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Theodore Arabatzis
Jürgen Renn
Ana Simões *Editors*

Relocating the History of Science

Essays in Honor of Kostas Gavroglu

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Chapter 1
Introduction

Ana Simões, Theodore Arabatzis, and Jürgen Renn

The idea for this volume was aired among friends from various countries of the European Periphery, pausing in a hotel lobby somewhere in Corfu during the 8th Science and Technology in the European Periphery (STEP) meeting, organized by the Department of History and Philosophy of Science of the University of Athens in June 2012. If the initial motivation was the coming retirement of Kostas Gavroglu, this was, of course, just as good an opportunity as anything else to pay tribute to a superb scholar and a very good friend. That this book ended up being edited by the three of us is the result of practical reasons: we have in a way become the spokespersons for an extended group of Kostas's friends (and colleagues). No more no less.

Only in exceptional cases are the volumes of the *Boston Studies in the Philosophy and History of Science* dedicated to key contributors to the history and philosophy of past and recent science in the form of a Festschrift. The volumes in honor of Marx W. Wartofsky, Robert S. Cohen, John Stachel and Silvan S. Schweber are such exceptions. This volume has been assembled in order to celebrate the work and achievements of Kostas Gavroglu, one of the editors of this series and a pioneer of several new research directions in our field who has deeply influenced the lives and works of many of the authors who have contributed to this volume.

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Our paths crossed with Kostas's in ways that provide an open window to the many research interests and activities, contingencies apart, which became constitutive of Kostas's scholarly trajectory. Theodore Arabatzis was an undergraduate student when Kostas delivered a lecture on the history of special relativity at the University of Thessaloniki. The late Yorgos Goudaroulis, a close friend and collaborator of Kostas, introduced them to each other. Not long afterwards, in the fall of 1988, while a beginning graduate student at Princeton, Theodore once again met Kostas who at the time was on sabbatical leave at Harvard University. A memorable lunch in a Mexican restaurant in Cambridge marked the onset of a close friendship and collaboration. Jürgen Renn met Kostas through Bob Cohen and Sam Schweber in Boston, some time around the mid-1980s while he was an assistant editor at the Einstein Papers Project. They immediately became friends and have since remained in close contact, sharing not only interests in the history and philosophy of physics, but also in societal and political matters. Working jointly as editors-in-chief of the *Boston Studies for the Philosophy and History of Science* has proved a very smooth and rewarding enterprise. Ana Simões was a PhD student when she met Kostas for the first time at the 1991 History of Science Society Meeting in Madison, Wisconsin, USA, following the suggestion of Sam Schweber, and ever since they have been discussing and writing on the history of quantum chemistry. First over the phone and the fax machine, and subsequently through e-mail, real encounters have punctuated otherwise lively virtual interactions. Since 1999, their collaborative ventures expanded to the creation, development and consolidation of the international group Science and Technology in the European Periphery (STEP), and they have met at bi-annual meetings across wonderful locations of the European Periphery, other in-between undertakings notwithstanding.

A globetrotter in Europe and the USA, Kostas has visited metropolitan cities as well as cities of the European Periphery, including extended sojourns at Harvard University, Boston University, Cambridge University, the Chemical Heritage Foundation and the Dibner Institute for the History of Science and Technology. Through individual and multiple encounters and copious networking (including the coordination of many research projects funded by the European Union, the European Science Foundation, the Greek state and private foundations), Kostas has left his imprint in established and emerging research institutions and university departments, as well as in publishing and editorial ventures, of which the series Boston Studies for the Philosophy and History of Science and the History of Science Series by Crete University Publishers deserve special mention. He chose, therefore, to shape disciplinary developments in very specific ways.

Having gone through undergraduate and graduate studies in the UK (Lancaster University, Cambridge University and Imperial College, London), Kostas's training as a theoretical particle physicist led him to the USA, where he held a postdoctoral position at the State University of New York in Long Island. In the late 1970s, he opted to leave behind the prospects of a standard university career in physics in the USA and go to Greece, a country in which he had never lived before, having been born and raised in Istanbul. This change of course was accompanied by a gradual shift from physics to the history and philosophy of modern physics, including relativity, quantum mechanics, and particle physics, considered from an integrated philosophical and historical perspective. And it went hand in hand with a transition from the reductionist world of particle physics to an examination of the non-reductionist vistas offered by low temperature physics, focusing on methodological and conceptual issues. Jointly with Yorgos Goudaroulis, this research avenue led, among many other ventures, to the major book *Methodological Aspects in the Development of Low Temperature Physics, 1881–1957. Concepts out of Context(s)* (Kluwer Academic Publishers, 1989). Historical and philosophical aspects of low temperature physics and the history of (artificial) cold have been a persistent thread in Kostas's scholarly career, having recently materialized in an edited volume about the *History of Artificial Cold: Scientific, Technological and Social Aspects* (Springer, 2013). Along the way, an interest in the biographical genre led to the writing of *Fritz London (1900–1954). A Scientific Biography* (Cambridge University Press, 1995). Attracted by a biographee who was no revolutionary, but just a 'normal' physicist, according to Kuhnian terminology, London revealed himself as an exceptional physicist, who introduced the notion of macroscopic quantum phenomena, held mounting reservations about reductionism in the sciences, and found himself at the origins of quantum chemistry. In this way, London became the thread tying historical and philosophical considerations with issues ranging from low temperature physics to quantum chemistry. Quantum chemistry as well as physical chemistry/chemical thermodynamics have been examined from the standpoint of disciplinary history, especially for the implications of their "in-between" character, conceptual apparatus and styles of reasoning, the circulation of knowledge and practices, institutional and societal aspects, and interface with philosophy of science. Papers, chapters and edited issues of journals have recently been capped by Kostas's jointly authored book with Ana Simões: *Neither Physics nor Chemistry. A History of Quantum Chemistry, 1927–1970* (MIT Press, 2012).

Having chosen Greece, first Patras, then Athens, for the continuation of his professional career, it is no surprise that Kostas's immediate surroundings became much more than the outer context(s) in which scientific theorizing unfolded. They became the springboard propelling the move to history of science, both in informing thematic choices and inviting institutional changes: They led to Kostas's involvement in the development of the Department of History and Philosophy of Science at the University of Athens, and the concomitant training of a professional community of internationally active young historians of science and technology. A significant aspect of that enterprise was the attention paid to understanding the convoluted past of the sciences in the Greek-speaking world, and in particular what has been called the Greek Enlightenment and the history of Greek universities. Beyond various contributions in paper, chapter and book format, as well as various editorial ventures and textbook writing, purposefully written in Greek and addressed to local audiences, one should highlight Kostas's direction of the following projects: Hellinomninon, the digital project implemented by the Laboratory of the Electronic Processing of Historical Archives and the Department of History and

Philosophy of Science at the University of Athens, which made available on-line a considerable corpus of printed and manuscript eighteenth-century Greek sources; and the international Project Prometheus, funded by the European Commission and aimed at understanding "the spreading of the Scientific Revolution from the countries where it originated to the countries in the periphery of Europe during 17th, 18th and 19th centuries." Last but not least, one should note Kostas's involvement in two important institutions: his presidency of the Executive Board of the Historical Archive of the University of Athens and his membership in the Executive Board of the John S. Latsis Public Benefit Foundation. Locality does indeed play a significant role, both in understanding the past of the sciences as in appreciating the recent (re)configurations of history of science communities.

In a sense, opting to name this volume *Relocating the History of Science* is both reminiscent of the new locations, away from the so-called centers, in which young communities have thrived shaping international standards of scholarship, but also of fresh directions to be followed by historians of science, which have resulted from Kostas's vision and networking abilities. A community builder and a charismatic leader, Kostas has played a major role in the emergence and consolidation of the history of science, not only in Greece but also in other contexts. The foundation of the international group STEP became central to the exploration of ways of doing history of science in the "periphery" appreciated by the "center", but at the same time freeing the "periphery" from hegemonic discourses or parochial traditions.

Within STEP, different methodological approaches have been used to discuss a variety of themes, encompassing travels, textbooks, the popularization of science and technology, science and technology in the press, national historiographies of science, science and religion, universities, transnational histories, and science and gender. These approaches have been particularly attuned to detecting specificities in the institutional, social, cultural and political contexts and functions of science and technology in peripheral places, and have delineated the contours of a new historiography of science and technology in the European Periphery. One such discussion took place during a memorable stay at Kostas's house in the island of Aegina in 2005, and was behind the jointly authored paper "Science and Technology in the European Periphery. Some Historiographical Reflections," published three years later in *History of Science*. STEP has thus contributed to the ongoing debates on the various difficulties that have hampered a systematic study of the sciences and technology in the European Periphery, the dynamics of the hidden agenda of Europeanization, and the role of both as privileged standpoints for illuminating and deconstructing the notion of European science and technology, in the sense of enlightening the process of the emergence of science and technology as a global phenomenon, and as one of the main building-blocks of the construction of an imagined European intellectual identity.

Historiographical and philosophical musings, together with a particular attention to cultural and political desiderata, have therefore been present in all topics to which Kostas has devoted his scholarly attention, from science in the Greek-speaking world during the Enlightenment, to science in the European Periphery and the history of universities, to the history of the physical sciences, including the history of quantum chemistry and the history of low temperature physics and artificial cold. Taken together with institutional and community building and networking ventures, they became the material embodiment of what it means to be a *true* historian of science: someone who has opted to shape his life as an organic intellectual, to convene Antonio Gramsci's apt terminology.

The contributions to this volume mirror Kostas's thematic and methodological choices, but do not exhaust them. They have been organized in five parts: history of modern physical sciences (including low temperature research as an "in-between" discipline; Einstein, relativity, and the role of locality; and quantum chemistry in relation to quantum physics); STEP matters (including both specific case studies and historiographical reflections); history and philosophy of science (including two of Kostas's favourite topics: styles of reasoning and reductionism); historiographical musings (centered on various aspects of the circulation of knowledge and the role of scientific biographies); and, finally, beyond history of science (mathematics, technology and contemporary issues, including essays on the European University and the politics of science). Authors, often entertaining strong intellectual and personal affinities with Kostas, were asked to write about topics or approaches directly related to his own interests but also reflecting their own research experiences. Lindy Divarci and Lucy Fleet were behind the scene secretly organizing this wide-ranging network of authors and carefully editing their contributions. The assortment produced is indicative of *problématiques* at the forefront of research, and the papers are of interest irrespective of their origin as a tribute to an outstanding scholar and a wonderful human being. May the reader find as much pleasure in going through the various contributions to this volume as we had in making it come to light.

Part I
History of Modern Physical Sciences

Chapter 2
Louis Paul Cailletet, the Liquefaction of Oxygen and the Emergence of an 'In-Between Discipline': Low-Temperature Research

Faidra Papanelopoulou

Abstract In 1877 Louis Paul Cailletet in France and Raoul Pictet in Switzerland liquefied oxygen in the form of a mist. The liquefaction of the first of the so-called permanent gases heralded the birth of low-temperature research and is often described in the literature as having started a 'race' for attaining progressively lower temperatures. In fact, between 1877 and 1908, when helium, the last of the permanent gases, was liquefied, there were many priority disputes—something quite characteristic of the emergence of a new research field. This paper examines Cailletet's path to the liquefaction of oxygen, as well as a debate between him and the Polish physicist Zygmunt Wróblewski over the latter's contribution to the liquefaction of gases.

Keywords Louis Paul Cailletet • Liquefaction of oxygen • Nineteenth-century low-temperature research • Zygmunt Wróblewski • Priority controversies

2.1 Introduction

The liquefaction of oxygen in December 1877 by the French physicist Louis Paul Cailletet (1832–1913), and a few days later by the Swiss physicist Raoul Pictet (1846–1929), is often considered to have heralded the emergence of another sub-branch of physics, that of low-temperature physics. Indeed, it is the case that after the discovery of a range of hitherto unanticipated phenomena related to the low temperatures, such as superconductivity in 1911 and superfluidity in 1938,

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This paper is based on the more detailed paper: 'Louis Paul Cailletet: the liquefaction of oxygen and the emergence of low-temperature research', *Notes and Records of the Royal Society* (2013) 67: 355–373. I would like to thank the editor of the journal, Robert Fox, for letting me present a reduced version of it in this volume.

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physicists have almost completely dominatedlow-temperature research. However, this was not the case during the early period of low-temperature research, that is, the period from 1877 to 1908 when all the permanent gases were to be turned into liquids. The most important developments in gas liquefaction took place in a period characterized by the acquisition of increasingly complex experimental apparatus and skills, the application of thermodynamics on physical and chemical research and the reappraisal of chemical theory; chemists as much as physicists were actively involved in early-low-temperature research.

Cailletet's liquefaction of oxygen has often been described in the literature as having started a 'race' for attaining progressively lower temperatures (Mendelssohn 1977). In 1877, and for a few years thereafter, Cailletet was the dominant figure in the area of low-temperature research, with the construction of more sophisticated experimental apparatus, the liquefaction of other gases and attempts to study their properties. His achievement was based on the development of highly complex physical and chemical techniques for lowering the temperature and for purifying gases. A few years later, in 1883, Zygmunt Wróblewski (1845–1888) and Karol Olszewski (1846–1915) working at the Jagiellonian University in Cracow, succeeded in producing small quantities of liquid oxygen in a stable form. A reconstruction of the history of the liquefaction of air by the French physicist Jules Jamin (1818–1886), in 1884, who undermined the contribution of the two Poles in the liquefaction of gases, got Cailletet involved into a rather heated debate with Wróblewski. The debate raised issues of scientific authorship, the paternity of the methods used and the results attained.

In this paper I focus on Cailletet's path to the liquefaction of oxygen placing special emphasis on the industrial environment in which he was reared, his dexterity in the design and construction of experimental apparatus, his close links with the Parisian scientific community, especially with the chemist Henri Sainte-Claire Deville (1818–1881), and his ambition to become a pioneer in high-pressure chemistry. By focusing on the debate between Cailletet and Wróblewski I highlight some aspects of the emerging field of low-temperature research.

2.2 Louis Paul Cailletet: Early Years

Cailletet was born in an industrial family in Châtillon-sur-Seine in Burgundy, and was privileged to attend the Lycée Henri IV in Paris, and the École des Mines (1854–1855) as an unregistered student. At the end of his studies he returned to his hometown to work at his father's ironworks, being interested in applying the knowledge gained in Paris.

From 1856 Cailletet published studies dealing with phenomena observed in the ironworks and on procedures that enhanced the quality of iron products (Seytre 2005, 5). Most of his accounts were published in the *Comptes Rendus* of the Académie des Sciences, where they were presented by the chemist Henri Étienne Sainte-Claire Deville. Deville's support was evident through comments that he wrote to accompany Cailletet's notes. Reciprocally, Cailletet's work endorsed and extended previous work conducted by Deville himself.

Cailletet's research was chiefly concerned with chemical metallurgy. Most of what he did focused on phenomena that had been observed in ironworks but never properly explained. In 1866, Cailletet published an extended note on the phenomenon of dissociation based on the investigations of Deville. Until then, the analysis of compound gases was usually conducted in conditions of gradual cooling, which allowed the dissociated elements to recombine. However, Cailletet decided to examine gases after rapidly cooling them at the moment of their collection, and in this way he was able to confirm that at very high temperatures these gases did indeed dissociate into their elements (Cailletet 1866).

In all his notes to the Académie des Sciences, Cailletet underlined that his experiments benefited from the industrial context in which they were performed. His experiments were on materials that could not be prepared in laboratories, and the quantities used were on an industrial scale, so that the phenomena observed were intimately linked to industrial production. Some chemical reactions, though, only occurred in extreme conditions, such as the high temperatures produced in blast furnaces, or when large quantities of metals were involved. Although his first experiments were conducted during the normal operation of the ironworks, Cailletet later built a 350-l cementation box with which he could experiment without depending on actual production processes. For his research, he devised a number of experimental apparatus with which he could collect the gases emitted from the blast furnaces and replicate the various phenomena observed in a laboratory he set up in Cjâtillon-sur-Seine (Cailletet 1865).

2.3 A New Chemistry: Chemistry at 300 atm

In 1868 Deville became director of the chemistry laboratory at the École Normale Supérieure. A year later Cailletet started a new series of experiments on the influence of pressure on chemical phenomena, which were not directly related to the phenomena observed in the ironworks. This change of his research interests was due to Deville. Until 1869, Cailletet continued to stress the industrial context of his research, and to insist on the benefits gained from working in such an environment rather in a laboratory. However, from 1869, in his experiments on high-pressure chemistry as presented at the Académie des Sciences, Cailletet never mentioned again the ironworks as a site where experimentation took place.

Cailletet's first note on high-pressure chemistry at the Académie des Sciences was presented on March 22, 1869. In the *Comptes Rendus*, Cailletet described how pressure slowed down chemical action and concluded his article with his first theoretical remark: '. . .affinity is not a specific force, but the chemical combinations and decompositions depend directly on the mechanical phenomena in which they develop' (Cailletet 1869, 398). Cailletet's views were similar to those of Deville, who in his *Leçons sur l'affinité* in 1867 declared that 'The hypothesis of atoms, the abstraction of affinity, all sort of forces [. . .] are pure inventions of our spirit, [. . .] words to which we attribute reality' (Bardez 2007, 479). Both Deville and Cailletet confined themselves to the study of the observable facts and abstained from drawing any conclusions touching upon metaphysical issues.

The results of March 1869 were contested by the chemist Marcelin Berthelot (1827–1907), who referred to his own experiments in arguing that the action of pressure on chemical reactions could not be explained in purely mechanical terms but was due to, among other things, changes in the masses of the reacting bodies (Berthelot 1869). Cailletet's response recast the foundations of the discussion. He saw Berthelot's intervention as a priority claim concerning experimentation in high-pressure chemistry. In his defence, Cailletet stressed that his aim was mainly to employ a new apparatus that was easy to handle and safe to use, and with which he could perform chemical reactions in specific conditions of pressure and temperature. From the very beginning, Cailletet's work represented one of the main traits of nineteenth-century French experimental physics and chemistry, which was the building of apparatus in order to throw light on phenomena, leading to the formulation of empirical laws. Although there was ostensibly little concern for the corroboration of hypothetical models, a great deal of attention was given to questions about how apparatus and instruments worked.

Cailletet's subsequent publications dealt with the compressibility of gases at high pressures, and focused mostly on the design of experimental apparatus. During this period, Cailletet had already in mind the possibility of liquefying the permanent gases, despite the fact that the eminent physicist and chemist Victor Regnault (1810–1878) had tried to discourage him from undertaking such a task (Seytre 2005, 7).

2.4 High Pressure Chemistry and the Liquefaction of Gases

Cailletet's ability in high-pressure experimentation was further exemplified in his research on liquid carbon dioxide, which was first liquefied by Faraday in 1823. With his apparatus, Cailletet succeeded in liquefying the gas and examining its properties (Cailletet 1872). A few years later, in 1877, Cailletet successfully attempted the liquefaction of acetylene and ethane with the same apparatus. Although the task of the liquefaction of the latter was relatively easy, Cailletet insisted on the simplicity and safety of his apparatus, as well as its possible uses in the classroom or the laboratory for experimental demonstrations (Cailletet 1877a).

Cailletet's experimental arrangement was based on the compression apparatus of Colladon and Andrews. Liquefaction was achieved by the compression of the gas and its sudden cooling through its own expansion. It is said that Cailletet adopted the technique of lowering the temperature through expansion after an *accidental* leak or an unintentional release of the pressurized gas during his experimentation with acetylene.[1]

[1] Although the Joule-Thomson effect was known since the 1850s, neither Cailletet nor Pictet made use of it. It was only in 1895 that the British William Hampson and the German Carl von Linde introduced it into their liquefying apparatus (Rowlinson 2010, 50).

In his brief note at the Académie, Cailletet scarcely mentioned the expansion of the gas as a decisive step towards its liquefaction. He emphasized instead the description of his liquefaction apparatus with almost no reference to the hydraulic pump used (Cailletet 1877a). In later publications, the depiction of the size of the hydraulic pump as compared to the liquefaction apparatus was impressive, and it can be considered as an indication of the fact that Cailletet attached far greater importance to compression than the lowering of the temperature by expansion (O'Conor Sloane 1920, 179).

The next gas to be liquefied was nitrogen dioxide, followed soon afterwards by methane (Cailletet 1877b). A few days before the cautious announcement of the liquefaction of methane, Cailletet received a letter by Berthelot, who gave him instructions on how to purify the gas so as to make sure that the mist observed was indeed methane and that no other gas was contained in it. In addition, Berthelot expressed his conviction that Cailletet would be able to liquefy oxygen and carbon monoxide by lowering the temperature of the gas tube and without needing to exceed 200 atm.[2] Berthelot himself had attempted to liquefy some of the permanent gases by applying pressures of over 800 atm, but without success, not being aware of the importance of the critical temperature above which no gas could be liquefied at any pressure, no matter how great.

2.5 On the Liquefaction of Oxygen

The liquefaction of oxygen was achieved almost simultaneously by Cailletet and Pictet in 1877, and the work of both scientists was presented at the Académie on December 24. Employing his usual method of gas liquefaction, Cailletet placed oxygen and carbon monoxide into his liquefaction apparatus, cooled them at -29 °C and compressed them at 300 atm. He then let the gases expand rapidly, and calculated that the temperature drop would be 200°. At the end of the expansion, he observed a thick mist, which he identified, after several trials, as the condensation of both gases. He was, of course, aware that he was still not able to collect the gases in liquid form but expressed his intension to use the necessary refrigerants in order to achieve this success in the near future (Cailletet 1877c).

Pictet's telegram, received by the Académie on December 22, stated that oxygen was liquefied at 320 atm and -140 °C produced by a mixture of sulfuric and carbonic acids. Immediately after, Pictet sent some explanations as to how he had achieved the liquefaction of oxygen, introducing a totally different procedure than that of Cailletet's. His apparatus consisted of a series of cold circuits, known as the cascade method later used by Zygmunt Wróblewski and Karol Olszewski, James Dewar (1842–1923) and Heike Kamerlingh Onnes (1853–1926) (Pictet 1877).

[2] Letter from Marcellin Berthelot to Louis Cailletet, 21 November 1877 (Archives de l'Académie des Sciences).

Moreover, the concerns that led Pictet to the problems of liquefaction were different from those of Cailletet. In contrast with Cailletet's purely experimental approach, Pictet's interest in the liquefaction of gases stemmed from a wider theoretical concern about the constitution of bodies. In his extended paper on his liquefaction work, in the *Annales de Chimie et de Physique*, Pictet presented his microscopic approach to the laws of nature within the context of the mechanical theory of heat and Clausius' kinetic theory of gases (Pictet 1878). For Pictet, all bodies were constituted of impermeable 'molecules', set in motion and subject to the force of cohesion. The liquefaction of a gas was achieved through compression, during which the molecules of the body precipitated one upon another and formed the 'liquid molecules'. In the case of the permanent gases, it was the motion of the molecules that counteracted the force of cohesion and did not allow liquefaction. Subsequently, the liquefaction of the permanent gases required both the exertion of pressure and the obtaining of great cold. Pictet referred to the term 'critical point' at least once in his works, an indication that he was aware of Andrews's and Van der Waals's work (Pictet 1878, 225).[3] Cailletet, too, was aware of the concept of the critical point, although he never made any claims about its underlying ontology, whether in Pictet's or in Van der Waals's work. This has also been the case for many French experimentalists, whose work, despite playing an important role in the consolidation of the significance of the critical point after Andrews's and Van der Waals's work, remained purely experimental and did not involve theoretical considerations.

The announcement of the liquefaction of oxygen by Cailletet and Pictet was received with enthusiasm. Although Pictet had reported the liquefaction of oxygen first, the Académie granted priority in the achievement to Cailletet. On December 2, a few days before Pictet conducted his experiment, Cailletet had sent a letter to Deville, with which he announced the liquefaction of carbon monoxide and oxygen. Deville had the foresight to deposit the letter in a sealed envelope with the Académie des Sciences on the following day. At the time there was no dispute over priority at least between the two parties. In a letter to Cailletet, Pictet denied any priority claim.[4] There was certainly no doubt that Pictet had arrived at almost the same result independently, and both of them received the Davy Medal, which was awarded to them in a ceremony at the Royal Society of London on November 30, 1878.[5]

The extensive reports of the liquefaction of oxygen that appeared in the French periodical press were cast in distinctly patriotic terms. In an article in the popular science journal *La Nature* on January 5, 1878 the geologist and scientific writer Stanislas Meunier had summarized the achievement. After describing Pictet's fourth demonstration of the liquefaction of oxygen, Meunier went on stating that his compatriot Cailletet was attempting the liquefaction of the other permanent gases, in particular nitrogen, air and hydrogen. According to Meunier, Cailletet had

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[3] Andrews's experimental results were interpreted in terms of molecular physics by Johannes Van der Waals (Kipnis et al. 1996).

[4] Letter from Raoul Pictet to Louis Cailletet, 19 January 1878 (Archives de l'Académie des Sciences [AAS]).

[5] Letter from the Secretary of the Royal Society to Louis Cailletet, 10 November 1878 (AAS).

already achieved the liquefaction of nitrogen (as a mist) under 200 atm and 13 °C, the liquefaction of air and that of hydrogen under 280 atm and −29 °C (Meunier 1878).

Cailletet had indeed claimed that he had liquefied nitrogen and air and that there was convincing evidence that he had successfully liquefied hydrogen as well. Apparently, hydrogen compressed at 280 atm and allowed suddenly to expand created an instantaneous very fine mist that disappeared immediately. Cailletet admitted that during his first attempts to liquefy hydrogen he had not observed anything in particular, but he believed that in the experimental sciences the habit of carefully observing phenomena finally led to the recognition of signs that had previously gone unnoticed (Cailletet 1877d, 1270). Cailletet's experiments were conducted repeatedly in front of prominent members of the scientific society, such as Berthelot, Deville and Mascart, all of whom had endorsed the (hasty) announcement of the liquefaction of hydrogen. In these experiments, testimony and repeatability were deemed necessary to validate the results.

2.6 Producing Liquid Oxygen at Stable Form

Having provided the first tangible indications that oxygen could be liquefied, Cailletet continued his research in broadly the same direction. He improved his equipment, devised new experimental techniques, focused on the construction of better manometers, and studied, alone or in collaboration, the changes of state near the critical temperature and the properties of liquid gases and those of mixed gas hydrates (Caillletet 1878a, b, 1879, 1880; Cailletet and Hautefeuille 1881a, b). In 1882 he started using liquid ethylene as a cooling agent. The evaporation of liquid ethylene had the merit of producing a high degree of cold, and Cailletet used it (despite its high cost) as an important tool in his work on the liquefaction of oxygen. However, his first experiments with liquid ethylene did not lead to conclusive results. Having cooled compressed oxygen at the boiling temperature of liquid ethylene (−105 °C) and then allowed it to expand, Cailletet observed a violent boiling and projection of a liquid in part of the cooled tube. But he could not determine whether this liquid pre-existed or was formed at the moment when the oxygen expanded (Cailletet 1882, 1226).

Small quantities of liquid oxygen were finally obtained by Zygmunt Wróblewski and Karol Olszewski in April 1883. The two Polish scientists sent a note to the Académie, which announced their success in liquefying oxygen in a stable form. Modifying Cailletet's apparatus, Wróblewski and Olszewski had evaporated ethylene in a vacuum and in that way obtained a temperature of −137 °C, below oxygen's critical point (Wróblewski and Olszewski 1883a). On April 18, Wróblewski sent a cordial letter to Cailletet in order to thank him for the congratulatory letter he had received, and generously insisted that his own success should properly have been Cailletet's.[6]

[6] Letter from Wróblewski to Cailletet, 18 April 1883 (AAS).

2.7 Liquid Oxygen. In the Mi(d)st of a Debate

Even a close reading of the published notes and memoirs in the *Comptes Rendus* of the Académie and the various scientific journals of the time provide us with an incomplete account of the events surrounding the liquefaction of the permanent gases. Although the announcement of the liquefaction of oxygen in stable form by Wróblewski and Olszewski was received with moderate enthusiasm by the French scientific community, an extended article by Jules Jamin on the history of the liquefaction of air in the literary and cultural magazine *Revue des Deux Mondes* provoked a fierce reply by Wróblewski, which revealed many details of a debate that the ‘official sources’ had attempted to silence (Jamin 1884; Wróblewski 1885).

Although Jamin acknowledged the contribution of both Wróblewski and Olszewski in the liquefaction of oxygen in a stable form, his emphasis on the importance of Cailletet’s initial achievement and several pejorative insinuations concerning the originality of the work of the two Polish scientists aroused Wróblewski’s indignation.

According to Jamin’s account, in 1877 both Cailletet and Pictet had clear indications that oxygen could be liquefied, but neither of them had managed to collect liquid oxygen in a stable form. In the article, Jamin argued that collecting liquid oxygen in a stable form called for refrigerants, such as ethylene, that were more powerful than carbon dioxide or nitrous oxide gas. As Jamin described it, Cailletet had started working on ethylene and had announced his project publicly,[7] when a year later the Académie received two telegrams from Cracow announcing the complete liquefaction of oxygen and nitrogen. Jamin referred to Wróblewski as one of Cailletet’s assistants in the laboratory at the École Normale, who, using Cailletet’s apparatus and with the collaboration of Olszewski, had managed to boil ethylene in a vacuum and reached $-150\,^{\circ}$C, a temperature that was sufficient for the complete liquefaction of oxygen (Jamin 1884, 99). However, when asking the question who deserved to be recognized as having liquefied these gases, Jamin opted, to Wróblewski’s dismay, for Cailletet. In order to support his view, Jamin reproduced a letter by J.B. Dumas, the now deceased former secretary of the Académie des Sciences, in which Dumas urged an unnamed colleague to support Cailletet’s candidature for the prix Lacaze, particularly in view of Wróblewski’s and Olszewski’s achievements (Jamin 1884, 102).[8]

[7] By ‘public announcement’ Jamin referred to a note published by Cailletet on the use of ethylene for the production of low temperatures (Cailletet 1882).

[8] The prix Lacaze for chemistry was indeed awarded in 1883 to Cailletet for he was the first to show that the so-called permanent gases could be liquefied, and because he provided a simple apparatus with which these experiments could be carried out without danger (Anon 1884).

2.8 From Wróblewski's Point of View

In his 30-page reply to Jamin, Wróblewski linked his current work on gas liquefaction to his previous research agenda on the absorption of gases by liquids and solids, which included the study of phenomena at high pressures and involved the liquefaction of the absorbed gases (Wróblewski 1885, 6). His earlier experiments had required the construction of suitable apparatus, which was designed by Wróblewski and commissioned to be built, at the beginning of April 1881, from the Parisian instrument maker Eugène Ducretet. The method and equipment were similar to those used by Andrews and Cailletet, but they had superior features; for example, the apparatus contained up to six times more gas (O'Conor Sloane 1920, 206).

Wróblewski was admitted in the chemistry laboratory of the École Normale in August 1881 in order to test his new apparatus and experiment with it. The laboratory after Deville's death was headed by the chemist Henri Debray (1827–1888). As there was not a suitable pump in the laboratory, Wróblewski bought a Cailletet pump from Ducretet, the 300th pump sold by that time. It was therefore obvious for Wróblewski that he could not be accused of improper behavior when the instrument he used was already widely available. The same argument had already been made in his paper on the liquefaction of oxygen, where he insisted that the use of Cailletet's apparatus was widespread in France, and that Cailletet's methods were not secret because he had presented them in public both at the International Electrical Exhibition in Paris in 1881 and later at Debray's laboratory at the École Normale (Wróblewsk and Olszewski 1884, 119). Wróblewski's experiments had begun a year before Cailletet returned to Paris to work with ethylene (Wróblewski 1885, 8).

