboratory, Chilton (Oxford)*, p. 2, PAB.

[57] Letter by Ian Corbett to Philippe Tourrenc, July 16, 1987, PAB.

teams in an embarrassing position toward their funding agencies and weakened the scientific credibility of a shared enterprise.[58]

However, despite this problematic start, the French-Italian proposal had the positive feedback of enlarging the nascent European working party to include Italy, ideally increasing critical mass for the future project of a European GW observatory. On June 19, INFN agreed to insert the proposal for a long-based interferometric GW antenna in the 1989–1993 5-year planning, to be submitted to the Italian Ministry of Research in December for approval. Tourrenc wrote about this positive achievement to the other European groups (June 29, 1987) emphasizing that the result obtained would have helped everyone. He pointed out that the European groups still had not agreed if they were tied together in order to collect money for individual projects or if they were trying to build a collective array. He also addressed other questions on scientific topics to be studied and discussed: "what is a minimum array? Do we have to take collectively a low-frequency goal?"[59]

These arguments were planned to be discussed during the second meeting of the European working party, which was held in Paris on September 30. This time Giazotto was personally invited by Ian Corbett.[60] Tourrenc and Schutz prepared two separate drafts, outlining the case for building three laser interferometric detectors in Europe. They presented their results at the meeting held in Paris in September, reaching an agreement on the calculations necessary to support the case for having at least three European antennas.[61]

Indeed the political strategy to be adopted by the European working party had to be slightly changed with the entry into the scene of the Italians and the French-Italian proposal. Until that time, the action line was pragmatically oriented toward a *minimum array* of two antennas 1000 km apart, which would give reasonable time difference resolution. The most likely locations appeared to be Germany and Great Britain, the nations that up to then seemed to have more chances of hosting their own interferometers. With the perspective of the Italian-French antenna, the scenario had widened and the possibility of having a real European observatory—able to make gravitational astronomy autonomously in the future, independently from USA—

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[58] Interview by the author with Jim Hough and Bernard Schutz, CERN (Geneva), August 28, 2017.

[59] Letter by Philippe Tourrenc to A. Brillet, I. Corbett, J. Hough, G. Leuchs, B. Schutz, and W. Winkler, June 29, 1987, PAB.

[60] Present people: Brillet, Corbett, Giazotto, Leuchs, Robertson (on behalf of Hough), Schutz, Tourrenc, Winkler.

[61] It is worth noticing that in the month of July there had been in Cardiff the first ever Gravitational Wave Data Analysis Workshop (July 6–9, 1987), sponsored by NATO and joined by groups from all over the world. All the interferometer groups and most of the bar-detector groups attended, but not the Pisa team. The organizing committee was formed by Bernard Schutz from University College (Cardiff), Peter Michelson from Stanford University, Guido Pizzella from the University of Rome, and Roland Schilling from the Max Planck Institute in Garching. Corbett chaired a round-table discussion about data exchange, to facilitate joint analysis, whose report is in the proceedings of the workshop. It was aimed at a fully international networking effort.

was certainly a strong scientific motivation in support of individual national projects coordinated in the framework of a European collaborative endeavor.

The first task of the working party was to produce a report describing the scientific aims and motivations of a European collaboration to be presented to the national funding agencies (BMFT, CNRS, INFN, SERC). The contents to be included in the report were discussed in the meeting in Paris. It was agreed that the report "would outline the science possible with three detectors (plus two in the USA) and then show what would be lost if the number were reduced to two and then one" and it would also "indicate that Europe could do some good science without the US detectors." The document would summarize the history and motivation for building the detectors and "would emphasize the current position of European leadership and the fact that a collaborative European project for three antennas was the logical step for the present situation." During the meeting, Corbett reported about the plans of the European Commission, which "did not appear to be planning to offer support in the construction of large facilities, but would assist in their operation and development." So in the report to be prepared, "no reference to EC funding for the big detectors should be made."[62]

The notes of the meeting also report a hypothetical funding scenario, where the different countries involved might have made available over a 10 year period the following amounts (millions of British pounds): Italy (10–15), Germany (15), Great Britain (5–7), and France (5).