Wróblewski began his experiments on the solubility of carbon dioxide in water, mainly because he could use Andrews's data for the relations between the volume, pressure and temperature of this particular gas. His experiments led him to the creation of the carbonic acid hydrate, and were warmly welcomed by the Académie (Wróblewski 1882).[9] By the time Wróblewski published his first note on carbonic acid hydrate, at the beginning of February 1882, Cailletet had returned to Paris with a new pump in order to start his experiments with ethylene. However, when he learned about Wróblewski's experiments on hydrates, he decided to start working on hydrates as well, and to postpone his new series of experiments on the liquefaction of oxygen. Wróblewski described in rather derogatory terms Cailletet's engagement with this new line of research and questioned his method. But Cailletet managed to produce the hydrate of hydrogen phosphate, and some other substances that he had not examined yet.[10] According to Wróblewski, Cailletet's concern,

[9] Wróblewski's studies on carbonic acid hydrate were published in a series of notes in 1882 at the *Comptes Rendus*, in which he acknowledged the hospitality shown to him by Debray. See for instance (Wróblewski 1882).

[10] Although Cailletet and Bordet's research was not complete, they published a note also in order to secure the study of the bodies that they had not managed to identify (Cailletet and Bordet 1882).

more than anything else, was to stay in history as a trailblazer in high-pressure chemistry (Wróblewski 1885, 9).

When Cailletet's 'razzia' (the term Wróblewksi used) to establish his priority in the field had simmered down, he started his experiments with ethylene. He conducted his experiments on the complete liquefaction of oxygen with the use of ethylene in March and April 1882 in Paris. But as we have already seen, his efforts failed once again. Because of these unsuccessful attempts, Cailletet considered the possibility of liquefying other gases that were more difficult to liquefy than ethylene, so as to lower even more the limit of 'extreme cold' (Cailletet 1882). In Wróblewski's view, such a proposal was a sign that Cailletet had not thought of evaporating ethylene in a vacuum.

Wróblewski described Cailletet's attempts to use ethylene as a powerful refrigerant in disparaging terms. Cailletet, he wrote, was unsuccessful both because his technique was faulty and because he failed to overcome the problems he encountered in his experiments. Hence, in the end, Cailletet succeeded in liquefying oxygen only after being introduced to Wróblewski's and Olszewski's method for the liquefaction of oxygen, nitrogen and carbon monoxide, which included the slow expansion of ethylene and the use of boiling ethylene in a vacuum (Wróblewski and Olszewski 1883b). Wróblewski agreed with Jamin on one point, however; '[Cailletet] n'est point un savant de profession, mais bien un curieux'. He had the material resources and wanted to make himself useful to science (Wróblewski 1885, 12; Jamin 1884, 97).

To show that he had the support of the French academic community, Wróblewski reproduced a series of letters addressed to him on the occasion of the liquefaction of oxygen. Perhaps the most telling one was a letter of April 24, 1883, in which Debray regretted that Cailletet seemed to lack Wróblewski's patience and perseverance. Cailletet, according to Debray, had an ingenious spirit but no truly scientific method. A few months later, on January 1, 1884 Debray replied to Wróblewski's note on the temperature of boiling oxygen in enthusiastic terms, and underlined the latter's great skill and remarkable determination (Wróblewski 1885, 23).

On his arrival in Paris in April 1884, Wróblewski found Cailletet in a state of defeat. Although he had reported to the Académie on November 19, 1883 that he was about to construct a new apparatus with which he would be able to liquefy oxygen in large quantities (Cailletet 1883), he confessed to Wróblewski that 'oxygen did not want to flow' (Wróblewski 1885, 25). Wróblewski tried to encourage Cailletet to continue his research, insisting that research on the liquefaction of gases was just beginning and Cailletet had plenty of time ahead of him for new achievements. Wróblewski also gave Cailletet practical advice concerning his new apparatus and the reasons why, in Wróblewski's view, Cailletet's experiments failed. As Wróblewski believed, although the experiments were very expensive, Cailletet had the resources to complete them and would have done so if only he had been more patient (Wróblewski 1885, 26).

2.9 Conclusion

Priority disputes about the liquefaction of gases, like the one between Cailletet and Wróblewski on the liquefaction of oxygen, were common enough throughout the first three decades of the history of gas liquefaction. After Cailletet's and Pictet's proof that oxygen could be liquefied, there was no doubt that the rest of the permanent gases could be liquefied as well. The liquefaction of hydrogen, finally achieved by James Dewar in 1898, was also to provoke intense controversy between early low-temperature researchers. Cailletet had claimed to have seen hydrogen mist in 1877, Raoul Pictet to have liquefied it in 1878, Wróblewski to have done so in 1884, and his former collaborator Olszewski claimed to have succeeded in 1885. However, once oxygen had been obtained in a stable form in 1883, whoever wanted to claim priority in the liquefaction of hydrogen had to obtain something more substantial than a transient mist of the gas.

The liquefaction of hydrogen posed a great experimental challenge since its critical temperature was estimated to be about −243 °C. As already mentioned, Cailletet had liquefied oxygen by compressing it and lowering its temperature with a simple expansion of the gas, without making use of the Joule-Thomson effect, known since 1852. It was Dewar who made systematic use of the Joule-Thomson effect with his liquefying apparatus after having learned about the effect's efficacy for cooling around 1895. In addition, Dewar made use of Wróblewski's deductions regarding the critical point of hydrogen after the latter's study of the isothermals of the gas and his use of Van der Waals's law of corresponding states (Rowlinson 2012, 114). Hydrogen was finally liquefied in May 1898, by what has been called a 'brute-force' approach (Gavroglu 2009). To demonstrate the decisive role of van der Waals's early work in the development of the field of low temperatures, Kostas Gavroglu has used as an example Dewar's failure to liquefy helium and has contrasted it with the research program of Heike Kamerlingh Onnes. In contrast with Dewar's lack of programmatic claims, Kamerlingh Onnes's main motivation for the liquefaction of helium was not the experimental challenge as such but the provision of supporting evidence for Van der Waals's law of corresponding states, and the generalization he himself provided in 1881 (Gavroglu and Goudaroulis 1991, lxiv).

In a similar manner, Cailletet's failure to produce liquid oxygen in a stable form and his dispute with Wróblewski may also be grounded on the former's 'atheoretical approach' and the theoretically informed approach of Wróblewski. Cailletet was a man of independent means and a skillful experimentalist. Apart from the chemistry laboratory of the École Normale, he had his private laboratory in his hometown where he was able to use in considerable quantities coolants, such as liquid ethylene, which were expensive and difficult to acquire. It was Wróblewski and Olszewski, however, who, with rudimentary means at the Jagiellonian University of Cracow, were able to produce liquid oxygen 'boiling quietly in a test tube'

(Mendelssohn 1977, 25). It appears that the combination of the more 'theoretically-minded' Wróblewski and the 'dexterous and inventive' Olszewski made up for the lack of resources and old equipment in Cracow (Delft 2007, 216). Moreover, it has been argued that Wróblewski and Olszewski succeeded where Cailletet failed because they had a better understanding of the physical principles involved in gas liquefaction (Mendelssohn 1977, 25). Both of them worked on the estimation of the critical temperatures and other physical constants of gases, used Van der Waals's law of corresponding states, and measured their isothermals. Cailletet, in contrast, never seemed greatly interested in the theory behind experimentation, even though he had inspired Kamerlingh Onnes through the boldness of his experiments and techniques (Sengers 2002, 89). The various comments both by Wróblewski and by Debray concerning his lack of 'scientific method', his lack of patience and his lack of perseverance may have alluded also to this characteristic.

It is interesting to examine why priority disputes were so frequent between 1877 and 1908. The liquefaction of oxygen initiated the emergence of a new research field, characterized by the introduction of new experimental techniques, initially in the absence of any particular theory to 'guide' the experiments. The early experimentalists themselves were not entirely confident about the results they obtained, and the same was true of the eyewitnesses who were called to testify to the success of their experiments. An important element in this confused situation was that the new research field involved not only the liquefaction of all the permanent gases but also the investigation of the fundamental properties of matter at very low temperatures. Work of such potential importance carried with it the promise of fame for its pioneers as well as of national pride. It is therefore not surprising that Cailletet aspired to a place in history as the 'founder of high-pressure chemistry' and that he was supported in his ambition by the French academic community, even though he did not always stand up for science in the way his peers expected him to do.

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Chapter 3
Lindemann and Einstein: The Oxford Connexion

Robert Fox

Abstract Albert Einstein's friendship with Oxford's professor of experimental philosophy Frederick Lindemann resulted in three annual visits that he made to the university, beginning in 1931. The visits, each of about a month, helped to promote Lindemann's ambitions for Oxford physics, then struggling for recognition in what was still predominantly an arts university. Einstein, in return, settled comfortably into college life in Christ Church, where he was housed, and was left free to pursue his mathematical and other interests. Among his activities, the three Rhodes Lectures (on recent developments in physics) that he delivered in 1931 and his Herbert Spencer and Deneke Lectures of 1933 were public highlights. But the personal friendships he established within Christ Church and in Oxford's musical circles also mattered to him. When the menacing turn of events in Nazi Germany led Einstein to seek a new life at the Institute for Advanced Study in Princeton in October 1933, Lindemann was instrumental in maintaining Christ Church's invitation for him to return to Oxford for short annual stays through to 1936. In the event, Einstein never returned, and the funding allocated for his visits was used (at his request) to assist academic refugees, a number of whom left an enduring mark on science in Oxford.

Keywords Frederick Lindemann • Albert Einstein • Oxford • Academic refuges • Physics

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This essay presents a first ordering of material from an ongoing study of Einstein's time in Oxford, and it is informed by the main archival sources used in the larger study. Although I do not systematically cite these sources here, I wish to acknowledge my debt for access to archives in the keeping of: Judith Curthoys, archivist of Christ Church, Oxford; the Warden of Rhodes House and Secretary to the Rhodes Trustees; and the Librarian of Nuffield College, Oxford (responsible for the papers of Frederick Lindemann, Lord Cherwell). I am also grateful to Keith Moore, librarian of the Royal Society for drawing my attention to the accompanying portrait sketch of Einstein and arranging for it to be reproduced. On the portrait, which was recently presented to the Royal Society by Professors Deborah and Brian Charlesworth, see McManus (2014).

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Frederick Lindemann and Albert Einstein first met in 1911 at the Solvay conference on physics in Brussels. After the conference, Lindemann wrote to his father that he "got on very well with Einstein"; only Hendrik Lorentz had made a greater impression on him.[1] As a fluent German speaker and one of the two secretaries of the conference (with Louis, duc de Broglie), Lindemann had easy access to Einstein, and their cordial (though hardly intimate) friendship endured until Einstein's death in 1955. While Lindemann never saw himself as approaching Einstein's stature as a physicist, he was from the start a champion of relativity theory and did much to promote Einstein's reputation in Britain.[2] From 1919, he did so from his position as the Dr Lee's professor of experimental philosophy at Oxford. His brief as professor was not easy. The university's Clarendon Laboratory had become a distinctly sad place in the later years of Robert Bellamy Clifton's long tenure of the chair.[3] Clifton had shown little interest in research, and his retirement in 1915 (50 years after his appointment and shortly before his eightieth birthday) had come as something of a relief to the university authorities. Much was expected of Lindemann, and Lindemann himself had ambitions to match. He arrived in Oxford with experience of Walther Nernst's laboratory at the University of Berlin, where he took a doctorate for a study of specific heats at very low temperatures in 1910. And he probably dreamed of recreating something of the intense research-oriented atmosphere that he had imbibed in Berlin. But in a post-war university beset with crippling financial difficulties, his room for manoeuvre was limited.

Jack Morrell has shown how, through the 1920s and 1930s, Lindemann marshaled the meager resources available to him in a way that gradually transformed Oxford into a major center for physics.[4] Among the resources that he used was his association with Einstein. In June 1921, early in his 37 years as professor, he took advantage of Einstein's brief stay in Britain (on his way back from the USA to Germany) to drive him from London for a day in Oxford. Five years later, he was quick to see the Rhodes Memorial Lectures, just founded to mark Oxford's gratitude for the munificence of Cecil Rhodes, as another opportunity of engaging Einstein in his plans for the Clarendon Laboratory. His attempt to have Einstein appointed as the first Rhodes Lecturer, for 1927, failed, despite the concerted efforts of Lord Haldane, the German politician and former diplomat Johann Heinrich von Bernstorff, and the historian H. A. L. Fisher, one of the

[1] Lindemann's comment is cited in Clark (1971, 144–5). For biographical information about Einstein, I draw not only on Clark's *Einstein* but also on other biographical sources, notably Isaacson (2007, Chaps. 16 and 18). On the theme of this essay, I owe much to discussions with Dr Paul W. Kent, Dr Lee's Reader in Chemistry at Christ Church from 1956 to 1972 and author of "Einstein at Oxford", in Biller (2005, 5–11).

[2] Of biographical sources on Lindemann, I have made particular use of Smith (1961) and Fort (2003, especially Chaps. 4–6). Harrod (1959) offers unique perceptions of Einstein informed by encounters in the Christ Church Senor Common Room.

[3] On these years in the Clarendon, see Fox (2005) and Gooday (2005).

[4] Morrell (1997, Chap. 9). See also Morrell (1992, 2005).

Rhodes trustees. An invitation was issued, but Einstein declined for a miscellany of reasons, including poor health, the difficulty of absenting himself from his duties at the Kaiser-Wilhelm Institute for Physics and the University of Berlin, and his judgement that the subject he might address would be of insufficient interest.[5] Characteristically, Lindemann refused to let the matter drop. By May 1930, he was campaigning to have Einstein invited again, and 6 months later Einstein finally accepted an invitation to deliver at least two Rhodes Lectures (eventually determined as three) in the following May, in return for a fee of £500.

Lindemann was resolved that Einstein's stay, of four weeks, should be a success. When the *Albert Ballin* arrived in Southampton from Hamburg on 1 May 1931, he went on board to greet Einstein and conducted him to shore in a private launch and then on to Oxford in his chauffeur-driven car. There Einstein was introduced to Christ Church, Lindemann's college and his home for most of his time in Oxford. Confronted with the formalities of the college High Table, Einstein found the first evening's dinner a "bizarre and boring affair".[6] But once he was settled in the comfortable rooms of the classical tutor Robert Hamilton Dundas, who was away from Oxford for the term, he warmed to the traditions of college life and entered on a round of sociable engagements ranging from discussions with the mathematicians Edward Arthur Milne and G. H. Hardy and the classical scholar and leading internationalist Gilbert Murray to musical evenings at the Oxford home of Helena and Margaret Deneke, the talented daughters of the late Philip Maurice Deneke, a German-born banker, and his widow Clara Sophia Overweg.

On Saturday 9 May, Einstein delivered his first lecture. The setting, in the Milner Hall of the Rhodes Trust's headquarters, Rhodes House, was grand, and the audience was large, probably over 400. Yet it is not clear how successful the lecture was. The subject, "The outlines and outstanding problems of the theory of relativity", was demanding and was made the more so by Einstein's lecturing in German, which few in the audience could follow at all easily.[7] The second lecture, a week later, was more accessible, and Einstein himself was happier with it. Entitled "The cosmological problem", it treated the evidence for the expanding universe

[5] Lindemann's determination to secure Einstein as a Rhodes Memorial Lecturer emerges strongly from correspondence in the file "Rhodes Memorial Lectures 1929–1969", in the papers of the Rhodes Trustees at Rhodes House, Oxford.

[6] Quoted in Eisinger (2011, 124). On Einstein's impressions of Oxford, his travel diaries are revealing. The diaries are in the Einstein papers, now in the Albert Einstein Centre of the Hebrew University, Jerusalem, with copies and transcriptions held by the Einstein Papers project at the California Institute of Technology. I am grateful to Professor Diana Kormos-Buchwald, director of the project at Caltech, for allowing me to see relevant extracts from the diaries. I have also drawn on Eisinger's *Einstein on the road*, especially Chaps. 6 and 8, which treat Einstein's visits to Oxford, with passages from the diaries translated into English.

[7] The texts of the three lectures have not survived. Short reports appeared in *Nature*, 127 (16 May 1931), 765; 127 (23 May 1931), 790; and 127 (30 May 1931), 826–7. And typed summaries, annotated in pencil (probably by Lindemann), are in the Cherwell Papers, Nuffield College, Oxford. The titles as given in *Nature* do not entirely match those given in the announcements of the lectures in the *Oxford University gazette*.

manifested in the red shift in the spectra of extra-galactic nebulae, a subject of great contemporary interest. (It was during this lecture that Einstein wrote on the blackboard that is now preserved in the Museum of the History of Science in Oxford.[8]) The third lecture was delivered on the following Saturday, 23 May, immediately after a special ceremony in the Sheldonian Theatre in which Einstein was awarded an honorary doctorate of science. Of the three lectures, this seems to have been mathematically the most demanding, and the by now greatly reduced audience found Einstein's treatment of elements of Riemannian geometry and the quest for a unified field theory embracing both gravity and electromagnetism tough going. As Einstein recorded in his diary, it proved too much for the Dean of Christ Church, who slumbered peacefully through most of it.[9]

By the time of Einstein's last day in Oxford, 27 May, his stay had fulfilled Lindemann's core expectations. Most importantly, it had served to draw attention to physics as a discipline of international significance within the university. It had also raised Lindemann's hopes of establishing a continuing association that would bring Einstein back to Oxford on a regular basis. The key to this plan was Christ Church, where Einstein had created a favorable impression among the "students" (the college's unusual term for its fellows) and reinforced his friendship with Lindemann, whose bachelor rooms (with adjacent accommodation for his man-servant) were not far from his own. On 28 June, at Lindemann's urging and after extensive internal agonizing about the wording, the Dean of Christ Church sent Einstein a formal invitation to spend a period of roughly a month a year in the college, over 5 years, in return for a stipend of £400 p.a. and accommodation. Each stay would leave Einstein with as much freedom as possible (something to which Lindemann attached great importance), the only expectation being that he would pursue his research. Competing invitations from the group of astronomers and physicists centered on Edwin Hubble, at the California Institute of Technology and the associated Mount Wilson Observatory were probably responsible for Einstein's delay in replying. But by the end of October, he had accepted Christ Church's invitation, and in April 1932 he duly arrived back in Oxford for what was intended to be the first of his five annual visits.

Again, Einstein settled into rooms in the college's imposing main quadrangle, Tom Quad; not Dundas's rooms this time, but others nearby. His activities resembled those of the previous year: a mixture of conversation in the Christ Church Senior Common Room and over dinner on High Table, discussions with scientific colleagues, walks, and music-making with the Deneke family. When he eventually returned to his rural retreat at Caputh near Potsdam in June 1932, it seemed likely that the 5-year cycle of visits would run its intended course. But the seeds had already been sown for the dramatic reorientation of Einstein's plans that, in the

[8] Museum of the History of Science, Oxford, inventory number 44,725; an image of the blackboard, with a commentary, is in the museum's collection database. A photograph of the blackboard and a note on its content are also in Kent (2005, 7).

[9] Eisinger (2011, 130).

following year, was to take him away from Europe for good. They had been sown by Abraham Flexner who, on a visit to Einstein in Christ Church, had raised the possibility of a position at the Institute for Advanced Study, then at an advanced stage of planning in Princeton.

At the meeting in Oxford, Einstein had prevaricated, but a subsequent encounter with Flexner in Berlin later in the year and the deteriorating conditions of work for Jewish scientists during the winter of 1932–1933 made the invitation from Princeton increasingly attractive. By the time Einstein returned to Oxford at the beginning of June 1933, the dye was cast. After a spring in which he had renounced his German nationality, relinquished his passport, and resigned from the Prussian Academy of Science, he was stateless and virtually cut off from the German scientific community. Even his life was in danger, to the point that he had spent the weeks between his return from a winter at Caltech in March and his departure for England 2 months later in the relative safety of a rented vacation home at Le Coq sur Mer on the Belgian coast. Once again, though, Christ Church provided a safe haven, albeit this time a busy one. In Oxford, there were two public lectures to give: the prestigious Herbert Spencer lecture, which Einstein delivered at Rhodes House in English, and the Philip Maurice Deneke lecture, in honor of the Deneke sisters' late father, delivered in German at Lady Margaret Hall.[10] These ate into what Lindemann and the college had intended as time for personal research and reflexion, as did the unrelieved anxieties about the political situation in Europe. After the month in Christ Church, continuing fears for his security led to his dividing the rest of the summer between stays back at Le Coq sur Mer and in Norfolk in the secluded home of the German-speaking conservative Member of Parliament and friend of Churchill, Oliver Locker-Lampson. In these weeks of relative isolation, Einstein visited Churchill at Chartwell and on 3 October, in a rare appearance on the national stage, spoke on "Science and civilization" in a packed Royal Albert Hall in support of the Refugee Assistance Fund.[11] Four days later, he left Southampton for New York and his new life at the Institute for Advanced Study (Fig. 3.1).

Einstein was never to return to Europe. Nevertheless, the plan for three further annual visits to Christ Church remained in place, and Lindemann and others in the college were anxious to honor their invitation. Einstein, for his part, showed no sign of wishing to sever his link with Oxford prematurely. But on 21 November 1933, barely a month after his arrival in Princeton, he wrote to Lindemann suggesting that, in place of the following year's visit, his annual stipend for 1934 should be

[10] The Herbert Spencer lecture was published as *On the method of theoretical physics* (Oxford: Clarendon Press, 1933), after translation from Einstein's German by his friends Gilbert Ryle, Denys Page, and Claude Hurst, all students of Christ Church. It is conveniently reproduced in Biller (2005, 12–19). The title of the Deneke lecture was "Einiges zur Atomistik"; see *Oxford University gazette*, 63 (1932–1933), 588 (8 June 1933).

[11] A film of Einstein speaking is available on You Tube at http://www.youtube.com/watch?v=ZBage5Ff57E, accessed 17 June 2014.

Fig. 3.1 Sketch of Einstein, in red chalk (sanguine), by Ivan Opffer, dated 1933. The sketch was almost certainly made during one of Einstein's stays in England in that year, before his final departure for the USA on 7 October (Reproduced with permission from the library and archives of the Royal Society. For further details, see the article by Joanna McManus (2014)

reallocated to help German scientists and scholars affected by the racial laws.[12] In May 1934, in a letter to the Dean of Christ Church, he repeated the suggestion, and the college took immediate action, providing £200 to support the classical philologist Eduard Fraenkel, recently dismissed on racial grounds from the University of Freiberg, and reserving the remaining £200 to facilitate visits to Oxford by German physicists.[13] Among the physicists who were considered and even approached as possible recipients, Max von Laue appears to have come close to an agreement with Christ Church. But, despite a visit to Oxford (during which he stayed with Erwin Schrödinger, now embarked on the three unhappy years he spent as a fellow of Magdalen College[14]) and almost certainly a discussion with the Dean in May 1934, he decided to stay in Germany, where he continued to direct the Kaiser-Wilhelm Institut für Physik as a declared critic of the Nazi regime through the second world war.

By now, the contribution that Christ Church could make to the support of displaced scientists was beginning to look a rather modest drop in the ocean of difficulties resulting from Hitler's assumption of dictatorial powers in March 1933. Hence it was more in hope than realistic expectation that the college invited Einstein to make another of his scheduled visits in the spring of 1935. Einstein

[12] Einstein to Lindemann, 21 November 1933, D57/26, Cherwell Papers, Nuffield College, Oxford.

[13] Einstein to the Dean of Christ Church, 9 May 1934, DP xx.c.1, Christ Church Archives.

[14] Schrödinger's difficulties in Oxford are discussed in Hoch and Yoxen (1987, 593–616).

declined, as he also did when invited, for the last time, for a stay in 1936. Although there is no reason to doubt the genuineness of his regret at being unable to accept the invitations, he was pleased to see college money being used to facilitate the relocation of German academic colleagues, though now in the broader context of the aid given by the Academic Assistance Council, a voluntary organization founded in 1933 to help German and Austrian scientists and scholars who had fallen foul of the anti-Semitic racial laws of that April.

In Lindemann Einstein had a loyal friend, always ready to act in his best interests and speak as his champion within Christ Church, and the friendship never waned. But the political events of 1933 imposed an abrupt adjustment in the priorities of both men. A salary, variously reported as 15,000 or 16,000 dollars a year and fine conditions for work in Princeton gave Einstein long-term security; and set against what Oxford might offer him, the needs of the wider community of Jewish refugees from Nazism were now of overriding import. On those needs, Lindemann shared Einstein's sensitivity. But he also understood, as a patriot, how helping the refugees to continue their careers in Britain would advance the cause of British science. Within this broader British context, he never lost sight of his particular aspirations for the Clarendon Laboratory, and these plainly informed his attempts to recruit in Oxford's already strong areas of low-temperature physics, in which the Clarendon had become the first laboratory in Britain to achieve the liquefaction of helium (in January 1933), and spectroscopy, the established speciality of the brilliant, hard-riding Derek Jackson.

Lindemann did his recruiting by correspondence and more immediately through the personal contacts he made during the Easter vacation of 1933, when he took his car (chauffeur-driven, as usual) to Germany, visiting the universities of Berlin and Göttingen, and probably others. Back in England, in May, he saw Franz Simon, the leading German low-temperature physicist, and secured the posts (with the aid of short-term financial support from Imperial Chemical Industries, for which he lobbied the company's chairman, his friend Sir Harry McGowan) that allowed Simon to resign from his chair at the Technische Hochschule in Breslau in June and to move to Oxford with his younger collaborator and former pupil Nicholas Kurti.[15] There, on a salary of £800 p.a., Simon joined his cousin Kurt Mendelssohn (who had advised on the liquefaction of helium project and been in Oxford on ICI money since April) and was soon to be joined by Heinrich Kuhn from Göttingen and the brothers Fritz London from Berlin and, in 1934, Heinz from Breslau.

The new arrivals in Oxford did not form an entirely stable group. Fritz London, partly because of the distance between his theoretical interests and the Clarendon Laboratory's experimental orientation, never settled.[16] Once his grant from ICI expired in 1936, he lived precariously before leaving for the USA in 1939. And other key figures moved on as opportunities presented themselves. Heinz London

[15] On Simon's troubles in Germany and his move to Oxford, see McRae (2014, Chaps. 3 and 4).

[16] Gavroglu (1995, 129–35). On the gulf between the experimental orientation of the Clarendon under Lindemann and the theoretical interests of the Londons (especially Fritz), Schrödinger, and Szilard, see also Hoch and Yoxen, (1987, 604–7).

(unhappy in Oxford, like his brother[17]) went to a post in Bristol; Schrödinger, a friend but never a member of the Clarendon-based team, moved on to Graz in 1936 and finally to Dublin in 1940; and Leo Szilard (another of Lindemann's recruits, who spent 2 years in the Clarendon) left for the USA. But Simon, Mendelssohn, Kuhn, and Kurti passed the rest of their careers in Oxford. The establishment of such a strong team, despite the university's financial precariousness and the now dated facilities of the ageing Clarendon Laboratory, was Lindemann's greatest achievement as professor. It owed much to his vision for the laboratory and his capacity (despite a cool demeanor) to make alliances, whether of genuine friendship or convenience, within and beyond the academic world.

The events of 1933 left Einstein and Lindemann with important shared goals in support of the refugee physicists they were both committed to assisting. But they put an end to the plan for an ongoing association bred of a friendship with roots in a more tranquil time. The enforced termination of the promising Oxford-based collaboration invites speculation on what might have been. Perhaps further regular visits by Einstein would have tempered the predominantly experimental style of Oxford physics with a more theoretical cast. The same query might be raised with regard to the other theoreticians whom Lindemann recruited: the Londons, Schrödinger, or Szilard. We cannot know, of course. What can be said, however, is that Einstein's Rhodes Memorial Lectures of 1931 and his month-long stays then and in 1932 and 1933 formed part of the armory that Lindemann deployed in his long struggle to present Oxford, hitherto seen as predominantly an "arts university", as a proper home for physics of international standing. Einstein's influence on the future course of Oxford physics was certainly not comparable with that of Simon (Lindemann's natural successor in the Dr Lee's professorship in 1956) and the other refugees who went on to invigorate the Clarendon by their presence for two decades and more. But memories of his visits and friendship with Lindemann lingered long after he left Christ Church for the last time at the end of June 1933. In that respect, as a much-needed gauge of Oxford's seriousness with regard to physics, Einstein's three brief stays, totaling barely 3 months in all, had an enduring legacy.

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Chapter 4
Einstein and Hilbert

John Stachel

Abstract Highlights of the twenty-odd-year relationship between Einstein and Hilbert are reviewed: the encounter that never took place (1912) when Einstein declined Hilbert's invitation to Göttingen; the fateful encounter (1915–1916) leading to a dispute over the final formulation of general relativity; the tragic-comic encounter (1928–1929) over editorship of the *Annalen der Mathematik* leading to what Einstein called "The battle of the Frogs and Mice"; *L'envoi* (1932) Einstein's final letter of congratulations to Hilbert on his 70th birthday.

Keywords Einstein, Albert • Hilbert, David • Gravitational theory • Brouwer-Hilbert controversy

4.1 The Encounter That Never Took Place (1912)

On 30 March 1912, David Hilbert, the eminent Göttingen mathematician, wrote Albert Einstein, then still living in Zurich:

> Highly esteemed colleague,
>
> I would be very happy if I had your theoretical works on gas theory and radiation theory in my possession. [My translation – I am trying to reproduce the slightly pompous tone of Hilbert's German] (CPAE5 1993, p. 439)

What Hilbert had in mind is made clear by Einstein's letter of 4 October 1912 (CPAE5 1993, p. 502), politely declining Hilbert's request that he deliver a lecture on the kinetic theory of matter at Göttingen. Einstein gave two reasons: He had nothing new to say on the subject, which was not quite true; and he was completely occupied with other matters, which was quite true. The nature of these "other matters" is made clear in a letter of 1 November 1912 from Arnold Sommerfeld to Hilbert:

Dedicated to Kostas Gavroglu with whom I share so many happy memories of Athens, Berlin, Boston and Corfu, and from whom I have learned so much.

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> Einstein is apparently so deeply mired in gravitation that he is deaf to everything else. (CPAE5 1993, p. 506, note 6)

Well, not quite everything else. He made a trip to Berlin in April of that year, during which he was offered a position at the *Physikalisch-Technische Reichsanstalt*. From the people he met during this trip and the institution named, it is clear that, as in the case of the Göttingen invitation, it was not primarily his work on relativity and gravitation, but his work on the quantum theory of solids that led to the Berlin offer. He declined this offer, but this Berlin trip was the beginning of a connection that a year later led to his appointment as a full member of the Prussian Academy of Sciences, and his subsequent move to the German capital.

The attractions of Berlin were not exclusively intellectual. During his 1912 visit, he had renewed acquaintances with his uncle Rudolf Einstein ("the rich" as Einstein called him), now retired, his wife Fanny, and their daughter Elsa Löwenthal, Einstein's cousin. Divorced in 1908, she and her two daughters had joined her parents in Berlin. Thus began an affair that ultimately led to Einstein's 1919 divorce from his first wife, Mileva Marić, and marriage to Elsa.