The gathering in Paris was preceded and followed by tensions and controversies, which are evident by reading the letters exchanged among the members of the working party. The problems concerned mainly how to shape the collaboration and the degree of independence versus coordination that the groups would have within it and also the emphasis to be given to the scientific achievements obtained up to that moment in the report in preparation.

Before the meeting, on September 4, Corbett had sent some notes to the working party about the possible forms of collaboration to undertake and to discuss in Paris. He came forward with the proposal that the members of the working party became a Coordination Committee, with a rotating chairman, which would look after "the EEC funded collaboration" and "the interactions, on behalf of the collaboration as a whole, with the funding bodies." He pointed out that "within the collaboration, groups would continue to be free to work in their own way" and "seek support for their own project," but "should also make it clear to their funding bodies that their project has to be seen as part of a collaborative European effort with the full backing of the other groups, and that it is not in competition with the other proposals."[63] Corbett's proposal was received rather coldly by the French group, as expressed by

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[62] Ian Corbett, *Notes on the Second Meeting of European Gravitational Wave Detector Working Group—30 September 1987 Paris*, in a letter from Corbett sent to Alain Brillet on October 2, 1987, PAB.

[63] Letter from Ian Corbett to the members of the European Gravitational Wave Working Party, September 4, 1987, PAB.

Brillet in a letter on September 16: “We wish to keep this technical collaboration distinct from an eventual ‘political’ collaboration, because it involves individual initiatives and would suffer from the necessary rigidity of an organized structure. Furthermore we don’t see the necessity of (and we don’t feel any pressure for) transforming our present working group into a formal Coordinating committee, with its monitoring and organizing power. We don’t want to compromise the funding of the Italo-French project and we don’t know yet what would be the consequences of the existence of this committee, so we prefer to keep the present informal group as it is, for now.”[64]

In these lines, one can read an underlying distrust in the interference of a centralized co-ordination body, a suspicion probably linked to some extent to the benchtop-experiment background of the Orsay group, not used to big collaborations. The French team appeared to be fearful of facing an unfamiliar terrain as a ‘political collaboration,’ rather than a purely scientific collaboration, moreover in an environment that, however small, was already characterized by a great competitiveness among the interferometry experts.

The reply of Corbett may sound today prophetic: “If I look at what exists, from the point of view of an outsider, I see co-operation but I do not see a collaboration. A collaboration needs objectives, agreements, structures and mechanisms, as exist within the EEC-sponsored collaborations. Unless these exist, no outside body will believe there is a true collaboration, and unless there is a true collaboration the case for three antennas in Europe is considerably weakened, because there is no cohesion in the programme. I believe the European effort has reached the point where it would be both timely and helpful to form a collaboration, broadly within the framework I suggested. We may not form the collaboration in Paris next week, but we could agree the general principles [sic]. [. . .] If people are saying that they believe their own interests would be better served by continued co-operation but not by formal collaboration, so be it. But I would regret it, and see it as a lost opportunity.”[65]

During the meeting in Paris, a compromise appeared to be achieved: the minutes of the gathering prepared by Corbett report that a form of collaboration “should now come into being, with the clarification that the Coordinating Committee would have no executive power and could not direct the work of any group, functioning by consensus.”[66] The collaboration was named EUROGRAV.[67]

By the end of November, the first draft of the EUROGRAV report was written by Ian Corbett and sent to the working party, but raised several criticisms and

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[64] Letter from Alain Brillet to the Members of the European Gravitational Wave Detector Working Group about Corbett’s proposals for the second meeting (to be held in Paris on September 30, 1987), September 16, 1987, PAB.

[65] Letter from Ian Corbett to Alain Brillet and Philippe Tourrenc, September 24, 1987, PAB.

[66] Ian Corbett, *Notes on the Second Meeting of European Gravitational Wave Detector Working Group—30 September 1987, Paris*, p.3, in a letter from Corbett sent to Alain Brillet on October 2, 1987, PAB.

[67] At this stage, also the possible participation of Spain was being investigated and discussed during the Paris meeting.

discussions, in particular from the French and German side. The British contribution appeared more highlighted than the others, so much that the Garching group pointed out, commenting on the draft in a message addressed to the working party: "We think it serves our common goal best if the various achievements from all groups are stressed on an equal basis, also with respect to priorities of chronology. [...] We consider it preferable not to start on a petty competition as to which group has contributed or first discovered or solved this or that particular part. If we tried that, it might take ages until we can agree on a draft."[68]

In a heated letter to Corbett on November 29, Tourrenc attacked the draft, saying that it had to be rewritten, as it represented a step backward on the path toward collaboration. He accused the Glasgow group of not trusting the scientific reliability of the French and Italians, as they did not intend to build a prototype. In Tourrenc's opinion, the Glasgow group feared that the British funding agencies may refuse to support their project if they allied with the "Latins," because they did not show sufficient experience and scientific credibility in the field.[69]

Making an a posteriori analysis, Hough pointed out recently that the Italians and the French underestimated the problem of thermal noise in their first proposal because they focused on the low-frequency strategy to detect gravitational signals from pulsars. In Glasgow, they had learned by working on their protoype that thermal noise was a major obstacle to aiming at the low frequencies. As Hough points out, ignoring the thermal noise "made the sensitivity at low frequencies look very much better than we believed could be realistically achievable." The choice of the Glasgow team was thus, as we have said, to concentrate "on reducing the thermal noise in the suspensions, not worrying so much about low frequency isolation".[70] Also Leuchs at the time stated in a letter to Tourrenc on August 28, 1987: "It is not clear to us that we want to concentrate on low frequency operation, which seems to be a little further in the future."[71]

It is interesting to notice the different scientific priorities of the European groups: what looked to the German and British teams a goal to be addressed in the future was for the French and Italians a problem to solve in the first place, and vice versa. These different approaches could have turned to be good reasons to complement each other and cooperate, but instead contributed to divide the groups and drive them away.

However, Tourrenc and Corbett met again a few days after the heated letter by Tourrenc in Paris (December 2–3) and many points were clarified. In the same month of December, the French-Italian project received the approval of the Italian Ministry of research to be included in the 5-year plan of INFN (1988–1993). An agreement on the final EUROGRAV report was finally achieved in March 1988.

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[68] Letter from Gerd Leuchs to Alain Brillet (also sent to Ian Corbett and Philippe Tourrenc), December 1, 1987, PAB.

[69] Letter from Philippe Tourrenc to Ian Corbett, November 29, 1987, PAB.

[70] Written comment by Jim Hough, e-mail to the author on April 9, 2019.

[71] Written comment by Jim Hough, e-mail to the author on April 9, 2019.

12 The Stranding of the EUROGRAV Collaboration

The EUROGRAV report was signed by all the members of the working group: Brillet, Corbett, Giazotto, Hough, Leuchs, Schutz, Tourrenc, Winkler. Sent to the national funding agencies BMFT, CNRS, INFN, and SERC, the document assumed as a starting point the existing national proposals and ratified a collaborative agreement among the European groups, which was "a logical extension of the existing European Community supported programme of research and development." The Coordination Committee would meet at least three times per year, in order to "coordinate and review the EC funded collaborative programme, the various working parties and the interactions with external bodies on behalf of the collaboration." Furthermore, the Committee would choose a spokesperson, who would act as "the interface between the collaboration as a whole and the outside world."[72]