4.2 The Fateful Encounter (1915–1916)

Before getting to the first meeting between Hilbert and Einstein, I must mention something that did *not* happen, but served to create a bond of sympathy between the two: Neither agreed to sign the notorious "Manifesto of the 93 [German Intellectuals] To the World of Culture." This document tried to justify, in the name of German "Kultur," the German invasion of neutral Belgium in 1914, soon after the outbreak of WWI. Einstein joined in a futile effort to launch a pacifist counter-manifesto "To the Europeans." It garnered only three signatures, and was not published until much later. Hilbert was not among the three, but Einstein valued him highly for his independence of judgment, writing in mid-1915:

> One is doubly overjoyed in these times by the few men, who stand quite above the situation and do not let themselves be driven by the sad currents of the time. One such is Hilbert, the Göttingen mathematician. . . . Hilbert now regrets doubly, as he told me, out of negligence not having cultivated more international connections. (AE to Heinrich Zangger, 7 July 1915, CPAE8A 1998, 144–145)

The occasion for this letter was an account of Einstein's first meeting with Hilbert:

> I spent a week in Göttingen, where I learned to know and love him [Hilbert]. I gave six two-hour lectures there about my now very much clarified gravitational theory and experienced the joy of completely convincing the mathematicians. (*ibid.*)

Einstein's joy at the sympathetic reception of his work by the Göttingen mathematicians, whom he had earlier scorned for what he regarded as mathematical pedantry, contrast sharply with his disappointment at the response of his colleagues in physics:

> Physicist humanity reacts rather passively to the gravitational work… Laue is inaccessible to arguments of principle, Planck also is not, Sommerfeld is a bit. A free, unconstrained outlook is really alien to the (adult) German. (Einstein to Michele Besso, after 1 January 1914, CPAE5 1993, 588–589)

At this point, a word of caution is necessary. Although Einstein is already beginning to speak of "general relativity" around this time, the theory he discussed in Göttingen was *not* the one that we now denote by that name. Rather, it was a variant of the so-called Einstein-Grossmann *Entwurf* theory, first formulated in 1912–1913, the field equations of which are not generally covariant. As we shall see, it was only in four papers dating from the late fall and early winter of 1915 that Einstein propounded the generally-covariant theory that we all know and (at least some of us) love as general relativity.

Among the enthusiasts attending Einstein's Göttingen lectures was Hilbert. He owed much of his already-considerable fame to his rigorous axiomatization of Euclidean geometry, and for several years had been attempting to apply his axiomatic method to the foundations of physics (Corry 2004). With the help of Max Born, then a *Privatdozent* at Göttingen, he was working on Gustav Mie's electromagnetic theory of matter. On the basis of a set of non-linear (and non-gauge-invariant) electromagnetic equations, this theory aimed to offer a field-theoretical explanation of the existence, structure and stability of the electron. Hilbert conceived the idea of enlarging Mie's program by combining it with Einstein's. He wanted to conjoin Mie's electromagnetic four-potential q_i with Einstein's ten gravitational potentials $g_{\mu\nu}$ in a set of non-generally covariant field equations to produce what we would now call a unified field theory of gravitation and electromagnetism (Renn and Stachel 2007).

While Hilbert was embarking with great enthusiasm on this program, Einstein was becoming more and more unhappy with the Einstein-Grossmann theory; and in mid-1915 returned to his earlier search for a generally-covariant gravitational theory. But the common ground between Einstein and Hilbert's programs soon led to some friction between the two men, with Einstein accusing Hilbert of wanting to "nostrify" [i.e., make his own] Einstein's work. I shall not here rehearse the details of the dispute except to note that it was quickly and happily resolved (Stachel 1999).

Indeed, on 5 December 1915 Hilbert and four colleagues proposed Einstein for membership in the Royal Society of Göttingen, to which he was duly elected on 18 December. Two days later, in a letter thanking Hilbert, Einstein wrote:

> I am taking advantage of this opportunity to tell you something else, which is more important to me. There has been a certain strained atmosphere [*Verstimmung*] between us, the cause of which I shall not analyze. I have fought against the feeling of bitterness associated with it, and indeed with complete success. I think of you again with unclouded friendliness and beg you to attempt the same with me. It is objectively too bad if two real guys, who have made something out of this shabby world, should not mutually enjoy each other. (20 December 1915, CPAE8A 1998, 222)

It is perhaps worth noting that Hilbert did not reply to this letter, and indeed had no further correspondence with Einstein until 27 May 1916, when he responded to a post card from Einstein with some questions about Hilbert's paper on his new theory, which had just been published. Hilbert invited Einstein to visit Göttingen again and stay with him; but in spite of several invitations over the next few years, this third visit to Göttingen never took place, perhaps because of Einstein's poor health during the last years of WWI (he had a stomach ulcer). However, they continued to correspond over issues connected with Hilbert's paper.

As Jürgen Renn and I have indicated (Renn and Stachel 2007), Hilbert made several mathematical errors in the course of work on his new theory. Some have questioned whether a great mathematician like Hilbert could really have made the mistakes we suggested. I shall let another great mathematician, Gian-Carlo Rota, answer. In a section of Rota 1997 entitled "Do not worry about your mistakes," he writes:

> When the Germans were planning to publish Hilbert's collected papers and present him with a set on the occasion of one of his later birthdays, they realized that they could not publish the papers in their original versions because they were full of errors, some of them quite serious. Thereupon they hired a young unemployed mathematician, Olga Taussky-Todd, to go over Hilbert's papers and correct all mistakes. Olga labored for three years; it turned out that all mistakes could be corrected without any major changes in the statement of the theorems. There was one exception . . . At last on Hilbert's birthday a freshly printed set of Hilbert's collected papers was presented to the *Geheimrat*. Hilbert leafed through them carefully and did not notice anything. (Rota 1997, 201)

I might add: or at least he did not say anything!

4.3 The Tragic-comic Encounter (1928–1929)

There is a special folder in the Einstein Papers that bears the following caption, typed by Helen Dukas, his assistant:

> Professor Einstein wanted this correspondence kept in a special folder under the title:
> *Der Frosch-Mäuser Krieg*

My account is based in large part on the contents of this folder.

I remind you that "The Battle of the Frogs and Mice" is the name of a Greek mock-epic poem, *Batrachomyomachia,*[1] dating from classic times, which was written to make fun of the style and content of real epic poems, such as the *Iliad*. Evidently, even in ancient times there were scoffers and cynics who used satire as their preferred weapon.

At what "epic" struggle was Einstein scoffing? It was the well-known (to those who know it well!) controversy over the foundations of mathematics raging in the nineteen-twenties between Hilbert, the fighting formalist and Luitzen Egbertus Jan Brouwer, the ingenuous intuitionist.

[1] See Chapman (1888) for a classic English translation.

The Brouwer-Hilbert debate grew increasingly bitter and turned into a personal feud. The last episode was the "*Annalenstreit*" [battle over the *Mathematische Annalen*– JS], or, to use Einstein's words, "the frog-and-mouse battle." It followed the unjustified and illegal dismissal of Brouwer from the editorial board of the *Mathematische Annalen* by Hilbert in 1928 and led to the disbanding of the old *Annalen* cohort and the emergence of a new *Annalen* under Hilbert's sole command but without the support of its former chief editors, Einstein and Carathéodory (Stigt 1988, 3).

I have neither the expertise nor the desire to enter into mathematical or philosophical aspects of this battle, but shall only touch on its human side and in particular Einstein's role in it. This seems especially worthwhile since this episode is not even mentioned in any of the many Einstein biographies I consulted.

How did Einstein become involved in the controversy? It started in 1919 with a gesture of friendship and confidence on Hilbert's part: He invited Einstein to join the editorial board of the *Mathematische Annalen*, then the premier German mathematics journal. In 1928, as a result of increasing tension between Hilbert and Brouwer, Hilbert wrote to the other editors of the *Annalen*, asking their sanction for the removal of Brouwer from the editorial board. At first Einstein tried to pass off the matter with a joke:

> Herr Brouwer is an involuntary champion of Lombroso's theory of the close connection between genius and madness. (Einstein to Hilbert, 19 October 1928)

He refused to sign the letter of expulsion and, in a letter to Constantin Carathéodory, a fellow member of the board, attributed this move to a momentary fit of pique on the part of Hilbert.

> It would surely be best to pay no attention at all to this Brouwer-matter. I would never have thought that Hilbert could be capable of such outbursts of emotion. (Einstein to Carathéodory, 19 October 1928)

Carathéodory replied in confidence, explaining to Einstein that the expulsion seems to have been a carefully calculated move on the part of Hilbert and several other board members. Carathéodory added that he was resigning from the board, but asked Einstein to keep his resignation secret for the present because he did not want to appear to be taking sides against Hilbert.

In mid-1929 Max Born intervened in the ongoing dispute, hoping to induce Einstein to side with Hilbert, largely on the grounds that Hilbert was a dying man and his last days should not be clouded by this controversy (Max Born to Albert Einstein, 2 August 1929). Curiously, Born did not include Einstein's letter of refusal in his collection of their correspondence, published thirty-odd years later.

Hilbert was indeed seriously ill at the time, but recovered and did not die until 1943, at the age of 81, and then it was from complications of a broken arm.

The last item in the "Frog-Mouse War file" is Einstein's reply to a 1930 letter from Jacques Hadamard, the eminent French mathematician, with whom he was on very friendly terms. After his expulsion from the *Annalen*, Brouwer had approached

Hadamard, among others, to join him in founding a new mathematics journal, and Hadamard turned to Einstein for advice. Einstein replied:

> It was a vile dispute between Brouwer and Hilbert, for which nevertheless in my opinion Hilbert bears the main guilt. (15 November 1930)

Nevertheless, in spite of Brouwer's ill treatment by Hilbert and his good works and intentions, in view of Brouwer's well-known and well-deserved reputation for querulousness Einstein advised Hadamard to keep hands off.

4.4 L'envoi (1932)

Luckily, I can end my story on a more pleasant note. The last letter from Einstein to Hilbert is dated 26 February 1932, congratulating him on his seventieth birthday:

> If I cannot allow myself to follow you along all your daring avenues of thought, yet I am able to form for myself a picture of the strength and beauty of your thought, and am obliged to you for sessions of cloudlessly beautiful experience.

This is a good place to end my tale, before the horrors of the Third Reich finally engulfed Germany—along with almost all the rest of Europe—only ending in the *Götterdämmerung* of 1945, which Hilbert was blissfully spared.

Although known to have been be upset by the actions of the regime, and seeing many of his closest collaborators such as Max Born, Richard Courant and Emmy Noether forced into exile, Hilbert stayed on at Göttingen until his death in 1943. Einstein of course left Germany in 1933, never to return and, as far as is known, never again to be in direct contact with Hilbert.

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Chapter 5
Quantum Chemistry and the Quantum Revolution

Sam Schweber and Gal BenPorat

Abstract The recent advances in the use of density functional theory (DFT) in quantum chemistry and in material sciences are considered from the perspective of the quantum theoretical description of the microscopic world. A viewpoint is presented on how to think about the quantum revolution and how to fit the DFT developments into it.

Keywords Density functional theory • Hacking type scientific revolution • Quantum revolution

5.1 Introduction

Walter Kohn, the physicist responsible for the development of density functional theory to calculate the structure of atoms, molecules and solids, for which he shared the Nobel prize in chemistry in 1998 with John Pople, recalled that when he was a young man Eugene Wigner once said to him "that understanding in science requires understanding from *several different points of view*" (emphasis in original) (Kohn 1996). We believe the same is true for the history and philosophy of science. It is our somewhat different perspective on the development of quantum theory from the one that Kostas Gavroglu and Ana Simões (2012) offered in their magisterial *Neither Physics nor Chemistry*, and in particular, our differing point of view regarding the nature of the quantum revolution that motivate our remarks. What follows is indicative of a larger project that we are working on, namely, to characterize the nature of the quantum revolution. We have focused on some fairly recent developments in quantum

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This paper is dedicated to Kostas Gavroglu as a token of my admiration, affection and respect. Sam Schweber.

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chemistry: the use of density functional theory (DFT) to study and predict the structures and properties of molecules. DFT allows fairly accurate computations of the structure and properties of much larger molecules than is possible by other *ab initio* approaches, such as Hartree-Fock,[1] when supplemented by approximate, but accurate, methods to handle electronic correlations.[2] In fact, it has replaced such calculations and has done so in a remarkably short time. What made this possible is the availability and use of supercomputers[3]; the strong coupling between applied mathematicians, computer scientists and the physicists and chemists using DFT methods[4]; the production and the sharing of the computer codes using DFT by researchers making computations[5] in condensed matter physics, quantum chemistry, material sciences and in nuclear physics; ***and*** the needs of the pharmaceutical and chemical industries and companies developing new materials for use in aerospace, computing, building and other applications.[6] The interrelation between the various elements responsible for this dramatic transformation offers an interesting case study in "post-modern" science (Forman 2002, 2007, 2010). In *Neither Physics nor Chemistry*, Gavroglu and Simões stressed the impact of computing on quantum chemistry, and their book ends with a discussion of the 1970 Conference on Computational Support for Theoretical Chemistry (Gavroglu and Simões 2012, 237–243). With that temporal limitation, and with their focus on the process of creation of quantum chemistry as a discipline, theirs is a story of quantum chemistry while it was still a "modern" science.[7] We are pointing to further developments of the process of creating a science, that is, neither physics nor chemistry, but an amalgam of physics, chemistry, material sciences and computing. We are not competent to give a coherent overview of these developments, but hope that it will stimulate others to do so.

In Sect. 5.1.1 we present our thesis regarding the nature of the quantum revolution and its relation to Hacking's concept of scientific revolutions as broad changes in styles of scientific reasoning and their associated languages. In Sect. 5.1.2 we make some comments regarding the description of objects by quantum mechanics. In Sect. 5.1.3 we present the description of atoms and molecules in the models that have been most extensively used in *ab initio* calculations in quantum chemistry: the Thomas-Fermi model, the Hartree-Fock model and the density functional approach.[8] We do so at a fairly technical level in order to stress the continuity in the underlying quantum mechanical assumptions, and to emphasize that the dramatic transformations that quantum chemistry has undergone are not Kuhnian revolutions, nor are they Hacking-type revolutions introducing new styles of reasoning. They are, rather, a

[1] See Bethe and Jackiw (1968).

[2] See for example Ren et al. (2012).

[3] See especially Kenneth Wilson's plea to theoretical physicists making lattice gauge theory calculations to learn what quantum chemists using DFT had accomplished. Wilson (1990).

[4] See for example Musa (2012), Varga and Drisco (2011), Trindle and Shillady (2008).

[5] For the development of the RSPt code at Los Alamos see Wills et al (2010).

[6] See for example Höltje and Folkers (2003).

[7] We are using "modern" and "postmodern" in the sense introduced by Forman (2012).

[8] Our presentation is based on Kohn's Nobel lecture, Kohn (1999).

change in what are considered the relevant variables and a change in the "language" used. The technical material may be skipped. In our conclusion we return to scientific revolutions, re-examine our claim that the quantum revolution is what we have called a Hacking-type (HT) revolution in the light of the exposition given in Sect. 5.1.3, and make some claims regarding what has happened to quantum chemistry since the 1970s.

5.1.1 HT Revolutions

Our point of departure is the thesis that the quantum revolution constitutes a Hacking-type (HT) scientific revolution (Schweber and Wächters 2000; Belfer and Schweber 2015; Schweber 2015), named after Ian Hacking who characterized the probabilistic revolution of the nineteenth century (Hacking 1987)[9] in terms of what we consider the crucial novel feature of a Hacking type scientific revolution: its ***style of reasoning.***

Styles of reasoning are the constructs that specify what counts as scientific knowledge and constitute the cognitive conditions of possibility of science. They are made concrete through the specification of theories, their ontological assumptions, and their explanatory models. A style of reasoning introduces new types of objects, evidence, sentences, (new ways of being a candidate for truth or falsehood), laws, modalities, and most importantly, new possibilities (Hacking 1981a, b, 1982, 1983, 1985, 1992a, b, 1994, 1996, 2002, 2009, 2012; Kusch 2010). Styles of reasoning are bounded in scope with definite limits of applicability. Styles of scientific reasoning are "big": they have to encompass several scientific disciplines. Furthermore, different styles of reasoning can co-exist.

HT revolutions are emplacement-revolutions, rather than replacement-revolutions. They change the way science is practiced without necessarily jettisoning all the previous concepts, transforming it from within by a shift of the questions being asked and the criteria for acceptable answers, this being a characteristic of an "emplacement revolution" (Humphreys 2011).

HT revolutions amalgamate pure and applied concerns. They transform a wide range of scientific practices and are multidisciplinary, with new institutions being formed that epitomize the new directions. The time scale of HT revolutions is the *longue durée*, but the *durées* have become shorter as the scientific community has increased. HT revolutions are linked with substantial social change, and after a HT revolution there is a different feel to the world.

An HT revolution is characterized by a new style of scientific reasoning and conversely, the genesis of a new style of reasoning is indicative that an HT revolution is in the process of evolving, with self-authentication and self-stabilization characteristic features of the evolutionary process. Following Crombie (1994) Hacking gave the following examples of styles of reasoning: postulation in the mathematical sciences, ordering by comparison of variety and taxonomy, experimental exploration and measurement, the statistical analysis of populations, the derivation of genetic development.

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[9] In that same volume see the two other introductory essays by Kuhn (1987) and by Cohen (1987).

The HT scientific revolutions that are of particular interest to us have an additional feature: they make use of a characteristic ***language*** to formulate, corroborate, self-authenticate and self-stabilize the style of reasoning it introduced.[10] For the probabilistic revolution the statistical analysis of regularities of populations was its style of reasoning and the calculus of probabilities its language.

The style of scientific reasoning we associate with the HT quantum revolution is characterized by the hierarchization of the microscopic physical world and quantum field theory is its language (Cao and Schweber 1993; Schweber 2015). The hierarchization has the property that to a high degree of approximation each level has associated with it an ontology and a dynamics; the dynamics specifies the interactions of the entities that constitute the building blocks of that level and governs the description of the temporal evolution of systems composed of these entities. The dynamical theory of a given level can be formulated as an "effective quantum field theory". An "effective" quantum field theory gives a description of phenomena in a certain domain bounded by some energy less than some energy cut-off Λ. An effective field theory assumes that the physics in the domain in which it is valid can be given in terms of "elementary" entities out of which the composite entities that populate that domain are built. Certain properties of these entities–such as their empirically determined mass and spin, their various "charges"–are specified and manifest themselves as in the "one-particle" spectrum of the field theories associated with them. It is the function of a lower level, more "foundational", theory to account quantitatively for the parameters that enter in the formulation of the "effective" field theory of a given level. The use of effective field theories has been justified (Weinberg 1995–2000; Banks 2008; Duncan 2013).[11] There is an important consequence of

[10] We do attach great importance to the notion of language. And in this we differ from Gavroglu and Simões. Thus, we do not consider the shift from atomic orbitals to molecular orbitals as a change in styles of reasoning, but rather a change in the ***language*** to address the problem of chemical structure. See Gavroglu and Simões (2012, 6–7).

[11] Conversely, the success of the approach of describing physical phenomena in terms of effective field theories is a reflection of the fact that appropriately isolated physical phenomena in a certain energy regime, probed and analyzed by instruments able to resolve effects only within a certain range of length scales, ***can*** be described most simply by a set of effective degrees of freedom appropriate to that scale.

Relativistic quantum field theories implicitly make statements about arbitrarily short space-time distances, and thus about arbitrarily high energies and momenta. Yet no conceivable experiment will be able to probe such distances. When calculating the predictions of a given theory the contributions stemming from these high-energy, short-distance components are divergent. The renormalization program to circumvent these divergences which was developed by Weisskopf, Bethe, Schwinger, Feynman, and by Dyson after World War II was given a new and deeper interpretation by Wilson and others in the early 1970s. Wilson was able to exhibit the effect of all possible modifications of a given theory beyond a certain cut-off in energy as a re-parametrization of all possible interactions between the entities that are assumed to populate the low-energy domain of that theory. Furthermore, he showed that starting from any set of interactions at the cut-off scale, a low energy physics description at a given level of accuracy could be formulated that depended only on a few relevant parameters (Wilson 1983). The great accomplishment by Wilson, Weinberg and others was demonstrating the universality of the low-energy physics, which resulted from the renormalization process thus justifying the use of effective field theories. See Weinberg (1995–2000) and in particular volume 2, *Applications*.

the use of effective field theories: as long as one does not probe beyond the energy and length scales in which they are deemed applicable, the description of the physics in their domain is not invalidated by discoveries at lower levels.

The article "The Theory of Everything" written by two outstanding condensed matter theoretical physicists,[12] is illustrative of the scope of the approach (Laughlin and Pines 2000). It begins by stating:

> The Theory of Everything is a term for the ultimate theory of the universe-a set of equations capable of describing all phenomena that have been observed, or that will ever be observed (5.1). It is the modern incarnation of the reductionist ideal of the ancient Greeks, an approach to the natural world that has been fabulously successful in bettering the lot of mankind and continues in many people's minds to be the central paradigm of physics. A special case of this idea, and also a beautiful instance of it, is the equation of conventional nonrelativistic quantum mechanics, which describes the everyday world of human beings-air, water, rocks, fire, people, and so forth. The details of this equation are less important than the fact that it can be written down simply and is completely specified by a handful of known quantities: the charge and mass of the electron, the charges and masses of the atomic nuclei, and Planck's constant. For experts we write

$$H|\Psi\rangle = i\hbar \frac{\partial}{\partial t}|\Psi\rangle \tag{5.1}$$

$$\begin{aligned} H = &-\sum_{j}^{N_e} \frac{\hbar^2}{2m}\nabla_j^2 - \sum_{\alpha}^{N_n} \frac{\hbar^2}{2M_\alpha}\nabla_\alpha^2 \\ &- \sum_{j}^{N_e}\sum_{\alpha}^{N_n} \frac{Z_\alpha e^2}{|r_j - R_\alpha|} + \sum_{j<k}^{N_e} \frac{e^2}{|r_j - r_k|} + \sum_{\alpha<\beta}^{N_j} \frac{Z_\alpha Z_\beta e^2}{|R_\alpha - R_\beta|} \end{aligned} \tag{5.2}$$

> The symbols Z_α and M_α are the atomic number and mass of the αth nucleus, $\boldsymbol{R}_\alpha$ is the location of this nucleus, e and m are the electron charge and mass, $\boldsymbol{r}_j$ is the location of the jth electron, and $\hbar$ is Planck's constant. Less immediate things in the universe, such as the planet Jupiter, nuclear fission, the sun, or isotopic abundances of elements in space are not described by this equation, because important elements such as gravity and nuclear interactions are missing. But except for light, which is easily included, and possibly gravity, these missing parts are irrelevant to people- scale phenomena. ***Eqs.* 5.1 *and* 5.2 *are, for all practical purposes, the Theory of Everything for our everyday world.*** (our emphasis)

That Laughlin and Pines can speak of a ***partial*** "theory of everything" implies (almost *a priori*) that they are committed to a hierarchical view of the physical world with fairly sharp boundaries between levels. They believe that ***like*** the standard model, the amalgam of quantum chromodynamics and electroweak theory,—i.e., the quantum field theory describing the leptons, quarks, and gluons and their interactions,[13]—that electrons and nuclei and the non-relativistic

[12] Robert Laughlin won the Nobel laureate for work he did explaining the quantum Hall effect, and David Pines is one of the most distinguished solid-state theorists.

[13] Physicists believe that the standard model constitutes a "theory of everything" in the sense that it "explains" the existence of neutrons, protons and mesons, their properties and the interactions between them, which in turn account for the existence of nuclei, ... The quantum electrodynamics component of the electroweak theory in turn explains the electromagnetic interactions between electrons and nuclei and accounts for the existence of atoms and molecules, and the "weak" component explains the radioactivity of nuclei.

Schrödinger equation that describes their Coulomb interactions, i.e. Eqs. 5.1 and 5.2, play the analogous role of being a ***Theory of Everything*** for that part of the macroscopic world of immediate concern to us: the world of atoms, molecules and condensed matter.[14]

For a host of reasons—in particular, complexity and computability—one is able to calculate the properties of ***but a very limited number*** of the entities that can result from bound states of quarks and gluons starting from the standard model. The proof of the existence of some of the mesons and some of the baryons, and the calculation of some of their properties, seem to be the limit. With very insightful approximations—such as formulating quantum chromodynamics on a ***lattice***—one is able to compute properties of these entities with impressive accuracy. But computing the properties of the deuteron, or *a fortiori* the level structure of a boron or carbon nucleus, by an *ab initio* standard model calculation is not possible with any constructible computer.[15] The same is true of *ab initio* calculations of the properties of molecules composed or more than 10 or so atoms, or of crystalline solids, based on Laughlin and Pines' "Theory of Everything," i.e. Eqs. 5.1 and 5.2.

When the Born-Oppenheimer approximation[16] is made in Eqs. 5.1 and 5.2 the Hamiltonian for the electrons becomes

$$H_{el} = -\sum_{j}^{N_e} \frac{\hbar^2}{2m} \nabla_j^2 - \sum_{j}^{N_e} \sum_{\alpha}^{N_n} \frac{Z_\alpha e^2}{\left|\vec{r_j} - \vec{R_\alpha}\right|} + \sum_{j<k}^{N_e} \frac{e^2}{\left|\vec{r_j} - \vec{r_k}\right|} \tag{5.3}$$

But an exact solution of

$$H_{el}|\Psi_{el}\rangle = E_{el}(R_1, R_2, \ldots, R_N)\, |\Psi_{el}\rangle \tag{5.4}$$

(with the ($R_1, R_2, \ldots, R_{N_n}$ fixed) cannot be obtained for many reasons. The two-body Coulomb repulsion in H_{el} makes Eq. 5.3 a partial differential equation that cannot be separated. Obtaining an (approximate) solution on a computer faces the problem of requiring an amount of computer time that scales exponentially with the number of electrons. And even if one could obtain a very good ***approximate*** many-body wave function $\Psi_{el}(r_1, r_2, \ldots, r_N; R_1, R_2, \ldots, R_N)$ it would contain far more information than one could possibly handle as it would demand an exponential amount of computer memory to store. A 20-electron wave function on

[14] Note that Eq. 5.2 only takes into account the Coulomb interactions between the charges and neglects spin and magnetic interactions, as well as the possibility of emission and absorption of photons. Systems for which this is a valid approximate Hamiltonian to calculate their level structure are called Coulomb systems.

[15] See Kadanoff (2011) and the references therein.

[16] The Born-Oppenheimer *Anzatz* consists in approximating the wave function for the electron-nuclear system as a product of a wave function for the nuclei and a wave function for the electrons in which the nuclei are considered clamped at the positions $\boldsymbol{R}_1, \boldsymbol{R}_2, \ldots \boldsymbol{R}_{Nn}$ It is justified on the basis that $m << M_z$. See for example Combes et al. (1981).

a modest $10 \times 10 \times 10$ real-space grid would require a computer to store 10^{60} numbers.

Furthermore, macroscopic systems display properties, such as the behavior of their specific heat near second order phase transitions, which are relatively independent of their microscopic constituents (Kadanoff 2013). Laughlin and Pines have dubbed such properties "emergent" and have asserted that they are governed by higher organizing principles than those embodied in Eqs. 5.1 and 5.2. But it should be noted that Laughlin and Pines's Eq. 5.2 is a Schrödinger equation describing a ***fixed number***, $N = N_e + N_n$, of particles. Such an equation—for rigorous mathematical reasons—cannot account for such emergent properties. But dramatically new mathematical properties ***do emerge*** if N goes to infinity, if the volume, V, the system occupies becomes infinite, but N/V, the density of particles, goes to a finite limit. The description then becomes a ***field theoretical one***.

The emergent properties that Laughlin and Pines refer to are the result of the spontaneous breaking of particular symmetries of nature in the process of forming the gaseous, liquid, or solid crystalline phase.[17] Thus the translational symmetry of Eq. 5.1 is broken in the crystalline state of a solid. The question therefore becomes whether the particular symmetry that a particular solid composed of particular atoms assumes can be computed.[18] More generally, the issue becomes answering the question: To what extent can we reconstruct the world knowing the "foundational" effective theories? And what is the relation and connection between them.[19]

5.1.2 Properties and Objects

The success of non-relativistic quantum mechanics in explaining the structure of atoms and simple molecules is due to the fact that these entities can be considered—in their usual terrestrial context—to be composed of point-like electrons and of (essentially) immutable nuclei, "immutable" because the characteristic energy level separation between the ground state and first excited states in nuclei is of the order of Kevs and higher, as compared to Evs at the atomic level.[20] The characteristic energy of the environment that nuclei find themselves in the terrestrial context range from a fraction of an Ev to a few Evs and hence nuclei cannot undergo

[17] A great deal can be said about "emergent" properties if one knows what symmetry group is broken, and what subgroups are left as a symmetry—and all this can be stated irrespective of the dynamics involved.

[18] We shall make some comments regarding this question in Sect. 5.1.2.

[19] As is well known, this issue was addressed by Phillip Anderson in 1972 in *Science*.

[20] Furthermore, the fact that the size of nuclei is very small compared to atomic dimensions and that their mass is very large compared to that of an electron justifies the approximation that in atoms and molecules the nuclei interact with the electrons only through electrostatic Coulomb forces.

nuclear transformations.[21] Similarly, the number of electrons remains constant since electron-positron pair production requires an Mev of energy.

Quantum mechanics attributes a hierarchical order to the microscopic world by virtue of energy scales. Energy scales translate into momentum scales and the latter into distance scales by virtue of Heisenberg's uncertainty principle.[22] The quantum mechanical description of atoms in terms of electrons and nuclei given by Schrödinger Eq. 5.2 is a low resolution one: one is not to probe distances smaller than fractions of an ängstrom, 10^{-8} cm.

Already in the first, the 1930, edition of his *Principles of Quantum Mechanics*, Dirac noted that quantum mechanics attributes a hierarchical order to the physical world by virtue of Planck's constant, h, and the length scale it introduces: a characteristic atomic length is of the order h^2/me^2, where m is the mass of an electron and e its electronic charge, a distance of the order of an Ängstrom as compared to macroscopic distances—centimeters and meters.

Chemistry in the nineteenth century had introduced another important scale regarding the micro and macro realms: a macroscopic volume of liquid, solid or gaseous matter contains of the order of Avogadro's number of atoms, Avogadro's number, $N_A = 6 \times 10^{23}$, being the number of atoms in a mole of a pure substance. Reflecting this fact a new constant of nature—Boltzmann's constant $k_B = 1.38 \times 10^{-16}$ erg/K, was introduced into physics. Boltzmann's constant connects the atomism of the micro level with the macro realm through the equation $R/k_B = N_A$, where R is the universal gas constant 8.3×10^7 erg/K,[23] nd also through the equation $F/N = e$, wherein e is the charge of an electron. F, Faraday's constant, is the amount of charge necessary to deposit one mole of a monovalent substance in electrolysis.

The roots of the hierarchical mode of reasoning can be traced back to the basic aim of the atomic hypothesis: to describe one stratum of "reality" through a theory of entities occupying a "lower" one. In the second half of the nineteenth century Clausius, Maxwell, Gibbs, Boltzmann and others noted the differing character of the micro and macro representations: the continuum descriptions at the macro level in terms of a few local observables as compared to the Avogadro number of variables involved at the micro realm.[24] However, as they believed that classical mechanics and classical electromagnetism governed the dynamics of the atomistic level they came to realize that any formulation based on Newton's laws of motion that describes the dynamics of entities interacting through gravitational and

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[21] We are neglecting the effects of cosmic rays or the radioactivity of nuclei.

[22] Furthermore, systems whose characteristic time T, mass M, and length L, are such that $ML^2/T >> h$ are "macroscopic" and described by classical mechanics, those for which $ML^2/T \approx h$ are "microscopic" and described by quantum mechanics.

[23] This equation links the atomistic dynamics with thermodynamics through statistical mechanics in a derivation of the equation of state for a very dilute gas.

[24] Eg. the Navier-Stokes description of fluid flow in terms of a local velocity field as compared to the Newton's equations describing the motion of the individual molecules.

electromagnetic forces[25] cannot explain the stability of systems composed of these entities (Renn 2008).

Lorentz's 1906 Columbia lectures—the account of his researches that related the *macroscopic* Maxwell equations to a *microscopic* representation of the electromagnetic field and of charged particles,—gave these developments their modern formulation (Lorentz 1909). Certain properties of the charged entities—such as their mass, their charge, their charge distribution,—were assumed to be unchanging. These being unchanging and immutable their history could be described by the change in time of certain quantifiable ***properties*** attributed to them: their position and their momenta.