It is interesting to notice that in the EUROGRAV report the name *Virgo* appears for the first time, to identify the French-Italian joint research activity. The concluding remarks of the document are particularly significant, when read today: "An array of three detectors in Europe would give the European groups the minimum independence necessary to enable Europe to maintain its leading position in the field. Technically and scientifically the European groups have the capability to construct and operate a network that could make the first detection of gravitational waves and that could reach the critical number of three antennas that would see the birth of gravitational wave astronomy. Three European detectors operating with an American array, built either simultaneously or subsequently, would become one of the most important astronomical instruments of the modern age."[73]

However, the 1988 EUROGRAV report very rapidly became obsolete, as the boundary conditions of the European groups, the teams themselves, and their relationships changed. In September 1989, the British and German teams submitted to SERC and BMFT a proposal for a joint 3-km interferometric GW detector (Hough et al. 1989). The idea of a joint project came from two referees of the British and German funding agencies: during a meeting in Bonn, Ian Corbett, who was at the time head of the SERC astronomy program, discussed the matter with Hermann Schunck, who had directed since 1987 the Fundamental Research Unit of BMFT.[74] At that stage, Corbett was clearly aware that SERC would

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[72] Brillet A., Corbett I. F., Giazotto A., Hough J., Leuchs G., Schutz B. F., Tourrenc P., Winkler W., Report of an Ad-Hoc Working Group on the Future of Interferometric Gravitational Wave Antennas in Europe—March 1988, PAB. Membership of the working group: Alain Brillet (CNRS, Université Pierre et Marie Curie, Paris), Ian F. Corbett (Rutherford Appleton Laboratory, Chilton), Adalberto Giazotto (INFN Section of Pisa and University of Pisa), Jim Hough (University of Glasgow), Gerd Leuchs (Max Planck Institute for Quantum Optics, Garching), Bernard Schutz (University College, Cardiff), Philippe Tourrenc (CNRS Université Pierre et Marie Curie, Paris), Walther Winkler (Max Planck Institute for Quantum Optics, Garching).

[73] Report of an Ad-Hoc Working Group on the Future of Interferometric Gravitational Wave Antennas in Europe, March 1988, PAB.

[74] Skype interview by the author with Ian Corbett, November 24, 2017.

not have embarked alone on an expensive and demanding undertaking such as the construction of an interferometric detector. Furthermore, the GW project was financially in competition with several British astrophysical endeavors, which served a much larger community and which were therefore favored by the British funding agency.[75]

In the same year of 1989, the complete text of the Virgo Project in English was submitted to INFN and CNRS. At this stage also, a privately owned plot of land in Cascina, near Pisa, was suggested as the most promising site for locating the future interferometer (Bradaschia et al. 1989).[76]

At the same time, a major event in European history took place: on November 9, 1989, the East German government announced the opening of the border between East and West Germany, heralding the demise of the Berlin Wall. Big history intercepted the small community of GW hunters. In the following years, the reunification of Germany would drastically influence the fate of the joint British-German project, resizing the scientific ambitions of the allied teams together with the dimension of the future antenna, which was shortened to a 600-m arm length interferometer, called GEO 600.

Karsten Danzmann, who in 1990 became the new leader of the German group, described the situation in a July 5, 1991, letter to Rochus Vogt, at that time the director of the LIGO project: "German unification is taking a huge toll on the BMFT budget. Thousands of scientists from the east have to be laid off or taken over. But both options are expensive. The research institutes of the old eastern academy of sciences have to be dissolved and new structures have to be built up, and so on . . . It is not only money, but also manpower. Everybody at BMFT is so busy restructuring the East that nobody has time to even think about Gravity waves."[77]

At the turn of the 1980s, the funding situation for scientific research was considerably dimmed on the British front due to an economic downturn, which also put an end to Margaret Thatcher's role as Prime Minister and Leader of the Conservative Party in November 1990. In January 1991, Sir Mark Richmond, Chairman of SERC, received a letter in support of the 3-km GEO signed by Brillet, Giazotto, and Tourrenc, and replied on February 1 in these terms: "[. . .] the overall SERC funding position for 1991–92 and subsequent years has been seriously undermined by the very poor Public Expenditure Survey outcome for science announced in November 1990. [. . .] In the short term SERC is having to

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[75] Interviews by the author with Alain Brillet, Nice, April 27–28, 2017, and Cascina, July 17–19, 2017; Skype interview by the author with Ian Corbett, November 24, 2017.

[76] With a resolution dated May 8, 1989, the municipal council of Cascina expressed by majority a "favorable opinion on the project for the installation of the large-scale interferometric antenna for the research of gravitational waves, as proposed by the INFN and the University of Pisa" (Bradaschia et al. 1989).

[77] Letter from Karsten Danzmann to Robbie Vogt on July 5, 1991, and forwarded to Alain Brillet on July 8, 1991, PAB. Herbert Walther, Director of the Max Planck Institute for Quantum Optics, proposed to Karsten Danzmann, who was working at the time at Stanford University, to take over the position left by Gerd Leuchs, who had decided to start working in the industry.

consider delaying participation in several projects by up to 5 years; GEO may well be one of these projects."[78]

Indeed, the British and the German teams made the joint proposal for a 600-m laser-interferometric GW antenna in 1994. The building of GEO 600 started in September 1995 in a field close to the University of Hannover, where the group led by Danzmann had moved in the meanwhile (Danzmann et al. 1994).[79] Meanwhile, on June 27, 1994, the agreement for the construction of Virgo was finally signed by Francois Kourilski, the President of CNRS, and Luciano Maiani, the President of INFN; the works in the site of Cascina started 3 years later in 1997.

In the period of time that goes from the first French-Italian proposal in May 1987 to the approval of Virgo and the proposal of GEO 600 in 1994, it is possible to identify two distinct negotiation phases aimed at giving birth to a European collaboration among the groups from France, Germany, Italy, and the UK. A first phase began with the entry on the European scene of the French-Italian project in the spring of 1987 and reckoned on a three-pole European alliance for interferometric GW detection, aimed at the construction of three antennas. This brief phase led to the preparation of the 1988 joint report, as described in the preceding pages. A second phase started with the joint British-German proposal in 1989, which reduced the poles from three to two: Italy and France on one side and Germany and UK on the other. Also the leadership of Danzmann in the Garching group since 1990 contributed to mark a new stage of the relationships among the different teams. The attempts to establish a European collaboration were thus resumed in the early 1990s in the spirit of having two European interferometers, an objective that had to be drastically rescaled following the reduction of funds for basic research in UK and in Germany. In these pages, only the first phase has been considered in-depth, postponing the analysis of the second one to a future work in preparation. However, it is now possible to draw some conclusions about the first period of negotiations analyzed so far.

13 Some Reflections and Conclusions

As argued in the 1988 EUROGRAV report, the scientific case for building an array of European GW interferometric antennas was indeed extremely strong: a

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[78] Letter from Mark Richmond to Alain Brillet, Adalberto Giazotto, and Philippe Tourrenc, February 1, 1991, PAB.

[79] The GEO 600 proposal was presented at the first Edoardo Amaldi Conference on GWs, held in Frascati over June 14–17, 1994, and the related paper titled "GEO 600: A 600 m Laser Interferometric Gravitational Wave Antenna" signed by Danzmann et al. is included in the proceedings published in 1995. Construction of GEO 600 ended in 2002. The British-German interferometer has been a fundamental facility to develop and test R&D for Advanced LIGO (the upgraded LIGO detectors which allowed the first detection), but its lower sensitivity compared to LIGO and Virgo, due to its shorter armlength, strongly limits its possibilities of detection.

GW observatory would have allowed the European scientists to be at the forefront of the new astronomy. It would also mean for European science to "capitalize on its past investment and present scientific and technological lead."[80] European cooperation was key to achieving these goals. So what went wrong and what were the main factors opposing the achievement of a collaboration, which had such strong advantages? Why could the European groups, which were at the time leading the experimental field of GW interferometry, not join forces to build a European GW observatory with at least two detectors of kilometric dimensions in Europe?