Fontana and Buss (1996) when attempting to formulate a computer language appropriate to describe chemical reactions succinctly and insightfully characterized the classical descriptions and their extensions:

> In a dynamical system, it is *not* the interacting entities that participate *as objects* in the formal constitution of "the system", but rather their quantitative properties and couplings. As a consequence, interaction is understood as the temporal or spatial change in the numerical value of variables. This change is captured by a set of (deterministic or stochastic) differential (or difference) equations. (Fontana and Buss 1996, 68)

What is new in quantum mechanics, and in particular in Schrödinger's wave mechanics, is that the interacting entities participate as ***objects***, whose structure, couplings and other attributes could change as a result of the interactions, and more particularly, that ***stable*** new *objects* could be formed.

Bohr's revolutionary papers of 1913 gave the initial insights on how the quantum theory could account for the stability of atoms. Central to his model was the recognition that the introduction of Planck's constant made possible the ***stipulation*** of non-radiating, stable bound states of an electron interacting ***electromagnetically*** with a proton, i.e. of the hydrogen atom (Bohr 1913; Heilbron and Kuhn 1969; Heilbron 1985; Kragh 2012).

With the advent of quantum mechanics, as important as had been the fully quantum mechanical calculations of Pauli and Schrödinger in obtaining the level structure of the hydrogen atom—a new stable object formed by the interaction of an electron with a proton, or that of Heisenberg in explaining the level structure of the (two electron) H_e atom, and the subsequent calculations to explain the periodic table—a further *crucial calculation* was that of Heitler and London (1927) that explained the formation of the H_2 molecule. By indicating how the charge density of the two electrons, when in a singlet spin state by being locatable between the two protons, lowered the energy by increasing the attractive forces between the electrons and the protons and simultaneously shielded the charge of the two protons thus reducing repulsive force between them, Heitler and London explained the stability of the H_2 molecule and formulated the quantum mechanical basis for the covalent bond.[26] The calculation gave a new quantitative perspective on bonding

[25] Or through more localized, specific atomic or molecular forces and are also coupled to the electromagnetic field.

[26] See Gavroglu and Simões (2012) for the details and subsequent developments.

and saturation. In addition, the directional characteristics of orbitals when electrons were not in s states were used to indicate how quantum mechanics could explain the bonding properties of the carbon atom so important to understand the structure of organic compounds. A morphic element was thus introduced into quantum mechanical explanations.[27]

The quantum mechanical modeling of the atomic and nuclear world has two further attributes that were recognized early and shaped the approach to understanding phenomena at the micro and macro levels:

1. A quantum description gives a measure of certainty to our knowledge of the world: it asserts that all hydrogen atoms in their ground state when isolated are identical; ibid for ^{23}Na atoms in their ground state. Similarly, that all lead $^{206}Pb_{82}$ nuclei in their ground state are identical[28]...
2. When computing the properties of atoms, molecules and solids the value of the parameters that enter into the Schrödinger equation describing the dynamics of the system and that characterize the electron and the nuclei—such as their mass, spin, magnetic moment, electric quadrupole moments—the values of these parameters are empirically determined. After the discovery of the neutron in 1932, and after models of nuclear structures had been advanced, these nuclear parameters were to be explained and their value quantitatively determined by the "lower level" theory that was to account for the structure and stability of nuclei, i.e., by a description of nuclear dynamics in terms of neutrons and protons and the nuclear forces by which they interact.[29]

During the 1930s, the quantum field theoretical demonstration that the electromagnetic interactions between charged particles could be explained as due to photon exchanges,[30] Fermi formulated of a field theory of β-decay and Yukawa suggested that in analogy to electromagnetic forces the short range nuclear forces between nucleons could be generated by the exchanges between them of a hitherto unobserved massive particle,[31] a novel conceptualization of physics began

[27] For a thorough account of these developments see Gavroglu (1995).

[28] Upon receiving the 1993 Orsted medal for his contributions to the teaching of physics, Bethe in his acceptance speech stated "that there is a certainty principle in quantum theory and that the certainty principle is far more important for the world and us than the uncertainty principle. That doesn't say that the uncertainty principle is wrong. It says that the uncertainty principle just tells you that the concepts of classical physics, position, and velocity, are not applicable to atomic structure." Bethe (1993).

[29] It did so first in terms of phenomenological inter-nucleonic potentials; thereafter in attempts to determine these potentials on the basis of meson theories; and more recently in terms of the standard model.

[30] See for example Fermi (1932). In the article Fermi stated that he considered the stability and other properties of the electron open questions due to the infinite self-energy problem in quantum electrodynamics. He thought the problem was to be resolved by a future theory. Subsequently, further infinities were encountered in QED. Divergences due to self energy and vacuum polarization effects plagued quantum field theories throughout the 1930s. These problems were partially resolved by the post World War II renormalization program.

[31] See Brown and Rechenberg (1996).

assuming an ever greater importance. It consisted in recognizing—at the level of accuracy of possible physical measurements and the corresponding theoretical representations—that the physical world could be considered to be *hierarchically ordered* into fairly well delineated realms and concerns: the macroscopic—consisting of solids, liquids, gases, their structure, and their properties; the molecular and atomic realm; the nuclear; and the sub-nuclear ones[32] and that the physical processes by which their connection is implemented could be given.

The atomic, nuclear, and subnuclear realms became describable by separate (foundational) ontologies and corresponding quantum dynamics. The ontologies refer to a given level—electrons and nuclei for the atomic and molecular realm, neutrons and protons for the nuclear level, with the latter's interactions at first described phenomenologically by nuclear potentials, and later assumed to be derivable from a quantum field theory of nucleons and mesons once mesons were included in the basic ontology. The entities that comprised the foundational ontology were considered the building blocks of the composite objects that populated that level.

The novel conceptualization of physics that emerged in the 1930s with quantum field theory bolstered the view that the aim of physics was to identify, classify and characterize the various realms, and to formulate and explain their interrelations. It was the task of the quantum field theoretical theories representing the lower levels to quantitatively account for the empirically determined parameters that described the "elementary" building blocks of the higher levels. This despite the fact that there was little confidence that quantum field theory was adequate to explain the nuclear domain in terms of subnuclear constituents.

The microscopic and sub-microscopic levels became considered more "fundamental" since it was believed that one could reconstruct the higher levels in terms of the knowledge of the properties and dynamics of the entities that populated the lower levels. The success of quantum mechanics in explaining the properties of atoms and molecules in terms of electrons and nuclei, that of simple molecules in terms of their atomic constituents, and of nuclear theory in accounting for some of the properties of nuclei—such as their masses and magnetic moments—in terms of the interaction potentials between nucleons as determined from neutron and proton scattering experiments, reinforced a commitment to reductionism.

Non-relativistic and relativistic quantum field theories (NRQFT and RQFT) address different questions. Non-relativistic QFTs were (are) concerned with idealized systems of infinite number of interacting particles whose vacua are

[32] One might add to these the cosmological—consisting of galaxies and their constituents, their evolution and dynamics. These hierarchies were not considered to be independent: accurate measurements of atomic energy levels reveal nuclear and subnuclear properties. Similarly, the recent startling discovery of the necessity of the presence of cold dark matter—consisting of as yet undiscovered subnuclear entities—in order to make sense of new cosmological observational data is proof of the linkage between the various levels. But it must also be noted that these observations have not destabilized our amazingly accurate representations of the atomic world. And needless to say, the linkage of the levels is made explicit as soon as one tries to answer evolutionary questions.

degenerate, where the limit $N \to \infty$, $V \to \infty$ with N/V finite in the limits is of crucial importance. In this idealized version spontaneously broken symmetry is a general feature of the nonrelativistic quantum theory of the structure of matter. Galilean invariance is always broken if N/V is finite because a state describing the system at rest is infinitely different from the state describing the system moving with constant velocity. Similarly, crystallization breaks translation invariance, and spontaneous magnetization breaks rotational symmetry.

The differences in quantum field theoretical methods in the non-relativistic and relativistic domains are evident in their usage. Until the 1970s the application of QFT in non-relativistic many-body physics reflected the ease with which the Bose or Fermi-Dirac character of the particles could be handled. It also allowed novel approximation methods to be introduced and to visualize perturbative calculations in terms of Feynman diagrams.[33] And in the statistical mechanical applications of NRQFT, use was made of the grand canonical partition function, allowing the consideration of open systems with varying number of particles.

On the other hand, RQFTs's aim is to explain the results of ever higher energy collision processes probing the structure of objects at ever smaller distances (Haag 2000). The states describing this situation differ from the vacuum state only in some finite space regions at a given time (Haag 1996). In this domain, the *S*-matrix played an ever greater role—but as Heisenberg emphasized the *S*-matrix is the roof of the theory not its foundation (Haag 1997). RQFTs came to be seen as the means to construct *S* matrices that respect Lorentz invariance and the cluster decomposition requirement, namely, that local processes be insensitive to processes occurring in the distant environment (Weinberg 1995; Duncan 2013).

A dramatic change occurred in the early 1970s with the works of Leo Kadanoff, Michael Fisher and Kenneth Wilson which solved the phase transition problem and introduced the renormalization group methods. Concurrently, the introduction of functional integral representations of quantum field theories transformed the application of field theoretical methods in many body physics. Functional integral representations and renormalization group methods likewise transformed relativistic quantum field. We refer the reader to the recent textbooks by Duncan (2013) and by Altland and Simons (2010) for a very perceptive and highly informative account of these developments and insights into the ways by which the two fields fructify each other.

There is no question that QFT is one of the most remarkable theories yet devised. The range of its applicability—some 20 orders of magnitude—is staggering. But its inability to incorporate general relativity also indicates its limitations: undoubtedly new concepts of space and time, objects, interactions, will have to be introduced to describe processes at Planck scale lengths. Just as there is a limit to the QFT representation when the assumption that space-time is a stage that is unaffected

[33] See for example Mattuck (1967) and Altland and Simon (2010).

by the processes occurring in it is no longer valid—and therefore that new concepts will have to be framed—there may be limits with the non-relativistic description at the mesoscopic scale.

5.1.3 Thomas-Fermi, Hartree-Fock and Density Functional Methods

The Thomas-Fermi (TF) model was formulated independently by Thomas and by Fermi in 1927 to calculate the properties of a neutral atom with Z electrons and a nucleus of charge Z.[34]

Instead of a many-electron wave function, which depends on $3Z$ electronic space coordinates, the basic quantity in Thomas-Fermi theory is the electron density $n(r)$, which depends on only three space coordinates. This represents a huge simplification for calculating, understanding and predicting the structure of atoms.[35] The reason for its extensive use is that it incorporates in a simple fashion both the uncertainty principle and the Pauli exclusion principle, and is computationally manageable in the case of Coulomb forces.[36]

The basic approximation of TF can easily be stated for the case $Z = 1$, in which case $n(r) = |\psi(r)|^2$ (when $\psi(r)$ is normalized) and a sharpened version of the uncertainty principle, the Sobolev inequality, then states that

$$\int |\nabla \psi(r)|^2 d^3 r \;\; \geq \;\; K' \int n^{5/3}(r)\, d^3 r \tag{5.5}$$

with K' a constant factor.[37] It is this approximation that is used in obtaining a bound on the kinetic energy of the electron. In the case of N electrons a similar bound for the expectation value of their kinetic energy can be given in terms of the one particle density n(r) now defined as

[34] Our presentation is based on Kohn's Nobel lecture (Kohn 1999).

[35] In fact, it yields an exact result for the energy of an atom in the limit $Z \to \infty$. This was proved by Lieb and Simon (1977). Elliott Lieb and Walter Thirring are responsible for extensive and generative contributions to the rigorous mathematical proofs of the quantum mechanical accounts of the stability of atoms, molecules and solids. See Thirring (2001).

[36] More generally when 1/r potentials are involved TF theory can be made to yield important results using analytical methods. Thus TF methods can also make statements about the stability of systems where gravitational forces are also involved. See the informative and insightful article by Spruch (1991) and the references therein to the papers by Lieb and by Thirring. See also Lieb (1981) and Thirring (1981).

[37] See Lieb (1981), Spruch (1991).

$$n(r) \;=\; N\int d^3r_2 \int d^3r_3 \ldots \int d^3r_N |\psi(r, r_2, r_3, \ldots .. r_N)|^2 \tag{5.6}$$

The modern version of TF theory is formulated in terms of an energy functional.[38] For the case of N_e electrons interacting with N_n (fixed) nuclei located at $\boldsymbol{R}_1, \boldsymbol{R}_2, \ldots,$ $\boldsymbol{R}_{Nn}$, the energy functional is given by:

$$E\left[n(r), R_1, R_2, \ldots, R_N\right] = \frac{3}{10}\gamma \int n(r)^{5/3} d^3r - \int n(r) \sum_{\alpha}^{N_n} \frac{Z_\alpha e^2}{|r - R_\alpha|} d^3r + \frac{e^2}{2} \int \frac{n(r)\, n(r')}{|r - r'|} d^3r\, d^3r' + \sum_{1 \le \alpha \prec \beta \le N_n}^{N_j} \frac{Z_\alpha Z_\beta e^2}{|R_\alpha - R_\beta|} \tag{5.7}$$

where the first term represents the TF approximation to the kinetic energy of the electrons,[39] the second the Coulomb attraction of the electrons to the nuclei, the third the Coulomb repulsion of the electrons with one another, and the fourth the Coulomb repulsion of the nuclei with one another.

In Eq. 5.7 γ is an arbitrary positive constant but to establish contact with quantum theory

$$\gamma = \left(3\pi^2\right)^{2/3} \tag{5.8}$$

This value of the constant γ is obtained from the formula for the density of a uniform, non-interacting, degenerate electron gas in a constant external potential.

Since $n(r)$ is supposed to be the electron density it is required that $n(r) \ge 0$ and that

$$\int n(r)\, d^3r = N_e \tag{5.9}$$

The energy functional Eq. 5.7 is to be an extremum with respect to variations of $n(r)$. The variational derivative of $E[n(r)]$ is

$$\frac{\delta E\left[n(r)\right]}{\delta n(r)} = \gamma\, n^{2/3}(r) - \sum_{\alpha}^{N_n} \frac{Z_\alpha e^2}{|r - R_\alpha|} + e^2 \int \frac{n(r')}{||r - r'|} d^3r' \tag{5.10}$$

[38] See Lieb (1981).

[39] This approximation is not good enough to explain molecular binding using TF. See Teller (1962); also Lieb (1981).

Since a Lagrangian multiplier must be added to $\frac{\delta E\,[n(r)]}{\delta n(r)}$ to insure that Eq. 5.9 is satisfied, the minimalization of $E[n(r)]$ yields

$$\gamma\, n^{2/3}(r) = \sum_{\alpha}^{N_n} \frac{Z_\alpha e^2}{|r - R_\alpha|} - e^2 \int \frac{n(r')}{||r - r'|} d^3 r' - \mu \tag{5.11}$$

which is the Thomas-Fermi equation.[40]

The energy functional Eq. 5.7 can be interpreted as stating that the electrons move independently in an external potential v(r) given by the two Coulomb terms on the right hand side of Eq. 5.11. The Thomas-Fermi equation thus determines the relation between v(r) and n(r).

In the late 1960s Dyson and Lenard gave a proof of the stability of matter composed of (fixed) nuclei and electrons interacting through Coulomb forces (Dyson and Lenard 1967; Dyson 1968). They proved that the ground state energy, $E^{(N)}$, of such a system was proportional to the number, N, of electrons ***provided*** that the Fermi character of the electrons is rigorously taken into account.[41] The stability depends essentially on the fact that electrons obey the Pauli principle. Their proof was greatly simplified by Lieb and Thirring. Inspired by the Thomas-Fermi model of the atom, they derived an upper bound for the kinetic energy of the electrons when evaluated in an approximate ground state electronic wave function, expressed in terms of the electronic charge density $n(r)$.[42] Shortly thereafter, Lieb and Lebowitz (1972) proved the existence of the thermodynamic functions for Coulomb matter in the limit as the number of electrons $N \rightarrow \infty$, and thus how properties—valid for all systems—emerge from this idealization. For Coulomb systems, the thermodynamic limit does not follow from the proof of stability that $E_N \geq (constant)\ N$ because one has to demonstrate that the long range Coulomb force is sufficiently screened so that separated portions of matter are sufficiently isolated (Spruch 1991; Brydges and Federbush 1981; Fröhlich and Spencer 1981).

At roughly this same time, Walter Kohn became interested in the Thomas-Fermi description of atoms because his researches on disordered metallic alloys had led him to consider descriptions in terms of the electron density distribution *n(r)*.[43] A host of empirical results[44] made it clear to him that TF theory yielded a fairly good approximate representation of the charge density that would be computed from the N electron wave function, $\psi(r_1, r_2, \ldots, r_N)$. This was the ground state solution of the N electron Schrödinger equation. He posed himself the challenge to establish

[40] For further details see Lieb 1981, 213–208; Bethe and Jackiw (1968); Lieb and Seiringer (2010).

[41] More precisely, they proved that $E^{(N)} \geq -AN$, with A a finite, N-independent constant.

[42] By doing so they could link the stability of matter with the mathematics of functional analysis. See Spruch (1991), Lieb and Seiringer (2010).

[43] See Kohn's Nobel lecture (Kohn 1999).

[44] For example, the approximate agreement between X-ray scattering by neutral atoms and cross-sections calculated using TF density distributions.

the exact connection between *n(r)* and the ground state wave function $\psi(r_1, r_2, \ldots, r_N)$ that obeyed the Schrödinger equation. This in turn led him to ask the more general questions: "Is a *complete, exact* description of ground electronic structure in terms of *n(r)* possible in principle [and] whether the density *n(r)* completely characterized the system?" (Kohn 1999). The answers to both these questions were provided by Hohenberg and Kohn (HK) and turned out to be yes. They were the starting point of modern density functional theory (DFT) (Hohenberg and Kohn 1964; Kohn and Sham 1965; Kohn 1966a, b).

The basic lemma of HK states that the ground state density n(r) of a bound system of interacting electrons in some external potential v(r) determines this potential uniquely (up to an additive constant). And since $\int n(r)\, d^3r$ determines the number of electrons N_e, the knowledge of $n(r)$ yields both H and N_e for the electronic system, and therefore all the properties derivable from H through the solution of the Schrödinger equation.

To obtain the energy E of the ground state of the system described by H KH invoked the Rayleigh-Ritz minimum principle, namely, that

$$
\begin{gathered}
E = \min_{\widetilde{\Psi}} \left(\widetilde{\Psi}, H\widetilde{\Psi}\right) \\
H = T + U
\end{gathered}
\tag{5.12}
$$

with T the kinetic energy operator of the electrons, U their interaction energy operator and $\widetilde{\Psi}$ a normalized, trial function so designed as to be an approximate solution of $H\widetilde{\Psi} = E\widetilde{\Psi}$.

To transcribe the minimalization procedure $\min_{\widetilde{\Psi}} \left(\widetilde{\Psi}, H\widetilde{\Psi}\right)$ in Eq. 5.7 in terms of trial densities one assumes carrying it out in two stages. One first fixes a trial $\tilde{n}(r)$. One then considers the class of trial functions $\widetilde{\Psi}_{\tilde{n}}{}^{\alpha}$ with this $\tilde{n}$ The *constrained* energy minimum with $\tilde{n}(r)$ fixed is defined as

$$
\begin{aligned}
E_v\left[\widetilde{n}(r)\right] &= \min_\alpha \left(\widetilde{\Psi}_{\tilde{n}}{}^{\alpha}, H\widetilde{\Psi}_{\tilde{n}}{}^{\alpha}\right) \\
&= \int v(r)\,\widetilde{n}(r)\, d^3r + F\left[\widetilde{n}(r)\right]
\end{aligned}
\tag{5.13}
$$

with

$$
F\left[\widetilde{n}(r)\right] = \min_\alpha \left(\widetilde{\Psi}_{\tilde{n}}{}^{\alpha}, (T+U)\widetilde{\Psi}_{\tilde{n}}{}^{\alpha}\right) \tag{5.14}
$$

Note that $F\left[\widetilde{n}(r)\right]$ does not require an explicit knowledge of v(r); it is a universal functional of the density $\widetilde{n}(r)$.

In the second step one minimizes $F\left[\widetilde{n}\left(r\right)\right]$ over all $\widetilde{n}\left(r\right)$,

$$\begin{aligned} E &= \min_{\widetilde{n}\,(r)} E_v\left[\widetilde{n}\left(r\right)\right] \\ &= \min_{\widetilde{n}\,(r)}\left\{\int v(r)\,\widetilde{n}\left(r\right)d^3r + F\left[\widetilde{n}\left(r\right)\right]\right\} \end{aligned} \tag{5.15}$$

The problem is that the functional dependence of $F\left[\widetilde{n}\left(r\right)\right]$ on $\widetilde{n}\left(r\right)$ is not known explicitly.

The Thomas-Fermi theory is recovered by making the following approximation in $F\left[\widetilde{n}\left(r\right)\right]$

$$\begin{aligned} T\left[\widetilde{n}\left(r\right)\right] &= \frac{3}{10}\gamma\int \widetilde{n}\left(r\right)^{5/3} d^3r \\ U\left[\widetilde{n}\left(r\right)\right] &= -\int \widetilde{n}\left(r\right)\sum_{\alpha}^{N_n}\frac{Z_\alpha e^2}{\left|r-R_\alpha\right|}\,d^3r + \frac{e^2}{2}\int\frac{\widetilde{n}\left(r\right)\widetilde{n}\left(r'\right)}{\left|r-r'\right|}d^3r\,d^3r' \\ &\quad + \sum_{1\le\alpha\prec\beta\le N_n}^{N_j}\frac{Z_\alpha Z_\beta e^2}{\left|R_\alpha-R_\beta\right|} \end{aligned} \tag{5.16}$$

Further approximations to the TF theory—such as the Dirac exchange term—can readily be incorporated into Eq. 5.16.

Kohn and Sham (KS) in their approach emulated Hartree in his method of approximating an N body wave function. It will be recalled that for the case of a neutral atom with a nucleus of charge Z, Hartree assumed that every electron moved in an effective one particle potential

$$v_H\left(r\right) = -\frac{Ze}{r} + e^2\int\frac{n(r')}{\left|r-r'\right|}d^3r' \tag{5.17}$$

the first term being the potential of the nucleus and the second that due to the average electron density distribution $n(r)$, so that each electron obeys the single particle Schrödinger equation

$$\left[-\frac{\hbar^2}{2m^2}\nabla^2 + v_H(r)\right]\varphi_j(r) = \varepsilon_j\,\varphi_j(r) \tag{5.18}$$

The mean density $n(r)$ is then

$$n\left(r\right) = \sum_j^Z\left|\varphi_j(r)\right|^2 \tag{5.19}$$

In order to satisfy the Pauli exclusion principle the sum over j runs over the Z ***lowest*** eigenvalues[45]: each state is then occupied by a single electron. To solve Eqs. 5.18 and 5.19 one starts with an approximate $n(r)$, usually taken to be the one given by FT, and solves for the φ_js. From these one obtains a new $n(r)$, from which a new effective potential according to Eq. 5.15 is obtained, and the new φ_js that result from Eq. 5.18 are calculated. The process is iterated until the old $n(r)$ and the new $n(r)$ agree.[46]

The Schrödinger Eq. 5.18 for the electrons in the Hartree approximation has them move ***independently*** of one another in an "effective" external field, v_H. This feature was the point of departure for Kohn and Sham. They investigated the DFT for a system consisting of N ***non-interacting*** electrons moving in a (given) external potential $v($r). The energy functional for such a system takes the form

$$E_{v(r)}\left[\widetilde{n}\left(r\right)\right] = \int v(r)\,\widetilde{n}\left(r\right)d^3r + T_s\left[\widetilde{n}\left(r\right)\right] \tag{5.20}$$

where $T_s\left[\widetilde{n}\left(r\right)\right]$ ***is the kinetic energy of non-interacting electrons in their ground state with density*** $\tilde{n}(r)$**.** The minimization of this energy functional, subject to the constraint that the total number of electrons is fixed, requires that

$$\delta E_{v(r)}\left[\widetilde{n}\left(r\right)\right] = \int \delta\widetilde{n}\left(r\right)\left[v(r) + \frac{\delta}{\delta\widetilde{n}\left(r\right)}\,T_s\left[\widetilde{n}\left(r\right)\right]|_{\tilde{n}=n} - \varepsilon\right]d^3r = 0 \tag{5.21}$$

Now for this case the ground state energy, E, and the ground state density, $n(r)$, can be obtained by solving for the eigenfunctions and eigenvalues of the following one particle Schrödinger equation

$$\left[-\frac{\hbar^2}{2m^2}\nabla^2 + v(r)\right]\varphi_j(r) = \varepsilon_j\,\varphi_j(r) \tag{5.22}$$

and $E = \sum_j^N \varepsilon_j \quad ; \quad n(r) = \sum_j^N \left|\varphi_j(r)\right|^2.$

For the case of electrons interacting through Coulomb forces Kohn and Sham stipulated the energy functional to be

$$E_v\left[\widetilde{n}\left(r\right)\right] = \int v(r)\,\widetilde{n}\left(r\right)d^3r + T_s\left[\widetilde{n}\left(r\right)\right] + \frac{e^2}{2}\int\frac{\widetilde{n}\left(r\right)\widetilde{n}\left(r'\right)}{||r-r'|}d^3r\,d^3r' + E_{xc}\left[\widetilde{n}\left(r\right)\right] \tag{5.23}$$

[45] j includes the spin quantum number.

[46] Instead of satisfying the Pauli principle by imposing Eq. 5.14, Fock took a determinant made up of one particle functions to approximate the Z electron Ψ function, The equation corresponding to Eq.n contains an exchange term and is known as the Hartree-Fock equation. See Bethe and Jackiw (1968).

with (the unknown functional) $E_{xc}\left[\widetilde{n}\left(r\right)\right]$ defined by this equation. All many-particle effects are contained in $E_{xc}\left[\widetilde{n}\left(r\right)\right]$; in particular, the many-particle contribution to the kinetic energy and the effects due to the Pauli exclusion principle.

The Euler-Lagrange equation which determines the extremum of Eq. 5.23, subject to the particle number constraint, is

$$\delta E_v\left[\widetilde{n}\left(r\right)\right] = \int \delta\widetilde{n}\left(r\right)\left[v_{eff}(r) + \frac{\delta}{\delta\widetilde{n}\left(r\right)}\, T_s\left[\widetilde{n}\left(r\right)\right]|_{\tilde{n}=n} - \varepsilon\right]d^3r = 0 \tag{5.24}$$

where

$$v_{eff}\left(r\right) = -v(r) + e^2\int\frac{n(r')}{|r-r'|}d^3r' + \frac{\delta}{\delta\widetilde{n}\left(r\right)}\,E_{xs}\left[\widetilde{n}\left(r\right)\right]|_{\tilde{n}\,(r)=\,n(r)} \tag{5.25}$$

Although it is very unlikely that the exact $E_{xc}\left[\widetilde{n}\left(r\right)\right]$ functional will ever be obtained since it embodies all the difficulties of the many-particle problem, many functionals have been developed to approximate $E_{xc}\left[\widetilde{n}\left(r\right)\right]$.[47] In this connection it is important to re-emphasize that DFT is an *ab-initio* approach. It is not characterized by the absence of any approximations, but by the fact that these approximations do not introduce adjustable physical parameters in the formulation of the theory. And the same is true for any refinement of the non-relativistic theory as expounded by HK and KS, e.g., when introducing relativistic or radiative corrections.[48]

It has not yet proven feasible to obtain the exact expression for the single-particle kinetic energy $T_s\left[\widetilde{n}\left(r\right)\right]$ as a functional of the density. This is the reason for Kohn and Sham not using the total energy expression directly in the variational procedure, but instead developing an alternative approach.

The form of Eq. 5.24 is the same as that of the Hartree Eq. 5.18 for non-interacting particles moving in the effective potential $v_{\text{eff}}(r)$ instead of $v_H(r)$. Kohn and Sham therefore inferred that the minimizing density is given by solving the one particle Schrödinger equation

$$\left[-\frac{\hbar^2}{2m^2}\nabla^2 + v_{eff}\left(r\right)\right]\varphi_j(r) = \varepsilon_j\,\varphi_j(r) \tag{5.26}$$

with the density distribution determined by $n(r) = \sum_j^N \left|\varphi_j(r)\right|^2$ and the ground state energy given by

[47] See all the articles in Nalewajski (1996). For a more current assessment see Engel and Dreizler (2011, Chap. 4, 109–217). They also give the formulation of KS that emulates Hartree-Fock.

[48] As can be seen in Engel and Dreizler's comprehensive account.

$$E = \sum_{j}^{N} \varepsilon_j - \int v_{xc}(r)\, n(r)\, d^3r - \frac{e^2}{2} \int \frac{n(r)\, n(r')}{\left|\vec{r} - \vec{r'}\right|} d^3r\, d^3r' + E_{xc}\left[n(r)\right] \tag{5.27}$$

As Kohn emphasized in his Nobel lecture, the practical usefulness of ground state DFT depends entirely on a positive answer to the question: "Can approximations for the functional $E_{xc}\left[\widetilde{n}(r)\right]$ be found, which are at the same time sufficiently simple and sufficiently accurate?" The answer to the question is yes by making use of empirically determined *n*(*r*), by using results from approximate Hartree-Fock calculations emulating the system, and by developing other approximating schemes, such as the local density approximation, already suggested in Hohenberg and Kohn and in Kohn and Sham (Engel and Dreizler 2011).

5.2 Some Concluding Remarks

In their introduction to *Neither Physics nor Chemistry*, Gavroglu and Simões commented that shortly after the papers of Heitler, London and Hund had laid the foundations of quantum chemistry "Dirac made a haunting observation: that quantum mechanics provided all that was necessary to explain problems in chemistry, but at a cost. The calculations involved were so cumbersome as to negate the optimism of the pronouncement." *Neither Physics nor Chemistry* chronicles the fact that "until the use of digital computers in the 1970s, the history of quantum chemistry is a history of the attempts to devise strategies of how to overcome the almost self-negating enterprise of using quantum mechanics for explaining chemical phenomena." But, as Gavroglu and Simões emphasized, "the progressively extensive use of computers brought about dramatic changes in quantum chemistry" and metamorphosed the field. "'*Ab initio* calculations', a phrase synonymous with impossibility, became a perfectly respectable prospect." (Gavroglu and Simões 2012, ix).

Indeed, by the 1970s the use of computers was so widespread, their speed and memories so great, and what programming made possible so broad in scope and extensive in applications, that computer science became a separate discipline. Being able to assess the efficiency of programs and the power and limits of computing systems had become necessary. A large number of departments of computer science offering graduate and undergraduate degrees in computer science became established in the early 1970s. Perhaps more indicative of the revolutionary character of computing and computers is the fact that mathematics department began appointing professors of computational mathematics.[49]

[49] The proof of the four-color map problem using computers was undoubtedly one of the factors responsible for the change.

Understanding and confronting the limits of computation, just as understanding and confronting the limits of the "foundational" theories of physics, became a distinctive feature of physics after the 1980s. We noted that *ab initio* calculations based on Schrödinger many-body theory could only be carried out with any accuracy for molecules with fewer that 20 components. DFT changed that. And it therefore also changed the views regarding how much of chemistry could be derived from quantum physics. In order to assess its impact, it is helpful in this connection to recall Dirac's "pronouncement" in its original form:

> The underlying physical laws [i.e. the general theory of quantum mechanics] necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known, and the difficulty is only that the exact application of these laws leads to equations much too complicated to be soluble.[50]

Expressing the laws of quantum mechanics applicable to the domain of non-relativistic electrons and nuclei in the language of DFT and of its extension, time dependent DFT (TDDFT),[51] a language that focuses on the one-particle density distribution and its generalization (Kohn 1999), allows the computation of the structure and properties of fairly large molecules. Using DFT and advanced computer codes, it has even been possible to predict the crystalline structure of some solids when given their atomic constitution (Glass et al. 2006; Wentzcovitch and Stixrude 2010).