Different factors played an important role, as we have seen. Some factors were contingently linked to the relationships among the small groups of physicists working at GW interferometric detectors, while others were related to the particular historical moment experienced by the research field. External events also had a dramatic influence, as mentioned above, in particular the reunification of Germany and, as Ian Corbett points out, the change in overall research funding priorities in UK, which had reduced the support for basic research in the physical sciences, in favor of life sciences and applied science.[81]

The following lines summarize some internal difficulties within the research field identified so far through the present historical analysis.

- **Scientific disagreements.** The French-Italian and British-German teams did not share the same scientific approach to building the detectors; in particular, they did not agree on the need for working on a prototype before building a full-scale interferometer. The British and German groups had been working on their prototypes in Glasgow and in Garching for several years, learning and gaining experience in the field and trying different solutions. They were skeptical of the French-Italian choice of bypassing the construction of a prototype and immediately building the final interferometer, without testing various technologies and measurement principles in a smaller and more easily controllable facility. On the other side, Brillet and Giazotto claimed that to test properly such cutting-edge experimental setups, intended for unprecedented accuracy measurements, it was essential to work at such a scale. In particular, they argued that none of the components of a small prototype would meet the requirements needed in a real detector, and that after the development of a prototype device, the specifications would change for the next one, which would lead to circular efforts and wasting time. From Brillet's and Giazotto's point of view, since the final component specifications could be calculated, it was more efficient to develop each individual component—seismic isolation, laser, mirrors, and so on—to assemble them into a suitable full-size infrastructure. It was a methodological contraposition, strongly related to the scientific arguments used to support the requests for funding. BMFT and SERC would not take the risk of financing a full-size interferometer, without the evidence that it was possible to build and

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[80] Report of an Ad-Hoc Working Group on the Future of Interferometric Gravitational Wave Antennas in Europe, March 1988, PAB.

[81] Written comment by Ian Corbett, e-mail to the author, April 1, 2019.

operate smaller equipments. The approval of the 1989 German-British project was indeed subject to demonstrating the feasibility of coincident measurements, accomplished with the two prototypes for a continuative period of time of 100 hours.[82] For the British and German teams, it was thus fundamental to proceed step by step and demonstrate the principles of operation on the small scale before embarking on a large and expensive undertaking. The arguments produced by Brillet and Giazotto to convince INFN and CNRS were completely different and equally reasonable, as we have seen. Both approaches have proven successful over the long term. However, at the time, this divergence constituted a serious obstacle to establishing a shared strategy before the funding agencies and thus finding a form of advantageous collaboration.

- **The difficult balance between national ambitions and international collaboration**. The tension between being engulfed by a European collaboration and maintaining one's own independence was the mirror of the competitiveness and of the individualistic drives of the single groups, which, in dealing with their funding agencies, feared not finding the winning strategy. To some extent, the collaboration was not only considered instrumental in inducing the funding agencies to approve their projects, but it was also felt as a possible ballast that could slow down the approval of national projects. The different scientific approaches and backgrounds of the various teams were intertwined with the different national scientific environments. In the attempts to establish a shared agreement, the proposed EUROGRAV appears to some extent as a facade, a useful strategy before the funding agencies, which nevertheless hid a disunity of views deep enough to undermine a real collaboration. In the case of the American LIGO, the existence of a single funding body, the National Science Foundation, facilitated the approval of a double interferometer working as a gravitational observatory. In comparison, to arrange agreements among financing bodies from different European countries was a much more difficult task.
- **The difficult transition from bench-top experiments to Big Science**. The carrying out of a long-based interferometric antenna required a true change of scale in terms of number of people involved and of funding, with respect to the first and second generation of GW detectors: the room-temperature resonant bar detectors and the cryogenic resonant bars.