Based on rigorously proven theorems, DFT has reformulated many-electron quantum mechanics making the observable one-particle electron density distribution the ***basic variable*** of interest, rather than the unobservable and inaccessible many-electron wave function. And because DFT has been made accurate and efficient, it has become the method most used in computational physics and chemistry.[52] Thus the number of DFT publications per year has increased nearly exponentially during the last two decades, reaching over 10,000 during 2010.[53] Similarly, over 50 books have been published that deal with DF theory since the appearance of Parr and Yang in (1989) and Engler and Dreizler's comprehensive account of DFT in 2011, which lists close to 800 references!

Because DFT is so widely applicable, what has happened is that DFT, TDDFT, new powerful computers and new powerful computing codes have transformed parts of condensed matter physics, parts of quantum chemistry,[54] as well as material

[50] Dirac (1929). Quoted in Gavroglu and Simões (2012, 9).

[51] See for example Marques et al. (2012), and especially Tempel (2012).

[52] See for example Wills et al. (2010).

[53] See Chap. 1 of Wills et al. (2010).

[54] Quantum chemists can now calculate many properties—such as structure, binding energies, vibrational frequencies, . . .—for systems containing hundreds of atoms and more. Similarly activation energies of chemical reactions involving fairly complex molecules can now be computed.

sciences[55] and made an amalgam of them. Practitioners in these fields can readily be accommodated in either physics or chemistry departments and for some of them in computer science departments. Many of the articles in the subject that were published in what were traditionally called physics journals could have been published in "chemical" journals, and conversely—or in journals devoted to computational physics and chemistry as many such journals devoted to various facets of the enterprise have been created. In DFT—and in other similar type of theory, e.g. lattice gauge theory—the effective nature of the description applies to computability as well as to experimental relevance. DFT *became* a meaningful entity only with the advent of powerful computers.

It should be clear that DFT lends further credence to our characterization of the quantum revolution as an HT scientific revolution. In its genesis, quantum mechanics amalgamated parts of mathematics with parts of physics and parts of chemistry. It did so by carving the physical world into fairly well delineated domains. More recently, computers and computations have become an integral part of a further amalgamation process, and have given new tools ***and new languages*** for expressing "effective quantum field theories".[56] What has happened in quantum chemistry is a manifestation of this process.

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[55] As noted, the most stable crystal structure for a particular combination of atoms can now be calculated and determined even when the structure is not known experimentally. See note 77.

[56] See Weinberg (1995–2000), in particular volume 2, *Applications*.

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Part II
STEP Matters

Chapter 6
Centers and Peripheries Revisited: STEP and the Mainstream Historiography of Science

Agustí Nieto-Galan

Abstract This chapter describes the history of the international research group "Science and Technology in the European Periphery" (STEP). It analyses STEP's genuine academic culture, its complex relation with the mainstream historiography of science and the crucial role that Professor Kostas Gavroglu has played in the making of the whole project, from its foundation in Barcelona in 1999 to its further intellectual, academic growth. It also describes in detail STEP's main achievements through conferences and publications on subjects such as scientific travels, scientific textbooks and their circulation, national historiographies of science, science popularization in the periphery, scientific controversies, with the most recent meetings covering different topics organized into thematic sessions. Taking "centers" and "peripheries" as flexible and dynamic categories, the STEP research agenda has enriched the study of circulation of knowledge in the past and shall also contribute to a new multicultural approach to a truly European history of science in the future.

Keywords Centers • Peripheries • STEP • Circulation of knowledge • Appropriation • Academic hegemony • History of science

6.1 Introduction

On September 22, 1997, on a sunny, beautiful day in the gardens of the European Cultural Centre of Delphi (Greece), the coffee break was particularly enjoyable and upbeat. It was the last session of the final European Science Foundation (ESF) Conference on "The Evolution of Chemistry in Europe, 1789–1939"—a 4-year project that had brought together historians from different European countries to work on several fruitful topics: the reception of the new French

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chemical nomenclature in the late eighteenth century, the circulation of chemistry textbooks throughout the nineteenth and twentieth centuries, the professionalization of chemistry in the nineteenth century and the making of the modern chemical industry and its technological networks across the continent. For that purpose, academic expertise from the "center" (mainly France, Germany, the UK and the US) was put to work in collaboration with "peripheral" groups (mainly from southern Europe, but also integrating the Scandinavian countries and some colleagues from the East).

Although the scholarship and academic traditions were varied and sometimes uneven, the 4-year project bore fruit with the publication of a series of important collective volumes.[1] But more than that, it managed to build new personal and academic ties that have lasted for decades and have probably contributed to the making of a more "European" history of chemistry and history of science in general. In fact, that late summer 1997 still belonged to a time in which the dream of the political, social and cultural construction of Europe—beyond crude business and explicit economic interests—was still on the agenda. Those were happy times and friends and colleagues had lively discussions during that coffee break on the future avenues of historical research, as well as on local constraints, potential comparative issues and collaborative enterprises that could contribute to the continuity of the project.

Kostas Gavroglu had helped with the local organization in Delphi—actually we were all picked up in the center of Athens and brought to Delphi by quite a long coach journey along narrow roads under the tacit "oversight" of the gods of Olympus. Two years earlier in 1995, under the scientific leadership of David Knight and Helge Kragh, Kostas had already organized one of the workshops of the ESF project in Delphi, in that case, on the making of the chemist as a profession in Europe in the nineteenth to twentieth centuries. Some months earlier, I met Kostas for the first time in London at 21 Albemarle Street at the entrance to the Royal Institution. Not much later, I soon noticed how, from Mayfair's urban cosmopolitism to Delphi's delightful landscape of olive trees, Kostas seemed able to masterfully synthesize the best of both worlds.

In that coffee break at the final ESF conference in 1997, we were chatting once again on the troubles and challenges of "peripheral" historians of science who have neither Faraday's, Darwin's or Newton's stories to tell. We avidly discussed the need to develop a genuine, original historiographic framework to properly place supposedly "marginal, obscure and provincial" case studies into mainstream international historiography. We aimed to enrich traditional research subjects and priorities with new questions, new languages and new cultures, in a word, to try to give a voice to scholars who, for linguistic, cultural and even political reasons, faced serious difficulties in being heard in mainstream European historiographic

[1] The main volumes worth mentioning are Bensaude-Vincent and Abbri (1995), Knight and Kragh (1998), Fox and Nieto-Galan (1999), Bensaude-Vincent and Lundgren (2000).

forums dominated by Anglo-American scholarship. This was probably the seminal *omphalos* of STEP.[2]

After Delphi, further informal discussions gradually gave birth to the idea of gathering a small group of colleagues to found a new research group that would be dedicated in particular to reflecting "peripherally" on the history of science and technology. It took some time until we met for the first time in Barcelona in 1999. After some discussion, the new group was finally named "Science and Technology in the European Periphery" (STEP). The core of founding members included: Ana Simões, Maria Paula Diogo, Ana Carneiro, Marco Beretta, Anders Lundgren, Arne Hessenbruch, Anders Lundgren, Berna Kilinc, José Ramón Bertomeu-Sánchez, Antonio García-Belmar, Agustí Nieto-Galan, Manolis Patiniotis and Kostas Gavroglu. After producing a very short, programmatic text and opening a very simple webpage (http://www.uoa.gr/step) and an e-mail list (nodus@uv.es), the group began to work on a small scale with the original purpose of organizing thematic workshops on subjects that could potentially help to analyze "center-periphery" problems in the broader sense. Excerpts of the 1999 founding document are worth quoting here:

> The history of the transmission of the new scientific ideas from the "centre" to the "periphery", especially during the last five centuries, is a subject which deserves further investigation. Europe is going through profound transformations and these changes create a new context for (re) examining a host of issues associated with the transmission of the sciences. Recently, new nation states have come into being, new borders emerged, new institutions appeared, and old institutions restructured themselves. These changes will induce many scholars to look again at the past, and science in Europe will be among the subjects to be systematically examined. The work that has already been done, as well as the newly available sources, combined with a more open intellectual environment and increases in funding for trans-national and trans-cultural contacts might offer an unprecedented opportunity for a critical re-examination of the historical character of science and its institutions in regions and societies in Europe for which there has been little or no work at all. How should we try to study the long-standing question of the tension between particular local practices and the trends of the progressive homogenization of an international scientific community? How has this tension been particularised in the framework of a Europe aiming to dictate global policies, while at the same time was facing the shifting of boundaries among nations and cultures? And, in addition, how should we deal with the old problem of the transfer of scientific knowledge, in a historiographical context offering a great variety of approaches?".[3]

After that formal and probably too optimistic declaration, the main research aims of the group were stated in detail through six main points:

1. Reconsidering the "centre-periphery" model which has been the dominant mode of dealing with the studies on the transfer of scientific knowledge;
2. Bringing to the fore the concept of scientific appropriation and attempting to study various local discourses;

[2] In 1994, the rationale of STEP had already been outlined and developed by Kostas Gavroglu in the European project "Prometheus": European Community Project, Human Capital and Mobility, Scientific and Technical Cooperation Networks *Project Prometheus—The Spreading of the Scientific Revolution from the countries where it originated to the countries in the Periphery of Europe, during the seventeenth, eighteenth, and nineteenth centuries*, CHRX-CT93-0299, 1994–1996. For more details, see Ana Simões's chapter in this volume.

[3] STEP, first Meeting. Barcelona 1999. Unpublished manuscript.

3. Examining systematically the relationship between science, politics and the rhetoric of modernization in societies at the European periphery;
4. Joining forces to find out more about scientific travels;
5. Using networks to further our understanding of the dynamics of the various scholars from the societies in the periphery of Europe;
6. Intensifying the efforts to catalogue and make available to the international community the archival material in the peripheral countries.[4]

Since all the founding members considered travels to be a good topic for the kick-off, the next step was to quickly organize a workshop on "Scientific Travels" the following September 2000 in Lisbon. In 2002, on the island of Aegina, the group tackled the problem of scientific textbooks and their circulation; in 2004, in Aahrus, the national historiographies of science; in 2006, on the island of Minorca, the popularization of science in the periphery; in 2008, in Istanbul, the problem of scientific controversies in the periphery, as well as a revision of old STEP subjects. Galway in 2010 and the island of Corfu in 2012 hosted the next STEP meetings, both covering different subjects and organized into thematic sessions. Lisbon 2014 aimed to provide continuity to the STEP conferences, but also to reassess the main aims and objectives of the group 15 years after it was founded in Barcelona.

In the following sections, I shall describe in more detail STEP's main achievements through all these conferences, and also what I think still remains to be done in the near future. But let us first begin by sketching the academic spirit that the group has created over these years and the crucial role that Kostas has played in the whole endeavor. During a good part of its short history, STEP has developed a particular organizational style, which has only been seriously impaired by the recent draconian cuts to research budgets—mainly at the European periphery. I think that this style deserves to be known by and spread to our fellow historians of science and scholars at large.

6.2 A Genuine Academic Culture

Ever since it was founded—and here, again, is Kostas's invaluable contribution—STEP was conceived as an informal, flexible but efficient network of scholars working together without the constraints of formal scientific societies. It also aimed to help its members to overcome bureaucracy and the frequent academic provincialism of their local institutions. Admission to STEP only required, and still requires today, a simple e-mail with a short paragraph introducing the new member's main research interest and his/her potential links with the group. After acquiring a password, any member can be active in the webpage by providing information on meetings, events and new publications.

[4] STEP, first Meeting. Barcelona 1999. Unpublished manuscript.

STEP has never had stable research funding. It has mainly relied on the dynamism of each group or member and their own ability to raise money. At least up to the meeting in Istanbul in 2008, the local organizers always paid full accommodation for all STEP members attending and giving papers at the conference, who only covered the cost of their flights. In our present times of economic and political crisis in Europe and the growing commoditization of knowledge, this framework might sound naïve or utopian. Actually, the continuing growth of the group and progressive cuts in public research funding have made this ideal endeavor almost impossible. As a result, the conferences in Galway (2010), Corfu (2012) and Lisbon (2014) became closer to standard international meetings, with registration and accommodation fees, official web pages, private sponsors and so on.

I have to admit that my expression of nostalgia of a supposed "golden age" of academic solidarity may cause certain uneasiness among many colleagues, and even STEP members. This is obviously a controversial issue and probably another sign of the difficulties that Europe as a whole is facing today in building up a consistent political project for the future. Once the academic hegemony of the Anglo-American model has been taken for granted, we risk forgetting that the plurality of cultures and languages across Europe is inevitably associated with academic plurality. I am not debating the usefulness of English as "lingua franca" for our present Republic of Letters, but in my view STEP has contributed to encouraging historians of science to deal with academic plurality, accepting that, perhaps for the best, there is still a certain cultural "incommensurability" between a paper published in English in *Isis*, for instance, and another article appearing in Italian in *Nuncius* or in Spanish in *Dynamis*. From its founding, STEP has been designed to deal with linguistic plurality, to fight against linguistic barriers and cultural traditions and to try to build up a new European framework from peripheries to centers and vice-versa. We use the concept of "periphery" in two senses, as a topic of study in the past but also as a scholarly condition.

One could consider that the gradual metamorphosis from the old STEP meeting style to a more standardized academic conference is a sign of maturity and success. Nevertheless, in those 15 years some virtues have sadly disappeared: to frankly share, discuss and compare case studies from different countries in different periods; to encourage the use of a plurality of languages and cultures; to establish new ways of working on comparative history; and even to develop a schema of analogies and differences between travelers, teachers, instruments-makers, university professors and professional scientists in different countries, regardless of their "peripheral" nature in terms of science and technology.

Since its foundation, STEP has also struggled with the renewal of its leadership. There is no doubt that, among the founding members who gathered in Barcelona in 1999, Kostas played a very important role. I still remember him flying in from the US to Barcelona during his stay at the Dibner Institute in Boston and soon after his father's death, to support the old, "crazy" idea that emerged in the coffee break in Delphi 2 years earlier. However, this initial driving force by a close circle of Kostas's friends and colleagues was not in contradiction with the idea of giving voice after its 1999 inception to the new generations, to such an extent that I dare say that the group is already led today by a third wave of scholars. In all these years, a vaguely defined Steering Committee charged with the main basic tasks needed to maintain the STEP network and the organization of the biannual conferences has sufficed to keep the pace. Even though at first the Steering Committee was mainly made up of national representatives from the different countries—one has to admit that the groups in Spain, Portugal and Greece have played a very active role in the project—it later shifted, after 2008, to representatives from the different research groups, and now, in the present, it has a more pragmatic arrangement. This includes STEP members in the SC, regardless of their national origin, but ones who are personally committed to leading the group in a horizontal, tacit organization with a reasonable division of tasks.

It goes without saying that from the informal coffee break in Delphi and the founding workshop in Barcelona to the last meeting in Portugal in 2014, the group has experienced deep transformations. What began as a modest gathering of a very small group of colleagues-friends has grown considerably in both quantitative and qualitative terms: the e-mail list (nodus@uv.es) holds more than 200 STEP members; figures on scientific productivity in books and papers issued from former STEP meetings and from individual research done under the STEP agenda are quite significant: STEP has gained visibility in national and international forums[5]; the webpage has been renewed and updated (http://www.uoa.gr/step); and new collaborations with historians of science beyond Europe, in particular with Latin America colleagues, have been developed in recent years. Furthermore, the historiographic framework of the group has been discussed, fine-tuned and widely spread.

After 15 years, a research project can probably be considered mature so now might be the time to assess what the group has "really" achieved and what, in my view, is still lacking: its pluses and minuses, its own original character, its limitations and avenues it still has to pursue in the future.

6.3 Enriching Mainstream Historiography

Taking "centers" and "peripheries" as flexible and dynamic categories, STEP research has indeed contributed to enriching the study of the circulation of knowledge in the past. It was after the founding meeting in Barcelona that the early STEP agenda had to be put into practice. For that purpose, the 2000 conference in Lisbon was devoted to exploring scientific travels from centers and peripheries as a useful

[5] This was the case, for instance, among other cases, of the European Society for the History of Science (ESHS) second International Conference, held in September 2006 in Cracow (Poland). A session was devoted there to "Science and Technology in the European Periphery (STEP): (Re) assessing some of historiographical issues". It aimed to discuss a number of issues associated with the "appropriation" of scientific ideas and practices from the various centres of Europe to the regions of the European periphery. Other STEP sessions were organized regularly in international meetings, whereas single STEP members progressively spread that historiography in individual papers and books.

category to better understand the ways in which knowledge flowed in different historical periods. Detailed reconstructions of several journeys from the eighteenth century onwards were compiled and nicely edited in the book *Travels of Learning* (Simões et al. 2003), which was probably the group's first important publication. It offered a reappraisal of the topic with case studies from Portugal, Spain, Greece, Turkey, Russia, Hungary and the Scandinavian countries. *Travels of Learning* is already 12 years old, but I am still convinced that the idea of spotlighting "travels" as an analytical category for the history of science was a seminal and very useful STEP contribution, which even today deserves further development. The book was advertised as follows:

> Travels have without doubt been a perennial source of attraction to scholars in different fields. Yet historians of science have seldom looked at travels within the European space. *Travels of Learning* will help to fill this gap. It offers a reappraisal of the topic of scientific and technological travelling and takes the viewpoint of the European peripheries [...] The book covers different periods of time and different local settings, and uses a variety of methodological approaches. It contributes to the clarification of mechanisms of appropriation of scientific ideas, instruments, and practices and of technological expertise.[6]

Two years later on the Greek island of Aegina, the group worked on another aspect of the circulation of knowledge, this time textbooks as mediators between experts and lay people, in particular in "peripheral" countries. Departing from the work on chemical textbooks, which had been developed some years earlier as part of the aforementioned ESF project on "The Evolution of Chemistry in Europe" (Bensaude-Vincent and Lundgren 2000), the result after Aegina was a special issue of the journal *Science and* Education (Bertomeu-Sánchez et al. 2006a) which included a selection of conference papers and again discussed mechanisms of knowledge transfer in specific local contexts and their actors, as well as their inevitable link to mainstream scientific knowledge and the standardization of academic disciplines and professions.

The special issue reinforced the STEP agenda, this time from the perspective of the classroom and the textbook as a cultural object, with a particular emphasis on localities in the periphery. As the editors stressed, the papers it contained analyzed "the changing local education systems in which textbooks were written, printed and read", and claimed for the historical reconstruction of teaching spaces, which "... are usually neglected by master narratives in history of science ... but ... can provide valuable resources for a fresh comparative approach to the history of scientific teaching practices", (Bertomeu-Sánchez et al. 2006b, 658). The publication stressed how scientific teaching is no longer regarded as an act of passively transmitting knowledge but as one of the chief spaces in which scientific knowledge is constructed. Since teaching is an activity under strong social, economic and political pressures, a cultural analysis of textbooks offered an accurate window to study science and technology and their social and political underpinnings in the European periphery.

[6] http://www.springer.com/new+%26+forthcoming+titles+(default)/book/978-1-4020-1259-4 (last accessed, 12-10-2014).

In 2004, the fourth STEP meeting in Aahrus (Denmark) provided an ideal setting to discuss the never-ending challenge of writing national histories of science with our Danish colleagues, who were then involved in an ambitious project for the publication of a history of science in Denmark (Kragh et al. 2008) which was associated with the Danish History of Science Project and the Department of History of Science (IVH) at the University of Aarhus. The call for papers already stressed that the writing of a four-volume work on science in Denmark from the Middle Ages until recent times had reinforced the historiographic debate about luminaries such as Tycho Brahe, Nicolas Steno, Ole Rømer, Hans Christian Ørsted and Niels Bohr, but also about "second class, peripheral" Danish scientists. The Danish project, which could also be applied to other small, peripheral European countries, emphasized that:

> Just like literature and art, science has served nation-building purposes, and in this process historiographies of science have played a major role. While this is the case for all countries, it is to be expected that countries in the European periphery, by virtue of being peripheral, have something in common that *is not shared* by the greater nations in the scientific centre. We want to explore how the peripheral status is reflected in the histories of science written in the various countries. The proposed theme is neither limited to particular sciences nor chronologically limited. In many countries, we find histories of science back in the seventeenth century, and later on all countries developed their own historiographical traditions with regard to their specific local scientific heritage. The aim of the workshop is to explore in a comparative perspective these traditions. Although a substantial part of the workshop is expected to deal with past historiographical traditions, we will also discuss the contemporary situation of history of science in the relevant countries".[7]

The meeting obviously brought to the fore the problem of the local scale of the historical narrative and the inevitable dependence on big countries and international trends by local actors in small, peripheral contexts such as Denmark. But in addition, it also stimulated the publication of a collective volume on national historiographies of science, first in the Greek journal *Neusis*[8] and later, in English, in the Italian *Nuncius*.[9] Both volumes probably became one of the landmarks in

[7] Fourth STEP meeting. Call for papers (unpublished text).

[8] Patiniotis (2006). The volume contains the following papers: [In Greek] Manolis Patiniotis, "Nation, Science, Identities. Historiography of Science in the European Periphery". 3–16; Ana Simões, Ana Carneiro, Maria Paula Diogo, "Issues in the Historiography of Science in Portugal. A look from the standpoint of four twentieth century types of sources" 17–39; Berna Kılınç, "History of science as a civilizational project" 40–49; Agustí Nieto-Galan, "The history of science in Spain: Imperial past, peripheries and the making of the modern state" 50–74; Ernst Homburg, "Boundaries and audiences of national histories of science: Insights from the history of science and technology of the Netherlands". 75–109.

[9] *Nuncius*, 23 (2008): Nieto-Galan, A., "The history of science in Spain: a critical overview", 211–36; Simões, A., Carneiro, A., Diogo, M.P., "Perspectives on contemporary history of science in Portugal", 237–63; Patiniotis, M., "Origins of the historiography of modern Greek science", 265–89; Kilinc, B., "Ahmed Midhat and Adnan Adivar on history of science and civilizations", 291–308; Homburg, E., "Boundaries and audiences of national histories of science: insights from the history of science and technology of the Netherlands", 309–45.

STEP's efforts to write a comparative history of science. In that case, simply bringing together papers on the historiography of science in Italy, Greece, Portugal, Spain, Turkey and the Netherlands helped the reader to identify common questions and concerns when grappling with the challenge of writing national histories of a science developed in countries that have acted as centers as well as peripheries in different historical times. Nation-building, the rhetoric of backwardness, foreign versus local luminaries, utilitarian discourses, scientific travels and local educational policies, among other issues, emerged as potential common themes for further comparison.

After Denmark, the group discussed the need to produce a more consistent theoretical framework to support further research and to attract new members—actually our nodus list had been growing steadily throughout those early years. Informal gatherings in Greece, hundreds of exchanged e-mails and draft versions ended up in a manuscript of consensus, which was signed by nine STEP members and appeared in *History of Science* in 2008 (Gavroglu et al 2008), while other STEP members soon contributed substantially to debates on the national and transnational circulation of scientific knowledge at a comparative level, as a positive sign of the vitality of the group and its capacity for renewal (Simon et al. 2008; Simon 2012, 2013; Turchetti et al. 2012).

From that simple wish list in 1999 to the group's first historiographic paper, there is no doubt that substantial progress was achieved. What we used to informally call the "theoretical paper" (TC) tried to define boundaries between old diffusionist models and colonial-postcolonial studies, and the new STEP historiography at a European level. It discussed in-depth concepts such as *appropriation* as active processes of the circulation of knowledge in which the supposedly peripheral receivers play a much more active role than in the old model. As suggested in the TC:

> A historiography of appropriation allows us to examine systematically the particular forms of the fusion of aspects of the science and technology with local traditions, and the specific forms of resistance encountered by these new ideas and techniques; the extent to which such expressions and resistances displayed local characteristics; the procedures through which the new ways of dealing with nature were made legitimate; the commonalities and differences between methods developed by scholars at the periphery for handling these issues and those of their colleagues in the central countries of Western Europe; the role of new scientific ideas, texts and popular scientific writings in forming the rhetoric of modernization and national identity; the prevailing mode of scientific discourse among local scholars; the relation between political power and scientific culture; the social agendas, educational policies and (in certain loci) the research policies of scientists and scholars; the shifts in ideological and political allegiances brought about as the landscape of social hierarchy changed; the consensus and tensions as disciplinary boundaries were formed, especially as reflected in the establishment of new university chairs; and the ideological undertones of the disputes, and their cognitive content. As a result, what emerges from this is a richer and more complex picture of how science and technology were integrated in the European periphery (Gavroglu et al. 2008, 160–161).

What I think the theoretical paper wanted to make clear was that the STEP agenda went beyond a simple "antiquarian" collection of more or less "interesting" case studies from countries on the geographical periphery of Europe. It was a *historiographic standpoint* that could enrich and perhaps later, in due course, challenge some aspects of the mainstream historiography of science: "Starting from the periphery (or, better, *standing* on the periphery) might offer a clearer view of the intricate ideological constructs which accompany the establishment of science and technology, and at the same time, unveil their socio-political dimensions" (Gavroglu et al. 2008, 168). And for that purpose, research subjects that STEP had already tackled and those to come in future conferences became an intrinsic part of that historiographic standpoint.[10] The making and publishing of the TC ran parallel to the development of a new STEP research subject, which not only contributed substantially to enriching our historiography, but also provided more international visibility for the group. It was precisely in 2006, on the island of Minorca, that STEP devoted its fifth conference to the "Popularization of Science and Technology in the European Periphery" as a further dimension of the circulation of knowledge between experts and lay people, but also as a cultural product traveling from centers to peripheries (Nieto-Galan and Papanelopoulou 2006; Papanelopoulou et al. 2009). The meeting in Minorca provided very valuable raw material for the publication of a collective volume, published in 2009 by Ashgate, which analyzed the double "peripheral" character of popular science with new case studies. As reviews published in prestigious history of science journals have shown, the book has enjoyed a notable impact among academic circles.[11] The call for papers emphasized the potentialities of studying science popularization in peripheral countries in the following terms:

> Since a vast majority of peripheral cities in Europe have never had a Newton a Darwin or an Einstein, the historical analysis of their scientific culture should be rather focused on the spread of scientific ideas in every local context than on the history of great luminaries. What kind of images of science were developed by peripheral working scientists and early popularisers and for what kind of audiences, from late eighteenth century – the period of the emergence of the public sphere – until late 20th century - in the heart of a mass information society -? We can try to answer this question through different levels of analysis, which might be useful in the process of the writing of the papers for our next STEP meeting on "Popularisation of Science in the European Periphery".[12]

Popularization of Science and Technology in the European Periphery and the STEP project of science popularization as a whole also contributed to bringing the *daily press* to the fore as a very valuable primary source, which historians of science have not yet properly exploited but which is of great interest in central and

[10] "In particular, studies focusing on travels, forms of scientific practice and teaching, scientific controversies and on ways of communicating science in the European periphery have raised interesting questions, and provided clues to the re-examination of historical and historiographical issues." Gavroglu et al. (2008, 168).

[11] Take for instance the case of *Isis*, which does not usually review collective volumes, as a positive sign of its reception among the international community, Bensaude-Vincent (2010).

[12] Fifth STEP Meeting. Call for papers. (unpublished manuscript).

especially peripheral countries. A special issue of *Centaurus* devoted to this new topic, also published in 2009, is another important contribution by the STEP group to mainstream historiography (Papanelopoulou and Kjaergaard 2009). It is fair to mention that the idea of a crude empirical approach to the daily press was Kostas's seminal inspiration in an informal gathering of some STEP members in Aegina in 2005 as part of the early design of the TC. From that early stage onwards, it has been easy to conclude that writing the history of popularization practices in the European periphery implies a necessary recovery of a still-unknown yet enormous bibliographic heritage, including popular scientific books, science fiction novels, popular scientific journals, articles in the everyday press, pamphlets, publications and archive material from national and international exhibitions, public celebrations and tributes to local scientists and public debates on the acceptance or resistance to important theories such as Darwinism or even to controversial practices such as phrenology.

Istanbul 2008 was probably a landmark in the short history of STEP for various reasons. As mentioned above, this was the last meeting in which the old academic culture of local generosity could be put into practice thanks to the amazing hospitality of our Turkish colleagues and of Professor Feza Günergun in particular. It also became an intellectual bridge with the Eastern historiography of science, in particular with the Indian tradition of colonial and postcolonial studies in which our key concepts of "center" and "periphery" also played a crucial role (Günergun and Raina 2011; Vlahakis et al. 2006). This sixth STEP meeting provided a good opportunity to further our understanding of themes (travels, textbooks and the popularization of science in the European periphery) on which we had already made progress (see STEP publications) and at the same time opened for discussion the topic of scientific controversies in (or involving) the European periphery. As stated in the call for papers:

> While there is an extensive literature on scientific controversies they have seldom been addressed from the point of view of the European periphery. Controversies are instances of science-in-action which are particularly suited to highlight the dependence of science from local contexts, in their multiple social, cultural, political, institutional, and religious dimensions as well as from the idiosyncrasies of individual contenders particularly vis–àvis the cognitive dimensions of science. Therefore, they appear particularly suited to assess the specificities of the practices of appropriation of science throughout time and across disciplines in different sites of the European Periphery.[13]

Although the conference devoted several sections to the analysis of "scientific controversies" in the periphery, the multi-thematic nature of the meeting—now much closer to a standard international conference on the history of science—weakened our capacity to produce well-focused collective publications. But Istanbul 2008 was also a good occasion to celebrate the publication of the TC and the appearance of a new collective volume, this one published by the new generation of

[13] Sixth STEP Meeting. Call for papers (unpublished manuscript).

STEP members. Josep Simon and Néstor Herrán were in charge of coordinating the book *Beyond Borders*, which clearly aimed to make its mark in mainstream international historiography. The editors stressed the need to "problematize the local, the national and even the international through comparison and through the assessment and analysis of communication practices"; they called "for a more fruitful integration and diversification of national case studies in our field" and the "need to promote internationality in history of science as a requisite of outstanding scholarship" (Simon et al. 2008, 11).

Similar to Istanbul 2008, Galway 2010 was a fully multi-thematic conference with sessions organized by the different STEP research groups, but with the novelty of welcoming colleagues from America as a clear extension of common historiographical interests beyond the European borders. The meeting "built upon the work of previous conferences, but also encouraged a focus on areas which have so far been underrepresented in STEP (especially medicine and technology)". It particularly encouraged "contributions with a transnational dimension (either within Europe, or relations beyond Europe), or with a philosophical/theoretical angle on the nature of peripheries and their significance in the history of science, technology and medicine."[14] Galway also held a special session devoted to the Irish history of science with case studies that were unknown to the general STEP audience. As has happened in Denmark some years earlier, the meeting became an excellent occasion to learn about other aspects of science in the European periphery.

This is a trend that continued in the eighth STEP meeting on the island of Corfu, where the presence of Latin American colleagues was even more significant. The conference opened a window on current new opportunities for collaboration on joint research projects on both sides of the Atlantic. In addition, Corfu was also intended to give voice to the established STEP research groups, which, though tackling issues belonging to the mainstream historiography, were attempting to enrich it with new "peripheral" perspectives. Specialized research groups, mainly stemming from former STEP meetings, cover research subjects such as the cross-national, comparative and transnational history of science; experts; material culture of science: museums and collections; popularization of science; science for medicine; science in the press; universities; and women in science.