  A comparable change of scale is the one experienced by the field of particle physics in the 1950s and 1960s, with the advent of large research centers such as Brookhaven National Laboratories in the USA and CERN in Europe,

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[82] As pointed out by Bernard Schutz, the 100-hour run was mandated by BMBF and SERC as part of a demonstration that the groups were ready to scale up. In early 1990, the two prototypes took data in coincidence for 100 hours, and the Cardiff group was responsible for doing the data analysis. The loss of funding when SERC pulled out later that year made it hard to complete the analysis, and most of it remained unpublished, just in a series of Cardiff PhD theses. The learned lessons went into a proposal for a GW data format that was circulated among the other groups. Schutz states that the proposal "had a significant, although unacknowledged, influence on the 'frame' format that is now universally used." Written comment by Bernard Schutz, e-mail to the author on April 7, 2019.

with their highly sophisticated accelerating machines Cosmotron and the Proton Synchrotron. Unlike in a particle accelerator, however, the events detectable by a GW antenna are random, not repeatable nor reproducible in a laboratory. Furthermore, the detectability of transient events and the measurement of fundamental physical quantities, such as the distance of the astrophysical source and the polarization of the gravitational signal, are possible only by means of a network of antennas. In this sense, GW detectors are much more similar to astrophysical observatories such as gamma ray telescopes and radio telescopes, which have to work in a coordinated way to extract as much information as possible from incoming signals. Therefore, the development of interferometric detectors required hybrid features with respect to the great experiments of CERN and the wide networks of cosmic signal observatories. It was a matter of transition from bench-top experiments not only toward Big Science but also toward a very highly coordinated Big Science. The needed structure was, indeed, not a centralized experimental center as CERN, but two or more research centers located in different and well-spaced sites, working as a single machine. The kind of organizational expertise needed for this change of scale was not possessed by the laser interferometry experts in Garching, Glasgow, and Orsay, who were used to working in small and autonomous teams at bench-top experiments. The European grants shared by the teams did not affect this autonomy, leaving the groups free to conduct their own separate research activities in the framework of a very rarefied cooperation. It is not by chance that in the early negotiations for giving birth to EUROGRAV, the particle physicist Ian Corbett had taken such an active part as an outsider of the field, having an expertise in the large collaborations at CERN and familiarity with large international collaborations in astronomy and space science. It is also meaningful that in 1987, Adalberto Giazotto, another particle physicist, caused havoc among the interferometric groups, by successfully landing a GW interferometer project on a national funding roster. In the following years, the *forma mentis* and the organizational tools of Big Science were in a certain sense introduced in LIGO and Virgo mainly through the involvement of many scientists coming from particle physics, who became either supporters or active part of the projects, such as Barry Barish and Gary Sanders for LIGO, and Adalberto Giazotto and Nicola Cabibbo in Virgo. Nevertheless, the project of establishing a gravitational observatory in Europe, that is, a network of antennas rather than a single detecting machine, did not succeed, to some extent because the more purely astrophysical vision failed to emerge among the European groups.[83]

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[83] Virgo, in particular, experienced very different connections with the astrophysical community in Italy and in France. In France, the experimental research aimed at GW detection was supported since its beginning by several theoretical physicists and astrophysicists. This was not the case in Italy, perhaps partly because of the strong competition for funding and for academic positions. Unfortunately, the urgency of the experimental aspects and the prevalence of a particle-physics vision, aiming at the first detection rather than creating an astrophysical observatory, contributed to exclude at the time a wider involvement of the astrophysical community. This is surely a part of