Perhaps Corfu did not rise to the earlier expectations of potential collaboration with colleagues beyond the European borders. However, after some months of discussion, STEP has already found a way to pursue the Corfu transatlantic agenda. No doubt much remains be done in that direction. Lisbon 2014 may present new challenges for our historiographical focus, where the old case studies, which illustrated and complemented the already-settled mainstream questions, will inevitably find their proper place in the jungle of circulation, transmission, global, transnational, international, colonial and postcolonial studies. This jungle will serve as a battlefield for challenging the academic hegemony of the discipline in the next decades.[15]

[14] Seventh STEP meeting, call for papers (unpublished text).

[15] For the question of academic hegemony see Nieto-Galan (2011).

6.4 Conclusion: Challenging Mainstream Historiography

Nostalgia over the coffee break in Delphi in that pleasing chat with Kostas and other colleagues is probably over. Recent trends in the global, transnational and postcolonial history of science might challenge the STEP research agenda, but they also include concepts such as circulation, knowledge in transit, appropriation, go-betweens, mediators, etc., which seem to be in tune with STEP's original program, and I hope they will play a relevant role in the near future.[16]

In spite of all these potential convergences, I am convinced that the problem of the heterogeneous and plural nature of European science has not yet been sufficiently tackled. In fact, in these past 15 years STEP has precisely been trying to decenter this homogeneity, which is often taken for granted. Its main historiographic standpoint still lies in the idea that a more detailed, symmetrical analysis of science and technology in Europe would probably modify some relevant aspects of the big picture that historians have tacitly agreed to in recent decades. Perhaps in the near future, STEP research will indeed contribute to reshaping and even challenging some of the tacit assumptions of present-day mainstream historiography. It shall contribute, for instance, to discussing issues such as the idealization of modern science as an activity that was not necessarily taken for granted in certain European contexts; to bring to the fore the high political content embedded in scientific debates, especially in places that felt particularly backward in specific historical periods; or even to analyze in depth the local rhetoric overemphasizing the role of foreign scientific authorities.

Take, for example, the case of peripheral scientists educated under the influence of the scientific elites of the centers. Did they uncritically favor hagiographic accounts to strengthen the scientific culture of their country? Did the uncritical reception of science of the center tinge science in the periphery with "non-political, neutral, objective accounts" that often praised international authority? Or did peripheral scientists resist and actively appropriate foreign intellectual agendas? Could we perhaps extrapolate that framework to stimulate new critical reflections on the cultural mechanisms of the circulation of knowledge among experts and laypeople and vice-versa, in both centers and peripheries?

In a similar line, peripheral scientists played a very important role in the making and circulation of scientific literature, but often without a clear distinction between the experts' and the laymen's accounts. In the periphery, in a context of low professionalization of science, the boundary between amateurs and professionals is harder to establish, and further case studies in these contexts might contribute to reframing important mainstream debates on expertise, scientific authority and disciplinary boundaries. Future studies on "amateur science"—outside of Britain—might be fed by still unknown case studies from the peripheral contexts. Equally, detailed case studies of scientists in the periphery can also contribute to

[16] See for instance: Secord (2004), Schaffer et al. (2009), Renn (2012).

analyzing strategies by local political and economic elites. The activities of provincial scientific societies across Europe were often designed to improve the arts and manufactures of a specific locality, but also to legitimize the social prestige and political control.

But we could even consider other examples. In revisiting the reasons for the fall of the Spanish Empire in the sixteenth and seventeenth centuries, Jorge Cañizares-Esguerra and Antonio Barrera-Osorio have emphasized the importance of the complex network of knowledge exchange between the center and the colonies and its later influence on the natural philosophy of the Scientific Revolution in Northern Europe (Cañizares-Esguerra 2004, 2006; Barrera-Osorio2006). Of course, Spain can be considered a center when analyzing the intellectual production at the height of the Empire, but for decades, Spanish science has been traditionally relegated to a "peripheral"—even marginal—position in the big picture of the Scientific Revolution and the emergence of what we know today as modern science. Today this assumption is under serious revision and, inspired by new peripheral case studies, it could be potentially extrapolated to other episodes of mainstream historiography.

These are, of course, only preliminary avenues for further research, but, in any case, I hope that STEP contributes in the future to making a new big picture of the history of science in Europe. Perhaps not by chance and thanks to Kostas Gavroglu's intellectual passion and personal generosity, STEP's seminal idea was born in Delphi, in one the mythical sites of the origin of Western civilization, in a country whose citizens have suffered severe, unjust humiliation in recent years as a negative indication of the weaknesses and contradictions of the European political project. In spite of this, I still hope that the STEP research agenda can humbly contribute to reversing our present pessimism and to progressively developing a new multicultural approach to a truly European history of science, which is still to come.

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Chapter 7
At the Center and the Periphery: Joseph Pitton de Tournefort Botanizes in Crete

Lorraine Daston

Abstract The French physician and botanist Joseph Pitton de Tournefort (1656–1708) set sail from Marseille to Crete on 24 April 1700, in the company of the German physician Andreas Gundelsheimer and the artist Claude Aubriet, on a voyage to the Levant financed by the French crown. Among the many aims of the voyage – scientific, ethnographic, political, economic – was the identification of the plants described in Dioscorides' *Materia medica*, still an important source for the early modern pharmacopia, and the discovery of new plants, especially those with medical uses. Crete was the first port of call on Tournefort's two-year voyage, in part because ancient sources had praised its botanical riches. But Tournefort's own experience of Crete shook his expectations, both of the reliability of ancient botanists and the continuity of ancient and modern Greek culture. His perceptions of Crete fuse the seventeenth-century categories of the Ancients versus Moderns debate with incipient Enlightenment views on intellectual progress and stasis. The discoveries and disappointments of Tournefort's report on Crete, recorded in the form of letters to colleagues and crown officials back in Paris, reveal the moment when Greece ceased to be a purely historical and highly idealized notion and began to be relegated to the periphery by a self-declared West European center.

Keywords Joseph Pitton de Tournefort • Botany • Dioscorides • Theophrastus • Crete • materia medica • Center • Periphery

7.1 Introduction: The Fallen Gods of Greece

3 July 1700: The French botanist Joseph Pitton de Tournefort, accompanied by the German physician Andreas Gundelsheimer, the French artist Claude Aubriet, and a

In honor of Kostas Gavroglu

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native Cretan guide,[1] scales Mount Ida in the scorching summer heat in search of plants. For Tournefort, educated in the Greek and Latin classics by Jesuits in his native Aix-en-Provence, the place is steeped with mythological and botanical associations: Mount Ida was where the goddess Rhea hid her infant son Zeus from his father Cronos; it was also the highest peak on Crete, a place described by the ancient Greek botanists Dioscorides and Theophrastus as particularly rich in plants renowned for their medicinal virtues. Short of Parnassus, then inaccessible to foreign travelers because of border skirmishes between the Turks and Venetians, Mount Ida represented the summit of Tournefort's hopeful expectations to rediscover the lost heroic world of Homer and the lost plants of Dioscorides.

In the event, he was bitterly disappointed on all counts. As he wrote to his Parisian colleague Pierre Bonnet Bourdelot, physician to the Duchess of Burgundy, "though it may displease Jupiter, who was said to have been nursed and hidden here, this is the most disagreeable mountain I have ever seen in my life. [. . .] there is not even a little forest, here one finds neither landscape nor pleasant solitude nor brooks nor fountains". No romantics, Tournefort and his companions were unimpressed by the panoramic view of the sea from the mountain's peak and did not find the "horrible precipices" in the least sublime. They were greatly relieved to descend into a valley verdant with olive trees, cypresses, fruit trees, and laurels, populous with villages, and well irrigated by streams. Even the mountain plants were below par: "The plants of Mount Ida hardly merit the climb to see them; one finds them elsewhere more conveniently."[2] Elsewhere he wrote acerbically that the vine described by Theophrastus as native to Mount Ida was nowhere to be found, only pebbles, steep cliffs, and a few miserable goats. The mountain, he concluded, had no attractions except "its famous name in ancient history."[3]

Tournefort's disappointments on Mount Ida were emblematic of the contradictions of his mission. On the one hand, he had been sent by order of the French king Louis XIV to retrace the steps of the ancient botanists and physicians, especially Dioscorides, in order to identify firsthand and conclusively the plants specified in extant manuscripts so that they could be reliably used as materia medica. In a letter written to the abbé Bignon, official head of the Paris Académie Royale des Sciences, from Mykonos in December 1700, Tournefort boasted that he had already found more than half of the plants mentioned by Dioscorides and avowed that "Dioscorides restitutus was the finest work that an academician

[1] Guides were barely mentioned in the published account, but in letters Tournefort acknowledged that their help was invaluable: he mentions especially a "Greek valet who climbs like the devil, understands Provençal [Tournefort came from Provence], and is a great help to us." Joseph Pitton de Tournefort to Louis Morin (1700), 42.

[2] Joseph Pitton de Tournefort to [Pierre Bonnet] Bourdelot (1700), 57–59.

[3] Joseph Pitton de Tournefort (1717), 62–66.

could bring to light."[4] From this point of view, Crete and the other Greek islands Tournefort and his companions explored were still the center of the botanical world, sources of valuable plants and also precious knowledge linking them to ancient wisdom.

On the other hand, part of Tournefort's royal charge was to observe and report on the geography, religion, and customs of the places he visited in the Levant, writing not only to the abbé Bignon and his fellow academicians on scientific matters but also to Louis Phélypeaux, count of Pontchartrain, erstwhile secretary of the French navy (a post in which his son Jérôme Phélypeaux succeeded him in 1699) and Royal Chancellor to Louis XIV from 1699 to 1714. Long lists of plant species, dried specimens, and drawings by Aubriet were supplemented (and sometimes overwhelmed) by detailed descriptions of matters relating to the character and culture of the peoples (Greeks, Turks, Armenians, Jews, Persians) encountered on the voyage. Especially in places saturated with classical associations—Tournefort always had his Homer, Thucydides, and Strabo close at hand, as well as his Dioscorides, Hippocrates, and Galen—this double charge resulted in double vision, juxtaposing ancient and modern peoples and achievements, often in the form of a melancholy contrast between ancient grandeur and modern decadence. By 1700 cities like Candia (Heraklion) and Gortyn seemed shadows of their former selves in Minoan, ancient Greek, and even Roman times; Tournefort thought the majestic ruins of the ancient temples put the rude architecture of the churches erected on their sites to shame. Confronted with what he interpreted as signs of cultural decline, Tournefort not only found himself on the periphery of Europe; he was among those western European travelers on the eve of the Enlightenment who helped forge the language of center and periphery, of civilizations on the upswing and those on the downswing.[5]

This was a very different language from savagery (whether wild or noble), primitive simplicity, or original state of nature, as cultivated by early modern European travelers to the Americas. In John Whyte's illustrations to the Theodore de Bry edition of Thomas Harriot's *Briefe and True Report of the New Found Lands of Virginia* (1588), for example, images of the native inhabitants were juxtaposed to those of the British Picts as described by the Romans, suggesting that both peoples had been in similar situations of early cultural development.[6] Just as the ancient Britons had eventually matured into modern Elizabethans, so, hinted Whyte, the native Virginians might trace a similar arc of development. For Tournefort, however, the inhabitants of Crete had once been what he and other classically educated European savants still yearned to become: the likes of Theophrastus and Dioscorides, Hippocrates and Galen.

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[4] Joseph Pitton de Tournefort to the abbé Bignon (1700), 297.

[5] On the language of center and periphery in the scientific context, see Michel Blay and Efthymios Nicolaïdis (2001), and on Greece specifically in the early modern period, Dialetis et al. (1999), 41–71.

[6] Thomas Harriot, *A Briefe and True Report of the New Found Land of Virginia* [1590], with De Bry engravings based on John White watercolors (New York: Dover, 1972), 75–85. The section containing the engravings of the Picts was "to show howe that the Inhabitants of the great Bretannie have bin in times past as sauvage as those of Virginia."

Yet his observations on his travels began to tarnish even the reputations of these luminaries. While at home in Paris the battle between the ancients and the moderns on the medical front had been eloquently declared a draw by Bernard de Fontenelle,[7] Perpetual Secretary of the Académie Royale des Sciences, Tournefort came to believe that his gods had feet of clay, just as the fabled Mount Ida turned out to be a hideous pile of rocks. Dioscorides had only described common plants; Theophrastus had never bothered to travel. As measured by the emerging standards of early modern botany (and also geography and medicine), the ancients had begun to slip from their pedestal. A voyage begun in reverence ended in triumph, but only by redefining its aims: no longer the restitution of Dioscorides' *De materia medica* but the discovery of hundreds of new species unknown to any botanist, ancient or modern.

Crete was Tournefort's first port of call after setting out from Marseille, and it was in Crete that his conceptions of the botanical and cultural center and periphery first became unsettled, as he struggled to match his classical education with his observations of contemporary Greece and Greeks. In this essay I will therefore mostly focus on Tournefort's reports from Crete (with one brief excursion to Mykonos), both published and in personal letters, paying particular attention to those points at which ancient and modern diverged in his perception—but also where they surprisingly converged.

7.2 Background to the Voyage

Tournefort was already a seasoned botanical traveler when he embarked from Marseille to Crete on 24 April 1700. Like the sixteenth-century Flemish botanist Charles de l'Ecluse, Tournefort had pursued medical and botanical studies in Montpellier before traveling to Spain to gather plants. Earlier he had roamed his native Provence, the Dauphiné, and Savoy in the company of the botanist and artist Charles Plumier, having immediately abandoned preparations for a career in the church for botanical studies when his father Pierre Pitton de Tournefort, a lawyer in Aix-en-Provence, died in 1677. According to Fontenelle's academic éloge of Tournefort, his reputation as a botanist reached the ears of Guy-Crescent Fagon, physician to the royal family and director of the Jardin du Roi in Paris, who recruited the young botanist to Paris in 1683. Tournefort became professor of botany at the Jardin, undertook another trip to Spain and also traveled to Portugal, Holland, and England.[8]

[7] Bernard de Fontenelle (1754), Dialogue V: "Eristratus and Harvey", 80–83.

[8] Starting in the 1680s, Tournefort's plant-collecting trips were subsidized by the French crown: E. Bonnet (1891), 372–376; 393–395; 420–424. Joseph Laissus and Yves Laissus, "Joseph Pitton de Tournefort (1656–1708) et ses portraits," *90e Congrès des sociétés savantes*, Nice 1965, vol. 3, 17–46; Fontenelle's éloge of Tournefort is reprinted as the (unpaginated) preface to Joseph Pitton de Tournefort (1717), vol. 1. Trips were subsidized by the French crown, and he regularly sent back specimens to Fagon for the Jardin du Roi: E. Bonnet (1891), 372–376; 393–395; 420–424.

It is however unlikely that Tournefort, whatever his earlier travel experience and botanical qualifications, would have been singled out by Louis XIV for a generously state-funded expedition to the Levant without the intermediary of the abbé Bignon, who had been made president of the Académie Royale des Sciences at the instance of his uncle Louis Phélypeaux and who later (in 1719) became royal librarian.[9] It was the abbé Bignon who nominated Tournefort for membership in the Académie in 1691; only thereafter did his publications begin to appear: *Elemens de botanique* (1694), *Histoire des plantes qui naissent aux environs de Paris avec leur usage en médicine* (1698), plus over a dozen papers on botanical topics submitted to the Académie. When he was made professor of medicine at the Collège royal in 1706, it was probably also through the good offices of the abbé Bignon, whose personal physician he became that same year. It is likely that Fagon also put in a good word; at least Tournefort credited him with having explained to Louis XIV "the advantages that could accrue to natural history from this voyage" and who had Tournefort personally presented to the king on the eve of his departure from Paris in March 1700.[10]

There is no reason to doubt that the abbé Bignon was genuinely interested in having Tournefort verify ancient claims concerning natural history firsthand, as well as to "enrich [botany] with new species, and especially those that the most ancient physicians deployed for the cure of illnesses.".[11] But the additional mandate from Phélypeaux to observe religion, customs, commerce, and geography suggests that broader foreign policy aims were also involved in launching the expedition.[12] Since 1536 the French crown had sealed an alliance with the Ottoman emperor, an alliance that had served both sides in good stead during various wars in the sixteenth century. For France, the advantages were both military (primarily checking the expansionist ambitions of the Hapsburg Emperor Charles V) and commercial (promoting French trade in the eastern Mediterranean from the port of Marseille). Under Louis XIV these policies continued, again to put pressure on Hapsburg allies within Europe. French consuls were granted special status within the Ottoman Empire, French merchants dominated European trade with the Levant, French churches were established in Ottoman domains, and the French navy was allowed unusual freedom of movement in the eastern Mediterranean.[13] When Tournefort set out in 1700, French and Ottoman interests had been intertwined for over 150 years (much to the dismay of other European polities, which resented France's effective trade monopoly, feared the military force of its Ottoman allies, and chastised Christian kings for consorting with Muslim infidels).[14]

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[9] On the Phélypeaux dynasty and its patronage networks, see Sara E. Chapman (2004), especially 145–175 on Louis Phélypeaux's influence as Chancellor, 1699–1714. The genealogical chart on 205 shows the abbé Bignon's relationship to the Phélypeaux.

[10] Joseph Pitton de Tournefort (1717), 4.

[11] Joseph Pitton de Tournefort (1717), 3.

[12] See Chandra Mukerji (2005), 19–33.

[13] Daniel Goffman (2008).

[14] On Cretan trade under the Ottomans, see Molly Greene (2000), 141–173, and on French trade specifically, 128–136.

Curiosity about the geography, commerce, religion, and customs of the people was therefore understandable from a diplomatic and mercantile point of view: both merchant and naval vessels needed accurate maps and other geographic information; trade required up-to-date intelligence on local products and markets; all French travelers to the Levant (of whom there were a fair number by the late seventeenth century, encouraged by the privileged conditions offered to them by the Ottoman rulers) craved details about local *moeurs* and religion, in order to ingratiate themselves with their hosts. Given the longstanding Franco-Ottoman alliance, all of these interests may have seemed perfectly legitimate.

But the line between the friendly curiosity of an ally and the prying queries of a spy was a blurred one. It is, for example, not clear why Tournefort enumerated the Turkish military divisions stationed in Candia (Heraklion) in such detail,[15] nor why, when a Turkish official accused him and his companions of espionage for wanting to tour the caves of Rethymno and witness the harvest of ladanum,[16] Tournefort did not explain that he was an emissary of the French crown. Instead, he pleaded that he was only a doctor with charitable intent, offering remedies to the poor people of the countryside gratis.[17] At one point, Tournefort recommended in a letter to the abbé Bignon that Louis XIV send a few French corsairs to the Greek islands in order to put an end to Turkish maltreatment of Christians and Greek insolence towards foreigners.[18]

The remarkable detail in which Tournefort described the religious practices of the Greek rite, from the ecclesiastical hierarchy to priestly vestments to marriage and funeral ceremonies, also hints at an interest that perhaps surpassed ethnographic curiosity. Again and again Tournefort returned to the theme of the ignorance of Greek priests and monks (although he praised many of them for their hospitality and honesty): unable even to read literary Greek properly, they had lost touch with the gospel and patristic sources of their own faith. He was scandalized by the corrupt procedures by which patriarchs were enthroned and deposed under Ottoman rule and appalled to witness the adoration of the host at a point in the liturgy before it had been consecrated: "Through an inexcusable ignorance, the Greeks adore the bread and wine that are not yet consecrated; instead, at the time of consecration, they extinguish the candles and think no more of this sacred mystery."[19] These comments, addressed to Pontchartrain, may simply have been the spontaneous and parochial response of a French Catholic to a rite that deviated from his own. But there is evidence that Tournefort, possibly with the encouragement of Pontchartrain, took a deep interest in what he took to be the decadent state of Greek Christianity, returning to the topic repeatedly and making it the basis for his explanation of how the once-great Greek people had slid into ignorance and superstition, as we shall see.

[15] Joseph Pitton de Tournefort (1717), 48–49.

[16] A plant resin used as a fixative in perfumes and as an ingredient in medications.

[17] Joseph Pitton de Tournefort (1717), 85.

[18] Joseph Pitton de Tournefort to the abbé Bignon (1700), 300.

[19] Joseph Pitton de Tournefort (1717), 117, 143.

7.3 Outdoing Dioscorides and Theophrastus

Tournefort's charge to find the plants described by ancient Greek botanists was one he evidently embraced wholeheartedly at the outset of his voyage, measuring his success by the number of plant species mentioned by Dioscorides that he had managed to match up with living plants in Greece and Asia Minor. But he had hardly arrived in Crete in early May 1700 when doubts began to undermine his esteem for ancient naturalists. As soon as they had paid their respects to the French consul in Chania, he and Gundelsheimer were out eagerly searching for plants along the city walls. But there they found only plants "so common that we wouldn't even have deigned to look at them [had they been] in the environs of Paris, we who were full of fantasies about plants with silvery leaves, or covered by some rich down, we who imagined that Crete could produce only the extraordinary." Where, Tournefort asked reproachfully, were the botanical riches promised by Pliny and Galen, who had favored the plants of Crete above all others?[20] Although Tournefort was to discover his longed-for rarities, such as the *Orchis cretica*, in the inland mountains (though not, as we have seen, on Mount Ida), his suspicions of his ancient guides only deepened.

First, there were the plants that were not found where ancient botanists had located them. We have already heard how Tournefort felt cheated of the vine Theophrastus had assigned to Mount Ida; he was equally scathing about the missing evergreen plantain alleged by Theophrastus, Varron, and Pliny to grow around Gortyn.[21] He tartly corrected Dioscorides about *Origanum creticum latifoliorum*: "if Dioscorides had made the trip [to Crete], he would not have claimed that it bears neither flowers nor seeds."[22] He made similar reproaches to Theophrastus, who had described several kinds of Cretan palms that could not be found; Theophrastus too was guilty of sedentary botany, of trusting secondhand reports.[23] Oddly, Tournefort never considered the possibility that certain ancient plant species might no longer grow in Crete, although he was acutely aware of the effects of human cultivation (or neglect) on both landscape and plant appearance and vigor. The presupposition of his expedition was that, botanically at least, ancient and modern Crete were identical.

Tournefort judged predecessors by the standards of late seventeenth-century botany, which had become a science of robust adventurers, ready to endure pirates, storms, disease, hostile natives, impenetrable terrain, and all the other risks of long journeys in search of new plants. This was the first great age of global plant-prospecting, much but not all of it carried out under the banner of colonialism, as in the Spanish expeditions to the Americas, or of religious missions, as in the Jesuit activities in China, Japan, and India. Plants—used as medicines, spices, dyes, or

[20] Joseph Pitton de Tournefort (1717), 29–31.

[21] Joseph Pitton de Tournefort (1717), 73.

[22] Joseph Pitton de Tournefort (1717), 39.

[23] Joseph Pitton de Tournefort (1717), 56.

ornaments—were big business in the early modern period and their market cachet made botany into the first Big Science.[24] The Jardin du Roi in Paris, with which Tournefort had long been associated, avidly collected exotic specimens from all over the world, both for its herbaria and for cultivation. Tournefort sent back seeds as well as descriptions, drawings, and dried specimens to the Jardin, and was distressed to learn that a shipment sent to Fagon had apparently been lost.[25] The scientific value of *autopsia*, of inspecting plants firsthand and in situ, was inextricably intertwined with the economic value of plant collecting. Both conspired to make travel the sine qua non of early modern botany, and on that score, the ancient botanists fell short of Tournefort's rugged and rigorous standards.

In both ancient and modern contexts, the driving interest behind botany was pharmaceutical: plants were first and foremost ingredients in medications that could cure diseases or at least soothe symptoms. The spectacular discovery of the properties of Peruvian Chinchona bark ("Jesuit bark") as a specific against malarial fevers in the early seventeenth century fanned hopes of finding other marvelously efficacious plant remedies, which in turn loosened state and private purse strings when it came to funding botanical expeditions like that of Tournefort.[26] If Crete was such a paradise of plants as Tournefort had been led to expect by the ancient authors, surely the medicine practiced in places to which these healing plants were indigenous would flourish accordingly?

Here, too, Tournefort's expectations were disappointed. Time and again he commented on the ignorance of Cretan practitioners, both Greek and Turkish. He and Gundelsheimer physicked Greek monks with emetics, lice-infested novices with delphinium extract, and fever-stricken children with a few grains of tartar mixed in lukewarm water.[27] Neither the Turkish nor Greek physicians knew of the mercury cure for French pox (an Irish surgeon with experience serving in the French army had to be engaged to treat the Turkish viceroy for this malady)[28]; in Tournefort's opinion the best doctors in all the Greek islands they visited were the Jesuits, Capuchins, and Cordeliers, to whom the populace turned when the local doctors proved powerless to help.[29] He was struck by how the Cretan villagers brought out their sick to be treated in the street when he and Gundelsheimer passed through, "just as in the time of Hippocrates."[30] Using whatever local plants that were ready to hand in such cases, just as their ancient colleagues would have, the

[24] Londa Schiebinger (2004); Londa Schiebinger and Claudia Swan (2005); Harold J. Cook (2007); Daniela Bleichmar (2012).

[25] Joseph Pitton de Tournefort to the abbé Bignon (1700), 298; idem to Vaillant, 8 April 1701, ibid., 407.

[26] See the articles by Bleichmar, Cook, and Schiebinger in Londa Schiebinger and Claudia Swan (2005); also Londa Schiebinger (2004), 35–45; 73–104.

[27] Joseph Pitton de Tournefort (1717), 64, 125; Joseph Pitton de Tournefort to Guy-Crescent Fagon (1700), 289.

[28] Joseph Pitton de Tournefort (1717), 50.

[29] Joseph Pitton de Tournefort to Guy-Crescent Fagon (1700), 289.

[30] Joseph Pitton de Tournefort (1717), 104.

two Parisian physicians must have experienced a strange telescoping of time, ancient and modern collapsing into one another.

This impression could only have been strengthened by the information their patients provided about the vernacular Greek names of the plants used to treat them. Tournefort was thunderstruck by how often the colloquial modern names coincided with those used by Dioscorides and Theophrastus; indeed, the testimony of the modern Greeks became his main source for matching up the plants he found with those described in ancient sources. “I regard the brains of these poor Greeks as so many living inscriptions, which serve to preserve for us the names cited by Theophrastus and Dioscorides; although subject to various changes, they [the inscriptions] will doubtless endure longer than the hardest marble, because they are daily renewed, whereas marble is effaced or destroyed. Thus these sorts of inscriptions will preserve for centuries to come the names of several plants known to those clever Greeks, who lived in more learned and happier times; we learned in this manner over 500 [names].”[31]

The image of the modern Cretans as living inscriptions, at once faithful and durable yet as unknowing as marble, vividly captures Tournefort’s double vision of Crete, where the link between ancient and modern was both tragically severed and yet eerily ubiquitous. He observed how Greek priests each insisted on having his own chapel and recalled how the ancient Greeks had erected small temples to a swarm of local deities; how modern Cretans were still as deft with the bow and arrow as their ancient ancestors had been reputed to be (though they seemed to have forgotten their prowess with slingshots); and less flatteringly, how the Cretan character had remained as prone to deception and dissimulation as Plutarch had warned long ago.[32] Yet Tournefort also perceived many signs of jagged rupture with the past after centuries of foreign occupation, first by the Venetians and, after 1669, by the Ottomans, leading him to puzzle over the rise and fall not only of civilizations but of whole peoples.

7.4 Decadence and Ruins

Standing before the ancient ruins of Gortyn Tournefort intoned an elegy to the glory that was Greece. Among the broken but finely wrought columns and pediments of marble, jaspar, and granite sheep grazed; “instead of the great men who built such edifices, there are only poor shepherds, who don’t have the wit to catch the rabbits that run through their legs nor the pheasants at their feet.” Many elements of the ancient buildings had been carted away and cannibalized in more recent structures; Tournefort remarked upon a garden entrance graced by two beautiful antique columns topped with a crude clay keystone, a jarring emblem of the gap between

[31] Joseph Pitton de Tournefort (1717), 105.

[32] Joseph Pitton de Tournefort (1717), 135, 100, 88.

ancient and modern Cretan architecture.[33] Throughout his tour of the Greek islands (and also in Constantinople), Tournefort remarked disparagingly on the rude simplicity of the buildings that were rarely more than two stories high; the (by Parisian standards) modest Jesuit residence in Naxos impressed the local inhabitants because it contained an internal staircase and was altogether "pretty, for a country that doesn't know how to build."[34] With the exception of Hagia Sophia in Constantinople, Tournefort opined, no Greek church could hold a candle to the ancient Greek temples, even though the former were dedicated to the worship of "Jesus Christ, instead of the false divinities who were the object of the cult of their [the Greeks'] ancestors."[35]

With a practiced gardener's eye, Tournefort also measured the fields and gardens of Crete and found them wanting. The gardens were disorderly; fruit trees were abandoned; once fertile wheat field had been allowed to lie fallow; half of the olive crop had been allowed to rot for lack of harvesters; rushing streams that could have driven several watermills were left unharnessed; the local gardeners seemed ignorant of the art of grafting, although it had been well-known to the ancients.[36] Viewed with seventeenth-century French eyes, Crete was a once rich land allowed to go to rack and ruin, a failure of civilization to refine wild nature. For visitors accustomed to the formality of the gardens of Versailles, unpruned fruit trees and uncultivated fields suggested sloth and neglect. Whom to blame for this wretched state of affairs? Certainly, Tournefort held the Turkish occupiers partly to account: such matters had been better managed under the Venetians, who had maintained the fruit orchards and wheat fields as well as the ports and walls of the cities. When Tournefort described the state of the garden of the Cadiz in Critza as "à la Turque", he did not mean it as a compliment.[37] But he also pointed an accusing finger at the Greek priests and monks, who were in possession of all the best land, which they then proceeded to neglect: "All the most beautiful properties belong to the monasteries; perhaps that is what has ruined the country, for the monks are hardly fit to maintain an estate."[38]

Tournefort's judgments of the Turks and Cretans were by no means uniformly harsh: he praised the honor of the one and the hospitality of the other. The Cretan wines were excellent; Cretan women pretty if unfashionably attired (at least by Parisian standards); Cretan men skilled hunters and riders; Cretan horses without peer, especially in mountainous terrain. In contrast to barefoot French peasants, there was hardly a person on the entire island who was ill-shod (just as in the time of Hippocrates, Tournefort could not help noting, citing chapter and verse); despite the fact that only half the island was farmed, it still produced enough for export. Yet he

[33] Joseph Pitton de Tournefort (1717), 69–70.

[34] Joseph Pitton de Tournefort to the abbé Bignon (1701), 357.

[35] Joseph Pitton de Tournefort (1717), 136.

[36] Joseph Pitton de Tournefort (1717), 27, 45, 53–54, 61, 185.

[37] Joseph Pitton de Tournefort (1717), 54.

[38] Joseph Pitton de Tournefort (1717), 105–106.

marveled that the descendants of Scyllis and Dipoenis, ancient Cretan sculptors alleged to be the pupils of Daedalus, would stoop to treating marble with chalk; he refused to believe that the labyrinth of Gortyn was an ancient quarry, much less the fabled labyrinth of Daedalus, because it did not meet the high standards of ancient Cretan workmanship.[39]

Ultimately, Tournefort decided, the problem lay with the learned classes, especially the clerics. He explained that the most talented scholars had mostly fled to other Christian domains after the fall of Constantinople in 1453; the priests and monks who remained were ignorant even of their own language and religion. Although he frequently asserted the superiority of French over Greek and Turkish medicine in specific cases, he drew no sweeping conclusions about either the triumph of modern knowledge over ancient or the technological and scientific advances of Western Europe in the past century. By the mid-eighteenth century, Jean d'Alembert could rehearse the historiography of an enlightened age ignited by the scientific sparks struck by Bacon, Descartes, and Newton in the *Discours pré liminaire* (1751) of the great *Encyclopédie*.[40] But this Enlightenment narrative of a great cultural leap forward that separated the west from the rest lay in Tournefort's future. He did not describe himself as an *homme de lumières* nor the progress made in botany and medicine since the mid-sixteenth century as a scientific revolution. The modern Cretans had not been left behind; they had declined from their ancient knowledge and skill—and it was, Tournefort felt sure, the parlous state of the Greek Orthodox clergy that was primarily to blame.

Tournefort's view of the nature and causes of Greek decline emerges most dramatically in his account of how a whole town on Mykonos was terrorized by the malevolent ghost of a murdered peasant. After the burial, residents began to complain of loud noises in the night, of furniture upturned and lamps extinguished. When various exorcism rites performed by the priests failed to tame the *Vroucolacas*, the putrefied body was exhumed, its heart cut out by a local butcher amidst much incense to cover the stink, and burned—all to no avail. The *Vroucolacas* grew more violent, beating people up, shredding their clothes, breaking windows. Families left their houses at night and slept outdoors; some even fled to the countryside. Eventually, after long deliberations between the priests and leading citizens of the town, a liturgy was celebrated, naked swords thrust through the coffin, and the body burned in a grand bonfire on the island of St. George, amidst cries of joy from the townspeople. The demon was pronounced defeated and life resumed its normal course.

Throughout this drama, Tournefort and his companions observed with consternation that everybody was in the grip of what they regarded as a collective madness. "I have however never seen anything so pitiful as the state of this island: everyone's imagination was topsy-turvy [*renversée*]. More intelligent people seemed as struck as the others; it was a true disease of the brain, as dangerous as mania and fury."

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[39] Joseph Pitton de Tournefort (1717), 81–82, 109.

[40] Jean D'Alembert (1751), vol. 1, i–xlv, on xxiv–xxxiii.

Under these conditions, they deemed silence the better part of valor: “Not only would we have been treated as ridiculous but as infidels. How to turn a whole people around?” For Tournefort, the episode of the *Vroucolacas* proved definitively that “the Greeks of today are not the great Greeks” and that the former were enmired in “ignorance and superstition.”[41]

The fault did not lie in their native intelligence, Tournefort believed, but in their want of proper instruction—and not in philosophy or science or medicine but in theology. What could one expect, he asked rhetorically, from people who still adhered to the heresy of that the holy spirit proceeded from the son rather than from the father and who were vague on the location of purgatory and whether the torments of the damned were in fact eternal?[42] Greek Christians had departed from the doctrines of their own faith and with them, Tournefort implied, of their reason.

7.5 Conclusion: The Curetes of Rhea

The story of the *Vroucolacas* was one of the several points at which Tournefort grazed but did not confront the contradictions in his own version of the ancient and modern Greeks. For he knew full well that the ancients, whom he so admired, had worshiped false gods; Theophrastus and Hippocrates had been perfectly innocent of any knowledge of the Holy Eucharist, the approved version of the Nicene Creed, and the geography of purgatory. Other contradictions ran like fault lines through his account: he revered the ancient botanists (and geographers) but did not entirely trust them; he was at once restoring Dioscorides and superseding him. In his early letters to Pontchartrain and Bignon he piqued himself on being able to identify half the plant species in *De materia medica*; by the end of his voyage in 1702, he was touting 1,356 new species discovered—and still more the fact that all these novel species had required only 25 new genera and no new classes, a signal victory for the classification system he himself had introduced into botany (to be in its turn superseded by that of Linnaeus in 1753). He ridiculed the superstitions of the modern Greeks, yet strewed his reports with Homeric references and at one point wondered, citing Ptolemy of Ephesus, whether ruins in Gortyn were perhaps the remains of the temple at which Menelaus had sacrificed to Zeus after the abduction of Helen.[43]

Most arresting of all, he credited the priests and monks whom he so disdained for their ignorance—botanical, medical, theological, literary—with being the living avatars of the “wise Curetes, who held in their heads all the science of their time.” The Curetes were the spirits Rhea had set to guard the infant Zeus from Kronos on Mount Ida and who were also credited with inventing the arts of metal-working. If

[41] Joseph Pitton de Tournefort (1717), 161, 164.

[42] Joseph Pitton de Tournefort (1717), 165.

[43] Joseph Pitton de Tournefort (1717), 70.

Mount Ida had disappointed, the latter-day Curetes did not. Although they had forgotten the uses of the plants described by Dioscorides and Theophrastus and even the language of the ancient sages, they recalled the names of the plants. In fact, Tournefort's informants recalled nothing; the plant names had simply survived more or less unaltered in the vernacular. Yet Tournefort chose to transmute the continuity of language into continuity of memory and tradition. Just as he detected pagan survivals in Greek church rites (comparing them to holy festivals from his native Provence),[44] he fancied that he had caught glimpses of the ancient titans in the diminished moderns.

Tournefort's double vision of the Greeks mirrored his double vision of himself: was he restoring ancient botany or replacing it? Was the plant world of Dioscorides and Theophrastus the center or the periphery of materia medica? The seventeenth-century French debate over the ancients versus the moderns had centered on rival achievements in literature and philosophy[45]; it seems to have left scant trace in Tournefort's framing of his own identity or that of his Greek interlocutors. His dismay over the state of contemporary Greek learning was simply the obverse of his vast esteem for the ancients. He did not bow before them as infallible authorities and did not scruple to criticize and correct them. But they remained towering figures in his pantheon. Like so many classically educated savants of early modern Europe, he thrilled to opportunities for palpable contact with that glittering past: fragments of antique statuary, coins, inscriptions—and living voices offhandedly naming plants in the words of Dioscorides. At such moments, ancient and modern merged, unlettered priests and monks became Curetes, and center and periphery coincided.

Tournefort rarely remarked on how his Greek interlocutors viewed him and his companions, except to note their avidity and gratitude for medical treatments. But he did relate one revealing incident in which the roles of visitor and host, observer and observed, savant and peasant were suddenly if briefly symmetric. Having arrived in a remote mountain village in search of plants, Tournefort and his companions found themselves the objects of intense curiosity on the part of the residents, who had never seen foreigners before. After a moment of mutual scrutiny, both parties exploded with laughter, "they at our manners and clothes, and we at their foolishness."[46] For an instant, the categories of ancient and modern dissolved, and people were simply people.

[44] Joseph Pitton de Tournefort (1717), 135.

[45] Larry F. Norman (2011).

[46] Joseph Pitton de Tournefort (1717), 103.

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Chapter 8
Boscovich in Britain

J.L. Heilbron

Abstract The famous atomic theory invented by Roger Boscovich, which he described as a mixture of metaphysics and geometry, aimed primarily at a reform in the teaching of natural philosophy in Jesuit colleges. The suppression of the Society soon rendered that use moot. The theory lived on, however, and prospered, primarily in Britain. Among the causes of this unlikely success was the removal from the theory of the metaphysical traits that to Boscovich were its main attraction. What is known as the Boscovichian atom is not Boscovich's atom.

Keywords Boscovich, Roger • British atomism • Scottish philosophy • Thomson, J.J.

8.1 Introduction

Roger Boscovich, the most accomplished Jesuit astronomer, geodesist, and optical theorist of the eighteenth century, spent 7 months in England in 1760. He was just 50, and had been professor of mathematics at the Collegio Romano for 20 years. He had no reason to believe that he dwelt on the intellectual periphery of European science. A previous errand beyond the Alps, which had marooned him in Vienna for several months in 1757/8, had only enhanced his self-importance. He had spent his time there among admirers, mainly Jesuits and diplomats, and had completed his theory of everything, *Theoria philosophiae naturalis* (1758, 1763), an attempt to reduce all of physical science to the action of a single force between identical point atoms.

A few months spent in Paris before he sailed to England showed him that he had not dwelt in the van of European science. French mathematicians like Jean d'Alembert were as much in advance of him as he was of his fellow Jesuits. And in physics, the comparison between the apparatus and ability of Jean-Antoine Nollet and instruction in the subject at the Roman College was almost enough to bring a Jesuit natural philosopher to tears.[1] The unpleasant lesson continued across

[1] Boscovich to his brother Bartolomeo, 14 Jan. and 4 Feb. 1760, in Boscovich (2006a), 2: 203, 215.

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the Channel. At Boscovich's first stop in England, the Greenwich Observatory, he discovered that he was no astronomer royal.[2]

The following brief tribute to Kostas Gavroglu's pioneering work on science on the European periphery begins with an account of Boscovich's theory as he may have presented it in England (Sect. 7.2). It survived precariously in writings of Joseph Priestley until it found a home in the 1780s at the University of Edinburgh for reasons alleged in Sect. 7.3. These did not weigh heavily with nineteenth-century scientists, however, and although they often referred to "Boscovich's atom," they seldom designated by it anything that its author would have recognized (Sect. 7.4). The paper concludes (Sect. 7.5) with a reinterpretation of J.J. Thomson's atom with the help of Boscovich's ideas – not to claim that the one derived from the other, but to strengthen earlier assessments of the depth of Thomson's work. Throughout, the great question why the memory of Boscovich, a man in many ways peripheral to British physics, should have been preserved among British men of science, will hover in the foreground.

8.2 The *Theoria*

Walking up the hill through the park below the Greenwich Observatory on his first day in England, Boscovich experienced a sharp shock to his prejudices. The smiling people in the park did not appear to be the puritanical sourpusses of Jesuit mythology.[3] On the contrary, he found, again to his surprise, that being a Jesuit was a lesser disability in London than in Paris. He quickly gained access to leading members of the Royal Society, the principal instrument makers, and an impressive scattering of foreign diplomats and domestic aristocrats. His positive contributions to scientific discussions were confined mainly to optics, especially the theory of the achromatic lenses then newly invented, and to promoting expeditions to observe the upcoming transit of Venus. Soon after his arrival he presented a paper on the transit to the Royal Society, and shortly before his departure he dedicated to it a long poem on astronomy and Newtonian optics. To society in general he gave a quantity of impromptu Latin distiches and epigrams, an art of which he was a world champion, and as much conversation in French and Italian as it could absorb.

Perhaps misled by his welcome, Boscovich had expected to see his *Theoria* acclaimed in Oxford and Cambridge, but, alas, no one in either university had read it. The only Englishman to take an interest in it, John Michell, had devised a similar force-system, but, as he had not grounded it metaphysically, left unplumbed what to Boscovich was the core of the theory.[4] So Boscovich epitomized it in a paper he

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[2] Same to same, 29 May 1760, ibid., 2: 285.

[3] Same to same, 11, 14, and 22 May 1760, ibid., 274, 276, 279.

[4] Same to same, 19 Aug, 6 and 20 Nov 1760, ibid., 353, 389–90, 400; Feingold (1993), 517–18; McCormmach (2012), 57–68.

gave to Mitchell just before leaving England.[5] This interesting document, apparently now lost, would have associated the "strikingly sublime and noble idea" elaborated in the *Theoria* with the metaphysical principle or adage that Nature makes no jumps.[6] Considering elastic collisions from this point of view, Boscovich made the elements of his system points without extension, matter, or substance, and ascribed rebound in elastic collisions to a repulsive force between the points that increases to infinity as the distance between them diminishes to zero.[7]

The "sublime and noble idea" was to admit only one kind of point atom, or, rather, only one kind of force pattern, which belongs to points taken in pairs and can best be pictured as a curve crisscrossing a line that represents the distance between them. This strict monism, with its explicit reference to Leibniz's monads, is an essential ingredient of Boscovich's system and a main reason that it fails in application. To obtain the richness of nature from his drab ingredients, he supposed that different stable configurations of the ultimate atoms could present themselves as stable particles with specific chemical and physical properties. In practice, he had to infer the little he could say about the elementary force pattern from the physical properties of matter in bulk.[8]

The editor of the first serious modern book to bring Boscovich's ideas to an Anglophone readership saw seven original features in the *Theoria*. Firstly and secondly, the *point centers* or *puncta* are finite in number and identical in all properties but position. Thirdly and fourthly, they either lack or relativize the usual *mechanical quantities*: the mass of a particle is merely the number of its *puncta*, and its inertia arises from the resistance to its motion offered by the mutual repulsion between itself and its neighbors. Finally, the *force law* is unique, implies the existence of natural lengths in the ratios of the universal force constants, and represents an observable (itself) as a power series.[9] These were probably also the features that Boscovich recommended to Mitchell. They are not, however, the main content of the *Theoria*, which consists largely of elaborations of the mathematical properties of the force curve and qualitative applications of them to standard pieces of natural philosophy.

These applications in effect translated received physics into the language of point atoms so effectively that Boscovich could not point to a measurement or experiment that argued the superiority of his system over its rivals. For, despite the geometry and algebra with which he adorned it, the agreement he produced was merely rhetorical. He had no way to deduce from his force curve the places where two interacting particles cohere or combine chemically. When more than two point particles were in play, the number of possible equilibrium positions became, as Boscovich wrote, apparently in satisfaction, "incredible." "It is marvelous what a

[5] Boscovich to his brother, 20 Nov 1760, in Boscovich (2006a), 2: 402.

[6] Boscovich (1966), 6–7.

[7] Ibid., 27–9 (§§30–38), 40 (§§73–75).

[8] Ibid., 21 (§9); cf. Marković, in White (1961), 146.

[9] White, in White (1961), 119.

huge number of different laws arise [in the general case]....The calculation would be enormous."[10] In short, the *Theoria* was not an attempt at a mathematical physics but a mixture of "metaphysical and geometrical things."[11]

Even if it were known in detail, the unique law would not explain the world as we find it. That is because we are not aware of the circumstances in which it began. Only God knows why He chose the initial conditions. Nothing in the world, before or after its creation, constrained Him.[12] When he conceived his noble idea, Boscovich was close to despair over the state of Jesuit teaching of natural philosophy, and intended the *Theoria* as a corrective to its obsolescence. Thus his *Theoria*, despite its universal applicability, might best be interpreted as a parochial attempt to make the metaphysics of the systems of Leibniz and Newton compatible with God's freedom and the constraints of the Jesuit education system.[13]

8.3 Father Boscovich's Atom

Priestly probably became acquainted with Boscovich's point atoms while consulting Mitchell during the writing of his history of vision, light, and color.[14] He used them chiefly to disarm the objection that light particles as conceived by Newton would bang into one another in space and bounce off crystalline bodies. Priestley smudged Boscovich's picture slightly by making repulsion at the force centers finite so that light particles aimed directly at them could pass through them. In justification of this departure, Priestley argued, following Michell, that we are free to characterize the ultimate constituents of matter as we see fit. With this authorization, Priestley departed still further from the *Theoria* by assuming the existence of different sorts of atoms associated with various distance laws.[15]

These serious derogations were peccadilloes in comparison with Priestley's exploitation of Boscovich's sublime idea to support a materialistic anti-Trinitarian worldview. He argued that if matter cannot exist without the active powers of attraction and repulsion, and if, being everywhere penetrable, it has no solid parts, it has no qualities that distinguish it from mind. And if mind includes soul and soul is spirit and both are body, Christ's soul could not have existed before his mind or his matter.[16] Although nothing could be clearer, Priestley added a précis of Boscovich's theory to help slower readers appreciate the relationship between the

[10] Boscovich (1966), 49 (§100, 102), 81 (§209), quote.

[11] Boscovich to his brother, 14 Jan 1760, in Boscovich (2006b), 2: 203, and to G.S. Conti, 26 Apr 1760, in ibid., 5.1: 24.

[12] Boscovich (1966), 55–6 (§§124–6), 195–6 (§§555–6).

[13] Cf. Baldini (1993), 85, 88–93, 99–102, 105–9, and (2006), 405–8.

[14] Priestley (1970), 95–6; Schofield (1970), 242–8.

[15] Priestley (1772), 383, 390–4.

[16] Priestley (1777), ii–v, 11–13.

penetrability of matter and the vapidity of the Trinity.[17] Boscovich of course exploded at being made an accomplice in spreading the "abominable, detestable, and impious" doctrine of materialism.[18] In time Priestley came to see his accomplice as a liability. As an honorary citizen of the Republic of France, he tried to bend its leaders from atheism to his own Unitarian faith, an undertaking with little enough chance of success without a Jesuit among its theological authorities.[19]

8.3.1 The Scottish School

Like Priestley, the Scots who transmitted Boscovich's theory to the nineteenth century took it up owing to much wider concerns than matter theory. The principals were the representatives of Common Sense Philosophy at the University of Edinburgh, Dugald Stewart and John Robison, the professors of moral and natural philosophy, respectively.[20] The agreement between Boscovich's *Theoria* and Common Sense, which may not appear obvious, extended to three points: securing foundations by very general axioms neither demonstrable nor refutable, "impossible to disbelieve and impossible to prove;" suspicion of claims to know the world as it is; and enthusiasm for geometry as the finest file for honing the human mind. This last merit was particularly important for Boscovich's reputation among the Scots. He was a past master of geometry, in both senses of the word – elegant and outmoded. In geometry as well as in metaphysics and methodology, he displayed for Stewart that "rare blend of imagination, and of the reasoning powers [in which] the perfection of the human intellect will be allowed to consist."[21]

The great champion of Boscovich at Edinburgh, Robison, very probably became acquainted with the *Theoria* independently of Priestley, perhaps during his travels and sojourn in Russia, where he acted briefly as an instructor of the tsarina's cadets; for the "abbé" (as Boscovich styled himself after the suppression of his Order) enjoyed a high reputation in Eastern Europe.[22] The form of the unique force would have made a good subject of conversation between Robison and his friend F.U.T. Aepinus, the Petersburg academician who first reduced Franklin's system of plus and minus electricity to distance forces acting between particles of the electric fluid and between them and material atoms.[23]

In a remarkably long article on Boscovich in the supplement to the third edition of the *Encyclopedia Britannica* (1801), Robison set forth some of the

[17] Ibid., 24–8 (précis of Boscovich), 34–40.

[18] Priestley to Boscovich, 19 Aug 1778, in Priestley (1966), 166–7.

[19] Priestley (1793), 4, 24.

[20] Olson (1969), 92–4.

[21] Stewart, quoted by Olson (1975), 105–6.

[22] Baldini (2006), 418–23; Robison (1790), 83; Playfair (1822), 4: 156.

[23] Cf. Robison to Watt, Dec 1796, in Robinson and McKie (1970), 248.

epistemological merits of the *Theoria*. Above all, it explodes the prejudice that impulse is the only intelligible cause of the production of motion. We have no idea how impulse works and, by making it a first principle, run into obscurity even unto wickedness. Yes, wickedness. It is "arrogant" to set up as judges of first principles. That applies even to Boscovich's principles. But although the most that can be claimed for them is that they might be ingredients of a true theory, they have the present merit of completing the Newtonian vision, which is as close to the truth as we are likely to come.[24]

That vision revealed an astonishing feat of divine geometry in the stability of the solar system under gravity. "Cold, we think, must be the heart that is not affected by this beneficent wisdom in the Contriver of [this] magnificent fabric. . ..And he must be little susceptible of moral impression which does not feel himself highly obliged to the Being who has made him capable of perceiving this display of wisdom."[25] The same high moral sense pervades the *Theoria*, which ends in an Appendix on God. It was an additional, powerful recommendation of Boscovich's great work to his pious Presbyterian interpreter.[26]

With its invocation of God, impenetrable atoms, and distance force the *Theoria* elaborated standard Newtonian themes. Robison tightened the connection and broke with Boscovich's metaphysics by replacing the point centers with Newton's unbreakable kernels. Robison's common sense prevented him from conceiving a force attached to a point in space, or powers without a "substance to which they belong, to which they are related." How can one bundle of attractions and repulsions put another in motion? "These are words without ideas."[27] If Boscovich's theory thus diluted is just Newton completed, then why father systematic Newtonian force physics on Father Boscovich? The question gains force by recalling that Boscovich claimed to have integrated Newton's atoms with the monads of the arch-fiend of Newtonian physics, Leibniz. Robison explained that the credit could not go to any of the English Newtonians because they had not propagated the theory "chastely," that is, geometrically, and so "brought it into discredit."[28]

There is another reason. The Newtonian tradition included Priestly and, more significantly here, Laplace, whom historians credit with the first successful quantitative employment of short-range forces to physical phenomena. Robison did not care to admit either to the canon. Obsessed with the French Revolution, he placed Priestley and Laplace prominently among the enemies of humanity. Priestley had prepared the "minds of his readers for Atheism by his theory of the mind" and promoted the "detestable doctrines of Illuminism." Laplace had done brilliantly in

[24] Robison, in *EB*^{3}:*Suppl*. 1: 103a, 748b, 790a (quote), and (1822), 1: 267, 294, 297–8.

[25] Robison (1804), 434–5.

[26] Robison, in *EB*^{3}:*Suppl*. 1: 106–7 (art. "Boscovich").

[27] Robison, *System* (1822), 4: 292, 299 (quote).

[28] Robison, in *EB*^{3}:*Suppl*. 1: 790b (art. "Impulse"); cf. ibid., 1: 103n ("Boscovich"). For the Newtonian force tradition, see Schofield (1970), 37, 39, 238–9, 251–2; Heiman and McGuire (1971), 261–304; Heimann (1971); Cantor (1983), 71–2; Harmon (1993)

epitomizing Newton's *Principia* in his *Système du monde* (1796), but he had ended his exposition with a parody of its culminating lesson and deduced from the undistinguished place of the abode of humankind on a third-rate planet around a second-rate star that our belief in our special status is a conceited illusion. [29]

Robison's version of Boscovich reappeared posthumously in the fourth edition of the *Britannica* (1810) modified by the editor, Bishop George Gleig. Boscovich was now a miracle of mathematics and humility, "acceptable to every friend of humanity, and to every cultivator of science," although, like all mere mortals, unable to get to the bottom of things.[30] We must reject the concept of immaterial matter. But although the *Theoria* failed in the "very sublime attempt" to lift "interior veil of the temple of nature," it cannot be considered a failure. After all, its inspired author managed to go beyond Descartes, Leibniz, and Newton.[31]

Robison's student and successor John Leslie held even more strongly than his teacher to the ascendency of geometry over algebra and fought more tenaciously against the multiplication of hypotheses. As late as 1824 he inveighed against analysis for ending in "paradox and misconception" (he mentioned the square roots of negative numbers), and at all times he decried theories invoking invisible agents other than his own.[32] He professed to accept the "leading principles of the very ingenious. . .profound philosopher and elegant geometrician" who had hit on the "happiest and most luminous extension of the Newtonian system;" but, being modern and anti-clerical, he criticized the *Theoria* as outmoded in physics and larded with "obscure disquisitions. . .contain[ing] only the sort of antiquated metaphysics that savours of the theologians."[33]

The overtones in Boscovich's metaphysical geometry that had sounded most harmonious to Robison likewise died away in the writings of his student Thomas Thomson. To be sure, Thomson reckoned it "within propriety" to refer anyone wanting to know the cause of the diverse degrees of chemical affinity to "the study of Boscovich's curve," and so echoed one of Robison's notes to Joseph Black's *Lectures on chemistry*; but, contrary to Robison, Thomson did not think that chemists would gain much from the effort.[34] "We cannot help thinking it rather improbable (if it be possible) that two such opposite properties [as attraction and repulsion] should exist together."[35] Thomson's view of the likely profit to chemists of adoption of Boscovich's atom received wide dissemination not only in his own

[29] Robison (1798), 180–4 (Laplace), 329, 367, 369 (Priestley); Morrell (1971), 43, 51. Cf. Heilbron (2011), 214–228. Robison was not always faithful in his renderings of Laplace; James Keir to Watt, 24 Nov 1797, in Robinson and Mckie (1970), 283–5.

[30] Robison and Gleig, EB^4, 2: 42a, 45a, 46a (quote), 51–3, 54b–55a, 57–8, 59a.

[31] Ibid., 55a, 59a.

[32] Olson (1971), 40–1, and (1975), v–vi, 23, 211–12.

[33] Leslie (1804), 114–25, 515; cf. Morrell (1975), 69–71, 76–7, 78.

[34] Thomson, in EB^3:*Suppl*. 1: 342; Robison, in Black, *Lectures* (1803), 1: 515, 519–20; Robison to Watt, 30 Jul 1784, and 14 Jan 1798, in Robinson and Mckie (1970), 387, 285–6.

[35] Thomson (1801), in EB^3:*Suppl*. 1: 342, 260nD (art. "Chemistry"); cf. Feingold (1993), 522.

extensive writings, but also in a notice placed in the *London, Edinburgh and Dublin Philosophical Magazine* in 1805 by its founding editor, the Scot Alexander Tilloch. He warned that Boscovich's atom, which he understood to be a material kernel bearing a complicated distance force, could "convey but little information to a man who merely aims at an understanding of the properties of matter."[36]

8.3.2 Throwbacks

Like Boscovich, William Rowan Hamilton, the Astronomer Royal of Ireland, was a mathematician, astronomer, and poet who developed his mathematical physics with an eye to metaphysics rather than application and ascribed great importance to the theological and epistemological implications of his theories. And, like Robison, he admired Laplace's mathematics and deplored his atheism. "Beware of assuming as certainly true those Laplacian views of nature, which are indeed most current now [1835], but which ought. . .to be replaced by very different and far more religious views." Hamilton's interest in poetry and idealist philosophy brought him close to Samuel Taylor Coleridge, whose advice he solicited when he began to incorporate atoms into his physical theories. The concept that best fit Hamilton's requirements was "nearly that of Boscovich." Hamilton described it to Coleridge, whom he supposed to be ignorant of it, as a representation of the "phenomena of motion as produced by the action of localized energies," the loci being mathematical points, "[in] possession of, or connexion with, physical properties and relations."[37]

Hamilton had his Boscovichian theory in mind when writing his celebrated memoir, "The general method in dynamics," published in 1834 but long in development.[38] There he expressed the laws of a world that, although a "metaphysical idealization," he expected to find mirrored in nature; for, owing to the activity and contrivances of the "Supreme Spirit" that excites our perceptions, the ideal world of our thought corresponds to the phenomenal world of our senses. Recent work on crystal structure and the ether by French mathematicians seemed to Hamilton to confirm the existence of point atoms and diverse distance forces. "I am well aware how perfectly coincident [Boscovich's ideas] are on the physical side with those adopted by the great modern analysts."[39] This was to be too generous. The analysts did not require or develop the unique force, but only, in Laplace's manner, such forces as they needed for their calculations. By this misrepresentation, Hamilton perpetuated the British practice, recommended by the *Theoria*'s treatment of

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[36] *Philosophical magazine* 22, 141.

[37] Hamilton to E. O'Brien, 1835, in Graves (1882), 2: 398, and to Coleridge, 3 Oct 1832, ibid., 1: 593.

[38] Hankins (1980), 157, 166, 181–2.

[39] H.F.C. Logan to Hamilton, 31 May, and reply, 27 June 1834, in Graves (1882), 2: 85–8.

natural theology and metaphysics, of taking Boscovich rather than Laplace as the eponym for point atoms and distance forces.

Hamilton did not live on abstractions alone. He breakfasted, and one morning did so with Michael Faraday. He found to his surprise that Faraday shared his anti-materialism. "More and more the conception of matter [was becoming] an encumbrance and complication [to Faraday] in the explanation of phenomena, instead of an assistance." He preferred to think in terms of "a transference of power."[40] What Faraday may have owed to Boscovich's ideas in his progression from hard Daltonian atomist to soft field theorist is a matter of conjecture.[41] Although we know from Hamilton's breakfast news that Faraday entertained Boscovichian ideas by the 1830s, the golden text testifying to his adherence did not appear until 1844, in a "speculation" introduced to explain the difference between conductors and insulators. Faraday saw a solution in Boscovichian atoms whose powers, spread through space, determine the properties of bodies.[42]

Faraday's favorable remarks about point atoms were not a ringing endorsement but the expression of a preference for what, in 1844, he took to be the least objectionable representation of matter available. Boscovich's approach had the advantage not only of annihilating the obnoxious duality space-matter, but also of establishing a precedent against the acceptance of Newton's deduction, from one of his rules of philosophizing, that atoms must have hard extended cores.[43] In a key manuscript, perhaps dating from the time of resolution of his conundrum about conductivity, Faraday argued that it made no sense to suppose hard kernels as force carriers in the Newtonian manner. A nucleus without properties, without powers, presents no idea of anything. Who needs it? "God could. . .just as easily by his word speak power into existence around centres, as he could first create nuclei and then clothe them with power."[44] Faraday soon progressed beyond Boscovichian pictures to silhouettes of lines of force. Perhaps he had never invested much more in the pictures than in the "ordinary hypothesis of solid impenetrable molecules." Neither was "anything more than a contrivance," offering, at best, "deep, if insecure and partial insights into natural phenomena."[45]

[40] Hamilton to his sister, 30 June 1834, in Graves (1882), 2: 96.

[41] Agassi (1971), 80-1n, and, especially, Williams (1965), 88, 126–7, 279–82, summarized for criticism by Spencer (1967), 184–7.

[42] Faraday (1844), 136–44. Seven years earlier the editors of the *Philosophical magazine* had felt obliged to refer its readers to Priestley's description of Boscovich's atomism, assuming, apparently, that they did not know its basic assumptions. Williams (1965), 294–6.

[43] Cf. Agassi (1971), 84, 323, and Cantor (1991), 173. The rule states that the general properties that we observe in the bodies surrounding us must be assumed to belong to all matter whatsoever.

[44] Quoted by Levere (1968), 105–7.

[45] These views are recollections of Faraday's opinions expressed in Benjamin Brodie to Faraday, 14 Jan 1859, in Faraday (1871), 2: 920, where Brodie is given incorrectly as "Broost;" Cantor (1991), 2ll (last quote).

While the influence of the *Theoria* on Faraday's scientific work may be difficult to demonstrate, its resonance with his religious views is plain enough. An example is the conclusion of lectures on chemical affinity that he delivered in 1847:

> Our philosophy, feeble as it is, given to us to see in every particle of matter, a *centre* of force reaching to an infinite distance; binding worlds and suns together . . . Around this same particle we see grouped the powers of the various phenomena of nature . . . until at last the molecule rises up in accordance with the mighty purpose ordained for it, and plays its part in the gift of *life itself*. And therefore our philosophy, whilst it shows us these things, should lead us to think of Him who has wrought them.[46]

With this hymn, from a devotee of an obscure austere Protestant sect, the fortunes of the natural philosophy composed to modernize the thinking of the Society of Jesus peaked in Britain.

8.4 Boscovichian Atoms

"A labyrinth of Daedalus!" Thus did Thomas Young dismiss Boscovich's "speculations on the fundamental properties of matter, and the general laws of the mutual action of bodies on each other." The more intricate and speculative Boscovich waxed, the more "inaccurate and superfluous" his theory appeared to the prudent Quaker Young.[47] Charles Hutton, whose *Philosophical and mathematical dictionary* (1795, 1815) was consulted widely, had no more patience with Boscovich's theory than Young did. Hutton does not mention the atom under his entry "Boscovich;" under "Cohesion" he criticizes "some philosophers [who] have positively asserted that the powers, or means, are immaterial, by which matter coheres," and points to Michell and Boscovich as Priestley's sources for amalgamating spirit and matter. Under "Matter" he rejects Boscovich's approach altogether. "[A]ll power is the power of something; and yet if matter is nothing but this power, it must be the power of nothing, and the very idea of it is a contradiction."[48] The Boscovichian atom must have a kernel to have a kernel of truth. And even then it might not do for a clear-headed Englishman. "The most ingenious baseless fabrick that ever was reared is, in my opinion, [it is the opinion of a president of the Royal Society], that constructed by Boscovich."[49]

The most judicious assessment of Boscovich in Victorian Britain occurs in the *Britannica*, still friendly to him in its ninth edition (1875). In its article "Atom" James Clerk Maxwell collected and expanded his earlier statements about the

[46] Quoted in Levere (1971), 77. Cf. ibid., 96–102.