- **The absence of a coordinated GW community.** During the 1980s, the organizational and institutional conditions for a wider collaboration might have been facilitated by closer contacts with all the scientists working in the field of interferometric detection of GWs. This was not the case because the GW experts were still quite fragmented, lacking specific dedicated periodic gatherings or coordinating bodies, which could define an identity for the GW community. GW scientists met at general gatherings such as the Marcel Grossmann Meetings or at GRn Conferences. The interferometry experts participated in the conferences devoted to quantum optics, but there was no meeting or specific institution which could bring together all the different skills required by the field. The Edoardo Amaldi conference on GWs was held for the first time in 1994, in Frascati, and in the following years became the cornerstone gathering of the field. Indeed, during the second Amaldi Conference, organized at CERN in 1997, the Gravitational Wave International Committee (GWIC) was born as a working group inside the International Union of Pure and Applied Physics, with Barry Barish as the first chairman (1997–2003).
- **The poor chances of success in the short term and the uncertain chances in the long-term.** The scientific endeavor of GW detection was extremely ambitious and especially in those early times the chances of success appeared to be very slight. A substantial part of the scientific community was skeptical about investing such a great amount of public money for a research activity with such uncertain results, especially given that there was great pressure to invest in competing projects, such as the LHC, ESO VLT, several space missions, and so on.[84] In comparison, in the 1950s and 1960s, the change of scale required by particle physics experiments appeared as supported by much greater chances of success. The extreme uncertainty did little to facilitate a European collaboration, as each group played the card that seemed safer.

The interactions among the European experimental groups, scientific referees, and funding agencies during the decisive years of the negotiations for the approval of the various large-scale interferometric projects had an important impact on the subsequent development of the GW community and of its scientific endeavors. These interactions must be considered in the broader context of the events that

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the history that needs further investigation. Interview by the author with Leopoldo Milano, Naples, July 19, 2016; interview by the author with Stefano Vitale, Rome, February 19, 2019.

[84] In the previous paragraphs, we already quoted Anthony Tyson's speech to the members of the House of Representatives of the USA, on March 13, 1991. Tyson attacked the LIGO project with these words: "The following example may help us grasp the magnitude of the task. Imagine this distance: travel around the world 100 billion times (a total of 2400 trillion miles, or one million times the distance to Neptune). Take two points separated by this total distance. Then a strong gravitational wave will briefly change that distance by less than the thickness of a human hair. We have perhaps less than a few tenths of a second to perform this measurement. And we don't know if this infinitesimal event will come next month, next year, or perhaps in thirty years" (Tyson 1991). Looking at the first signal ever detected by LIGO, one can truly consider how descriptive Tyson's words were and how incredibly difficult is the task effectively accomplished 24 years later.

took place in Europe in the same period—the reunification of Germany and the sociopolitical and economic changes connected to the end of Thatcher's leadership in UK—and that influenced those projects especially of the European groups with greater experience in the field.

In 1997, the scientists of GEO 600 established with the LIGO researchers the LIGO Scientific Collaboration. The British and German teams actually became a fundamental part of the LIGO project, as the successful development of advanced technology with the GEO 600 interferometer was one of the keys to the success of the American detectors. Ten years later, Virgo and LIGO signed the already mentioned agreement on the full sharing of data, joint data analysis, and common publications. The establishment of these two wide and interconnected alliances does not mean that there has not been direct cooperation among the French-Italian and British-German groups after the attempts of promoting a European array at the turn of 1980s and in the early 1990s. There has been indeed a sharing of knowledge and of people, and coordinated efforts on a truly international level. However, it is not possible to ignore the fact that despite the early plans for a European observatory, the original European teams signed up to an official collaboration with LIGO rather than among themselves.

An accurate historical reconstruction of the original attempts to establish a European GW observatory can help to develop greater awareness in planning future international collaborations, especially for the next generation of GWs detectors, the *3G*, or third-generation interferometers—the first generation being LIGO and Virgo, the second generation constituted by Advanced LIGO and Advanced Virgo.

While this chapter is being written, negotiations are under way for the proposal of the Einstein Telescope project, which will hopefully be the first European-born ground-based GW interferometer. Compared to the particular period of time investigated in the previous pages, the last three items or challenges described in the above list have now been left behind by the GW community, and a much more promising starting point has appeared for the establishment of the first European center aimed at exploring the newly opened frontiers of gravitational astronomy.

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