[47] Quoted from Young's notebooks by Cantor (1983), 134, and from his *Course of lectures in natural philosophy and the mechanical arts* (1807), 1: 751, by Feingold (1993), 522.

[48] Hutton (1815), 1: 22–3, 331–2, and 2: 25–6.

[49] Gilbert ("Giddy") Davies to Charles Daubney, Professor of Chemistry at Oxford, in Levere (1971), 96–7.

nature of matter. In one of these, an address to the British Association in 1870, he had intimated that a generation earlier, when he entered the University of Edinburgh and Faraday was growing point atoms into lines of force, the task of writing about atomic theory belonged to the metaphysician, not, as in 1870, to the scientist.[50] The change was worked by the kinetic theory of gases, which, by delivering good estimates of the number and size of molecules, gave them the solid existence that to some minds only measurements can vouchsafe. Further studies of the gaseous state indicated that molecules exert no sensible force on one another unless they come very close together, where an attraction sets in followed by a repulsion that might give way to another attraction before the final repulsion that blocks complete intimacy. "[This is] quite in accord with Boscovich's theory of atoms." But not in its metaphysical version. "These attractive and repulsive forces may be regarded as facts established by experiment, like the fact of gravity, without assuming either that they are ultimate facts or that they are to be explained in a particular way."[51]

In "Atom," Maxwell presents Boscovich's theory, correctly, as purely monadic, and places it at one extreme of a metaphysical spectrum whose other end is occupied by the plenum of Descartes. On this understanding, Boscovich's *Theoria* is the culmination of the Newtonian tradition beginning with the editor of the second edition of the *Principia*, Roger Cotes.[52] Picked clean of metaphysical detail and historical accuracy, however, Boscovich's rich atomistics supported only the instrumental recourse to "new laws of force to meet the requirements of each new phenomenon."[53] Maxwell gave a brilliant example of Boscovichian opportunism by supposing an inverse fifth-power repulsion between molecules in a gas in order to eliminate velocity from a key integral in which it turned up to the power $(n-5)/(n-1)$.[54]

Lord Kelvin frequently mentioned what he called Boscovich's theory, initially negatively, as "a strange idea…barren tree…unavailing labour."[55] Later he invoked Boscovich's name in calculations about crystals, but these often concerned forces that extended only to nearest neighbors in the lattices and implied the existence of other sorts of forces acting beyond the crystal boundary. This was merely to salvage the flotsam from a sunken galleon. "[W]e have long passed away from the stage in which Father Boscovich is accepted as being the originator of a correct representation of the ultimate nature of matter and force."[56] Thank

[50] Maxwell (1890), 2: 221.

[51] Maxwell (1890), 2: 412 (first quote), and, referee report, 1876, quoted in Harmon (1998), 184. Maxwell (1890), 2: 457–61, describes ways of computing atomic dimension.

[52] Maxwell (1890), 2: 448–50 (art. "Atom"), and ibid. 316 ("Action").

[53] Ibid., 448, 214, and 471, resp.

[54] Ibid., 29, 41, 71.

[55] Kelvin, cited by Merz (1906), 1: 358n, from a text of 1860.

[56] Kelvin (1904), 123 (quote, text of 1884), 653–5, 645–6 (1893), 667–8 (1902). Thomspon (1910), 2: 889, 893, 1052, 1076–7, 1080.

goodness! Boscovich's "brilliant doctrine (if infinitely improbable theory)" exemplified the impediments that Faraday and Maxwell had to overcome to establish their field theory. In Kelvin's historiography, "Boscovich's theory [was] so unqualifiedly accepted as a reality, that the idea of gravitational force or electric force or magnetic force being propagated through or by a medium, seemed as wild to the naturalists and mathematicians of 100 years ago as action-at-a-distance had seemed to Newton and his contemporaries."[57] From which it appears that Kelvin's idea of "Boscovichian" differed little from Laplace's idea of "Laplacean."[58]

Kelvin's friend John Theordore Merz, philosopher, chemical entrepreneur, and literary executor of the nineteenth century, reviewed Boscovich's place in science with his usual accuracy. The *Theoria* was the "most celebrated attempt" to bring Newton's celestial mechanics to earth. It is not physics, however, but metaphysics. As a "purely metaphysical theorist," Boscovich had ignored Newton's reservations about distance forces, and, in anticipation of the Laplacean school, had tried to build molecular physics on shaky analogies to the gravitational theory.[59] Merz noted that, despite the parallel to Laplace, the French mentioned Boscovich only rarely and then unfavorably; that the Germans appear never to have heard of him; and that the British owed their familiarity with his name if not his doctrines to Priestley, Stewart, and Robison.[60]

Merz observed further that his friend Kelvin had managed to indicate how to calculate the stability and saturation of chemical molecules using Boscovichian concepts. Kelvin did outline a chemistry that might be considered opportunistically Boscovichian, but, significantly, he attributed it to Aepinus. Merz does not mention this theory, "Aepinus atomized," which Kelvin put together in 2 weeks of hard work in 1901, at the age of 77.[61] It supposes the electrical fluid to be made up of "electrions," "exceedingly minute and similar atoms...much smaller than the atoms of ponderable matter." The big ponderable atoms, supposed spherical, act on electrions outside them by an inverse-square force, and on those inside (Kelvin supposes them to be perfectly penetrable) by a direct-distance force. These assumptions in effect picture the atoms of matter void of electrions as uniform incompressible permeable spheres of positive electricity. Combined with electrions they have different properties that might mimic different chemical behaviors. "[B]ut it is possible that the differences in quality are to be wholly explained in merely Boscovichian fashion by differences in the laws of force between the atoms." "[A]s we are assuming the electrions to be all alike, we must [again] fall back on

[57] Kelvin (1893), in Hertz (1893), xi (quote), xv.

[58] Ibid., xi. Cf. Boscovich (1966), 179–80 (§507), on heat, and the parallel passage on electric fluid, 181 (§511).

[59] Merz (1906), 1: 357, 358n, 371n; 2: 29, 351n.

[60] Ibid., 1: 359n.

[61] Ibid., 1: 358n; Kelvin to Stokes, 4 Oct 1901, in Wilson (1990), 2: 751. Boscovich is not mentioned in the correspondence between Kelvin and Stokes printed by Wilson, some 656 letters over 55 years.

Father Boscovich, and require him to explain the difference in quality of different chemical substances, by different laws of force between the different atoms." [62] It need hardly be said that Boscovich would have repudiated the promiscuous multiplication of force species as vehemently as he did the application of the *Theoria* to materialism.

8.5 Thomson's Atom

Kelvin's freely penetrable, homogeneous spheres of positive electricity became the foundation of the earliest fruitful atomic model when, in 1903, J.J. Thomson adapted it as the home for his "corpuscles." Like electrions, corpuscles were what most physicists by 1903 called "electrons." In this first version of his model, Thomson supposed that atoms consist of electrical doublets, the negative end being much the more massive. This was a consequence of the electromagnetic theory of mass, which Thomson pioneered, and which makes the amount of ether dragged by an accelerating charged particle inversely proportional to its radius. When many such doublets congregate, we may imagine the ensemble to "resemble the Aepinus atom of Lord Kelvin." Having performed this feat of imagination, Thomson supposed that the great many corpuscles required to make up most of the weight of the atom circulated in concentric rings. To the obvious question, why does the lightest atom, hydrogen's, contain 1,000 corpuscles, he answered ingeniously that all lighter ones have disappeared owing to the more rapid loss of energy through radiation by atoms with fewer electrons.[63]

There was a strong if unexpressed and perhaps unintended reference to Boscovich in Thomson's model. The penetrable positive sphere that defined the atomic volume and confined the circulating atomic electrons had no analogy in electrical phenomena. Thomson did not like it and hoped to do without it, indeed, "without positive electricity as a separate entity." That would bring him back to Aepinus' single fluid and require replacing the positive spheres by "some properties of the corpuscles." Apparently, when assembled within tight enough dimensions they acted on one another as if the space they occupied had the properties of Kelvin's positive spheres, which, properly regarded, were merely a "way of picturing the missing forces which is easily conceived and lends itself readily to analysis."[64] These missing forces, a property of electrons in constrained quarters unknown to electrodynamics, might be considered Boscovichian.

Resort to Boscovich took another turn with Thomson's discovery that the number of electrons in an atom is insufficient to account for atomic weight. That made representation of the positive component by Boscovichian forces implausible.

[62] Kelvin (1904), 540–3 (text of 1901); cf. Wilson (1993), 601–13.

[63] Thomson (1904), 48–51, 92, 98 (quote), 103–4.

[64] Thomson to Oliver Lodge, 11 Apr 1904, in Rayleigh (1942), 140–1.

Thomson proposed another use for them. The form of the laws of spectral series indicated that the frequencies of the lines in an element's spectrum could not be overtones of the frequencies of the orbits he supposed the electrons to describe. A possible solution was to restrict electrons within an atom to discrete stable orbits in which they could radiate at frequencies not related harmonically. Thomson suggested two ways to achieve the restriction.[65]

The first way, which need not be considered here, rested on an analogy to the three-body problem in astronomy. And the second way? "Suppose we regard the charged ion as a Boscovichian atom exerting a central force on a corpuscle which changes from attraction to repulsion several times between the ion's surface and a point a distance from the surface comparable with molecular distances." Then at the distances from the center where repulsion changes to attraction a corpuscle launched at right angles to the line connecting it to the center would describe a circular orbit. The angular frequency of oscillation of the electrons around such an orbit can be expressed by the parameters in a Boscovichian force law.[66] This model, for which Thomson provided a version of Boscovich's famous curve representing force over distance, resembled the *Theoria*'s picture of the elliptical motion of a point atom controlled by fellow points anchored at the ellipse's foci.[67]

In his steps toward what became his quantum atom, Niels Bohr followed Thomson's atomic project closely.[68] If Bohr followed Thomson, and Thomson took his atomic model "directly from the theory and curve of Boscovich, and showed that a notion of 'allowed' and 'forbidden' orbits follows from it," any novice in Jesuitical inference could work out that Bohr's quantum atom is a direct descendent of Boscovich's *puncta*.[69] This ingenious suggestion suffers from the objection that there is no evidence for it and much against it. Half a century after Bohr's great invention, he had occasion to look into the *Theoria* for something to say at a meeting held in Zagreb to celebrate its 200th anniversary. Bohr judged that Boscovich had started the "general mechanistic views which inspired Laplace, and, perhaps less directly, Faraday and Maxwell." If Bohr thought that he stood in this line, the Zagreb celebration was the place to say so. But he could manage only a formal statement reversing the direction of influence: recent physics had discovered the historical relevance and moral value of the *Theoria* after arriving independently at a summit from which it could spy Boscovich.[70]

It is not possible in the present state of psychology to know what pieces of the mental furniture of a discoverer or inventor figured in the process of creation. The historian may judge on the basis of the written record that a particular furnishing did not play a significant part in the discovery or invention; but in the end the judgment

[65] Thomson (1907), 156–60.

[66] Ibid., 160–1.

[67] Boscovich (1966), 91–2 (§§230–2)

[68] Heilbron and Kuhn (1969), 223–4, 245–52.

[69] Gill (1941), 26–8.

[70] Bohr (2007), 104–5, 517, 23–4 (text of 1958/9).

rests on the historian's feeling for the plausible. It would be rash to rule out a contribution of Boscovichian theory to the thought of Faraday, Hamilton, and Thomson, and rash to assert it for Laplace, Maxwell, and Bohr.

The persistent association of Boscovich's name with a certain model in Britain but not on the Continent is a problem of a different order. We can trace the association back to Priestley and the Scottish school, especially Stewart and Robison, and find persuasive reasons for their attachment to it in their pedagogy, metaphysics, and religiosity. The Edinburgh philosophers and mathematicians kept Boscovich's name alive in their teaching and labeled with it the approach that their continental counterparts would call French or Laplacean. This practice, which foisted on Boscovich an opportunism foreign to his beliefs and objectives, may be related to the fear and disgust that ungodly *philosophes*, among whom Robison placed Laplace, awakened in Britons terrified by the imperialism of the French Revolution. Consequently, an invocation of "Boscovich's atom" by a nineteenth-century scientist may refer to nothing deeper than an arbitrary distance force chosen for its mathematical convenience.

Scientists who so bowdlerized Boscovich took only the outer coat, or periphery, of the theory of the *Theoria*. The core, the metaphysics, geometry, and theology, remained largely where he had placed it, at the head of a reform of Jesuit teaching of natural philosophy. When the man from the periphery – from Ragusa, which he knew was not the center, and Rome, which he discovered to be eccentric too – reached the undisputed center of eighteenth-century science, France and England, his reform package excited as little sympathy as efforts to save the Jesuit Order. But it had the right ingredients to fire up the University of Edinburgh. From Scotland, which was geographically though not scientifically peripheral, Boscovich's atomic theory reentered England in encyclopedic epitome and gained wings by dropping its metaphysical weight. By World War I, "Boscovichian atom" had become a catch phrase, like "Newtonian gravity" and "Laplacean determinism," with little connection to its carefully formulated original teaching.

When a home-town boy makes good his neighbors usually, and their descendants often, magnify his importance. In a country on the outskirts, the magnification can grow in direct proportion to its distance from the center and in inverse proportion to its size. Thus we read in a tribute to Boscovich by one of his fellow citizens, "The smaller our country and power, the greater the extent of our wisdom and virtue." The writer observed further, and correctly, that Boscovich's reputation would "spread and exalt the name of Ragusa."[71] Some more recent enthusiasts, not content with indications of Boscovich's possible influence on English physicists from Faraday to Thomson, have attributed to him anticipations of quantum statistics, nucleonic structure, and Heisenberg's universal length.[72] The Matthew Effect,

[71] Zamagna (1787), xi.

[72] Tadić, in *Philosophy* (1987), 126; Šlaus, ibid., 111; White (1961), 106, 124 (quotes); Gill (1941), 27–8, 31–47; Martinović (1988), 211–12.

"Unto every one that hath shall be given" (Matt. xxv.29), works on the periphery as well as at the center.

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[73] *EB*^{x} is used here and in the notes to signify the xth edition of the *Encyclopedia Britannica*.

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Chapter 9
Neo-Hellenic Enlightenment: In Search of a European Identity

Manolis Patiniotis

Abstract The Neo-Hellenic Enlightenment of the late eighteenth century is a local version of the Enlightenment associated with the contact of the Greek society with the European philosophical and political thought. According to the received historiography, the exposure to the ideals of the Enlightenment consolidated the Greek national consciousness and gradually led to the great national uprising against the Ottoman rule. The aim of this chapter is to discuss the historical and intellectual circumstances under which this perception was constructed and the implications such a local historiographic enterprise might have for the Enlightenment studies at large.

Keywords Constantinos Dimaras • Europe • Adamantios Korais • Modernism • Neo-Hellenic Enlightenment • Costis Palamas

9.1 The Historiographic Problem

The question "What is Enlightenment?" is one of the most frequently asked questions in the history of philosophy. Many historians and philosophers of the modern era spent time and intellectual energy considering it. However, the variety of the answers and the wide range of qualities attributed to the Enlightenment indicate that by asking this question and answering it in a certain way, each social formation did not actually aim at retrieving the true nature of *the* Enlightenment. It

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I have been working with Kostas for more than 15 years. The topic of our joint venture is the history of Greek science during the seventeenth and the eighteenth centuries. One important lesson I was taught by Kostas is that a historian should be equally concerned with historical facts *and* with the political decisions informing the received historiography. It is from this particular view that his (our) interest in the history of Neo-Hellenic Enlightenment emerged. As is, hopefully, shown, the construction of this concept represents an important chapter of the Greek national historiography. And although the ideas put forth in this chapter may slightly diverge from Kostas's perception, I must say that they are the outcome of our common work and our long exchange on all aspects of the matter.

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rather aimed at producing a certain *image* of the Enlightenment, which reflected the particular version of modernity this formation represented. In other words, it produced a self-representation. Of course, it is quite common for the same social context to produce different and conflicting versions of the Enlightenment, but this is only a measure of the antagonisms permeating this context and the diverging priorities of the social actors (Hunt and Jacob 2003).

During the Enlightenment the self-representation and the definition of the Enlightenment coincided. The *philosophes* defined themselves by presenting their time as the age of the establishment of a new intellectual realm. But soon after the end of this period, the French Revolution initiated a different version of this interdependence. It produced an account of itself as the culmination of the ideals brought forward by the eighteenth-century philosophers and political thinkers. According to many historians, the depiction of the Enlightenment as an intellectual movement deterministically leading to a political uprising, which sought to establish a new social order, was to a great extent the outcome of the Revolution's self-narrative (Chartier 1991: 5). It is highly probable that most of the original citizens of the Republic of Letters would have serious reservations about this scenario (Outram 1995: 119).

Nineteenth-century Romanticism is another crucial instance of the Enlightenment's retrospective reconstruction. Of course, neither Romanticism nor the Enlightenment ever described themselves as homogeneous intellectual enterprises. It is important, however, that the common backdrop against which the various aspects of Romanticism perceived their distinctive identity was a highly simplistic image of the Enlightenment as an intellectual attitude, which deprived Nature of her inherent vitality and human beings of their divinely assigned freedom and ingenuity (Hamann 1784). Scientific reason became the central concept of *this* Enlightenment and the laws of Nature replaced natural law, which was dominant in the French Revolution's version of the Enlightenment.

During the first half of the twentieth century, and especially in the interwar period, the Enlightenment experienced a new transformation. This time a significant part of the European intellectual community answered the question "What is Enlightenment?" by treasuring a common philosophical and moral heritage. Scientific reason was again taken as the Enlightenment's centerpiece, but under the particular historical circumstances it mostly functioned as a model for the organization of a society threatened by the outburst of irrational beliefs and practices (Cassirer 1951 [1932]). Philosophers who handled this heritage sought to provide instructions for the correct application of scientific reasoning not only to the sciences but also to the entire sphere of social life.

Notwithstanding its lasting influence, however, this was a rather ephemeral philosophical reconstruction of the Enlightenment, as just after the World War II the *Dialectic of Enlightenment* cast a heavy shadow on the very same kind of rationality that hoped to counterbalance the interwar emerging fascism. Resonating with Romantic criticism, the authors described the Enlightenment as the climax of a long tradition of rationality, which gave birth to a Europe of social and cultural barriers, legitimized the demoralization of reason, established the blind power of techno-science, turned knowledge against society, and led to the Holocaust. In contrast to Cassirer's *Die Philosophie der Aufklärung*, Horkheimer's and Adorno's critique did not aim at identifying and validating the intellectual and cultural setup of their era but at calling to action against its devastating consequences (Horkheimer and Adorno 1972 [1947]).

Of course, between and after the historiographic instances mentioned here there are many other major or minor resurrections of the Enlightenment. The aim of this brief review was not to examine all these instances in detail, but to show that the primary purpose of the question "What is Enlightenment?" is not to be answered once and for all; it is, rather, to motivate contemplation about the past, which produces meanings for the present. The answer given by each time and locality to this question, that is, the version of the Enlightenment each specific context produces, is the past of a particular present and reflects the attitude of the individuals towards this present. To subscribe to a specific version of the Enlightenment is not (and never was) self-evident; it has always been a matter of selection and thus inherently debatable.

And this brings us to the main theme of this paper, the neo-Hellenic Enlightenment. The facts are these.

In 1945, the historian Constantinos Dimaras (1904–1992) published a paper entitled "French Revolution and the Greek Enlightenment around 1800." In this article, the term "Greek Enlightenment," which has formed the cornerstone of modern Greek history of ideas, was introduced for the first time. Three years later the same author published his seminal work *History of Neo-Hellenic Literature* (Δημαράς 1948 & 1949). In this work he suggested a periodization of the Greek history of ideas from 1600 to 1821 that is still in use. He divided the whole period into three phases. The first phase starts around 1600 with the national and educational policy of Patriarch Kyrillos Loukaris and ends in 1669 with the end of the Ottoman expansion in the Greek-speaking regions of the Balkans. In the field of philosophy, this period was characterized by a revival of the interest in the study of nature and a synthesis between neo-Aristotelian philosophy and Christian Orthodox theology. The term "religious humanism" used by Dimaras to designate this period bears connotations of a glorious Byzantine past ("Byzantine humanism"). The second phase starts in 1670 and ends one century later (1774) with a treaty between Russia and the Ottoman Empire that broadens and secures the economic privileges of the Greek-speaking populations. The period is known as the "Century of the Phanariots," a name that reflects the increasing political impact of the social group of the learned noblemen of Constantinople. Phanariots, after having ascended the various lay offices of the Ecumenical Patriarchate, advanced themselves in the political hierarchy of the Ottoman Empire. According to Dimaras, their political program was inspired by the ideals of Enlightened Despotism, an eighteenth-century form of absolute monarchy aiming to apply the principles of rationality and toleration in administration and public issues. At the same time, they promoted an intellectual life receptive to the European—especially French—culture, so they became the first agents of modernization of the emergent Greek society. The last phase starts in 1775 and ends with the Greek war of independence in 1821.

According to Dimaras, this is the period of “Neo-Hellenic Enlightenment,” characterized by the introduction of the philosophical and scientific attainments of the European Enlightenment. Dimaras maintains that the progressive scholars of the time, seeking a rational foundation for the social life of the Greek populations of the Ottoman Empire, spread the ideas that gradually led to the great national uprising. Throughout this period, the acquaintance with the scientific ideas played a significant role in eradicating superstition, promoting a firm belief in Reason, and reviving the connection of the “enslaved” Greeks with their ancestors.

The influence of Dimaras’ historiographic contrivance has been huge. His tripartite scheme was readily incorporated into the national historiography and is active up to this date. Its most important aspect is the connection of the Greek Revolution with the intellectual awakening of the “enslaved” Greeks thanks to their exposure to the philosophical and scientific ideas of *the* Enlightenment. For most Greek historians, departing from this scheme is inconceivable: it is sober, without apparent nationalistic implications, and profoundly Eurocentric. Furthermore, the narrative structured around the notion of Neo-Hellenic Enlightenment was easily received by the broader learned public, becoming thus a constitutive part of modern Greek identity.

However, in light of recent developments in the historiography of the Enlightenment, Dimaras’ conceptualization of Neo-Hellenic Enlightenment turns out to be highly questionable. Given that there is a variety of culturally and politically laden answers to the question “What is Enlightenment?,” why did Dimaras choose, first, to incorporate the Enlightenment into the Greek historiography and, second, to opt for a particular version of the Enlightenment, especially in a period when two conflicting interpretations of the Enlightenment were in use?

9.2 Greek Identity and the Dilemmas of Modernity

Dimaras clearly and unquestionably endorses Cassirer’s Enlightenment or something quite close to it. Voltaire is his hero and the main stakes, according to his view, both in Europe and in the Ottoman Balkans, were anti-clericalism, the secularization of philosophy, and the establishment of modern science. Of course, one might argue that when Dimaras first invented the notion of Neo-Hellenic Enlightenment, the Frankfurt School critique was not yet known; thus, quite naturally he subscribed to Cassirer’s mainstream interpretation. However, Dimaras’ book, which epitomizes his research on Neo-Hellenic Enlightenment, was published much later, in the late 1970s, without indicating any change of attitude (Δημαράς 1993 [1977]); and, most importantly, Dimaras’ scheme was willingly adopted by his successors without any substantial modification of its basic assumptions. Where did this stability come from?

In this chapter a tentative interpretation is suggested, which, although needing further elaboration, indicates a promising line of research. Dimaras was a member of the so-called Generation of the 1930s. Some historians of literature are quite reluctant to suggest a straightforward connection (Βαγενάς 2004), but none would disagree that Dimaras, beyond personal acquaintance, also shared the concerns of this avant-garde group. The Generation of the 1930s consisted primarily of poets and painters but also of essayists, novelists, architects, and theater people. During the interwar period and immediately after World War II they represented the movement of modernism in the Greek society. They shared, in many ways, the worries and anxieties of those who felt the traditional forms of self-definition both in the arts and in politics to be fading out in view of an uncertain world emerging from the dominance of technology and its accompanying economic and political balance of power. Greek modernists, however, were especially sensitive to another problem too: representing a nation that had come into existence no more than a century ago and which for various reasons had not yet clearly defined its position between East and West, they found themselves entangled in the question "Where and what have the Greeks been until these days?"

This question has recurred throughout the nineteenth century. In the late 1850s, a professor of the University of Athens, the historian Constantinos Paparrigopoulos, had produced an account that incorporated the Byzantine period into Greek history, securing thus an uninterrupted continuity of the Greek nation from early antiquity to the present. Paparrigopoulos' novelty was not as much that he attempted to connect modern Greeks with their ancient ancestors—something that had already been attempted by European classicists and philhellenes—as that he invented a living subject, which substantiated this relationship. This subject was "Hellenism." Paparrigopoulos introduced the terms "first Hellenism," "Macedonian Hellenism," "Christian Hellenism," "Medieval Hellenism," and "new Hellenism." Greek history was, thus, the history of a subject, Hellenism, and its successive metamorphoses. In the course of time and, actually, in the course of the publication of Paparrigopoulos' multivolume work, this scheme slightly altered, resulting in three Hellenisms—the first, the Medieval, and the new—but what is important after all is that now Greek history was presented as a narrative dealing with the adventures of the same subject, under different historical circumstances (Λιάκος 1994: 183–184).

During the 1930s, the construction of a consistent narrative about the fate of "new Hellenism" became imperative in view of the most important historical event of the period. The influx of the Greek-speaking populations of Asia Minor, as a result of the huge population exchange between Greece and the young Turkish Republic (1922–1928), called for a fresh look over the ideological premises of the Greek national identity. The classicist symbolism of the Greek (natural and intellectual) landscape did not suffice to incorporate the new populations, who were more familiar with an Ottoman context reminiscent of the pre-nationalistic era. If they were to be integrated into the national body, the identity of "new Hellenism" under Ottoman domination should be carefully and systematically reconsidered. If they were to be Greeks among Greeks, it should be convincingly explained how Greeks could *generally* exist in the Ottoman context during the last centuries.

While the answer to this question was still pending, the intellectuals of and around the Generation of the 1930s were prompted to tackle another dimension of the identity problem: Given the continuation of the Greek nation from ancient times to the present, which is the position of the Greeks in a changing world and an unforeseeable future? Eighteenth-century Greece was to a great extent an ideological product of European colonialism. Without being itself a colony, it was a hybrid formation, familiar and exotic at the same time. Beyond doubt, Greece represented the ancient source of European civilization; but it also was the most contaminated part of the continent by the "oriental barbarism." This conflicting character of the young Greek state and its ambiguous position between East and West played a significant role in the discussions about Greek identity throughout the nineteenth century. The preferred answer was that Greece belonged to the wide European family. This was not an unproblematic answer, however. If Greece were to play a role in the modern world, it should define *the way* it belonged to Europe. A whole century after the establishment of the Greek nation state, the Europeanization of Greece was primarily perceived as an act of imitation involving the danger of alienation from essential national qualities. Thus, an important task for the Generation of the 1930s was to promote cultural mutuality and to show that Greece was tied to Europe, not as an external body, but as an intrinsic constituent of European civilization (Τζιόβας 2007: 8–9).

As becomes clear from this brief survey, the interwar Greek intellectuals were faced with conflicting tasks: reclaiming the past and engaging with the future. But this was exactly the dilemma of modernity at large. The various European societies, which experienced the ideological instability caused by World War I and the end of liberal optimism, looked for ways to secure their distinctive physiognomy in a new and uncertain world order. The reassessment of tradition became imperative as a means of self-determination, but the handling of the issue caused significant inconvenience and tensions among the modernists. The Greek scholars of the 1930s were not an exception to this. Their different attitudes towards tradition prompted significantly diverging answers to the question about the place of Greek culture in the context of modernity.

The generation of the 1930s was connected with a relationship of apprenticeship to the poet Costis Palamas (1859–1943). Palamas was considered at the time a national poet and the relationship of apprenticeship primarily aimed at this aspect of his work—the "art of being a national poet:" How could one serve the national consolidation by resolving the ambiguities of national consciousness? And, under the particular circumstances, how could a poet save the European profile of his art without betraying the normative function of the Greek values? (Τζιόβας 2005: 134–135). The work in which Palamas himself tried to resolve the conflict between modernity and tradition was *The Twelve Lays of the Gipsy*, published in 1907. The poem was an inconclusive attempt, as the narrative failed to offer the much sought after *synthesis* towards a radically new quality. The tradition remained an amalgam representing the many different faces of the Greek identity: it sprang from ancient and Byzantine origins, embodied the Romantic ideal of individual freedom, and pointed to a scientific utopia as the consummation of classical reason. However, the poem clearly aimed at serving as an exemplar to be followed by Palamas' successors in their own attempts to situate Greek culture in the context of modernity. Palamas' vitalistic metaphors implied the possibility of tradition's revival in ever new contexts and forms, which according to his view comprised the essence of modernity. Modernity in this respect was a *performance* of tradition (Τζιόβας 2005: 162–163).

The Generation of the 1930s moved beyond Palamas' historicism. Reconsidering tradition might offer a possibility of adapting in a changing world, but contributing to its shaping demanded something more original. This originality was to be found in the archetypal values of the Greek identity. Tradition is simply a set of historical forms, whereas archetypal values represent the diachronic cultural mark of a people. Only by employing archetypal values could the interwar scholars hope to overcome the persistent ethnocentric perceptions of the past and become involved with their contemporary cultural developments in the European context. Thus, they invented the term "Greekness" (ελληνικότητα) and assigned to it the status of esthetic category. Greekness is not a measurable substance but an intuitive perception and a relative historical reality. It incorporates the diachronic qualities of the Greek soul, which are expressed in different ways under different historical circumstances, but remain a source of inspiration and a universal esthetic paradigm. During the 1930s scholars and artists emphasized the mythological and atmospheric dimension of these qualities. The estheticisation of the Greek landscape, and more particularly of the Aegean, is a most typical example of this intellectual attitude. After World War II they turned to a more historical perception of Greekness embodied by specific figures and periods of the Byzantine and post-Byzantine era (Τζιόβας 2007: 8; Τζιόβας 2011: 321–362): this resulted, to a significant extent, from the influence of Orthodoxy, which found its way into the modernist account as a strand that reinforced the universal claims of Greekness (Γιαννουλόπουλος 2003).

One way or the other, Greekness was called into play as a set of diachronic and universal values that would allow Greeks to participate in their contemporary intellectual exchanges as equal partners. The modernists of the 1930s felt free to appropriate the latest developments in literature, poetry, and painting, but they also promoted the Greek qualities as indispensable constituents of modernity: Greek is modern. However, notwithstanding the distance between Palamas' inconclusive synthesis and modernists' idealizations, both attempts share a common element: they seek to ensure the idea that the Greeks have always been the chosen people and their culture an archetypal culture. Most historians studying the interwar period almost exclusively focus on the different perceptions of past and future involved by each intellectual trend and the ensuing political debates of the time; but they fail to see that it was the departure from this common element which gave rise to a third answer—one that eventually gained the ground. This was Dimaras' answer.

As a historian of literature, Dimaras had meticulously studied Palamas' work and, actually, published his studies two years before the publication of his article on the "Greek Enlightenment." The relationship between the two scholars, however, went through two different phases. In 1930, in a book review, Dimaras stated: "Our generation, in the years of its formation, was so much inspired and shaped by Palamas' work that no blame against his poems' form can gain our consent: We had been watered by his inspiration, we experienced the rhythm of his verses, we adopted his phrasal modes" (cited in Βαγενάς 2004). Six years later, he was less enthusiastic and more cautious. He was aware that the poet was able to capture the Greek drama, the "contradictions permeating the Greek soul," but he was unable to resolve them. Thus, he aphoristically asserted that "in order for our nation to stand up, we must reach the exquisite end of a mentality [viz. Palamas' intellectual attitude], which we shall reject to survive" (cited in Δρούλια 1994: 14).

Dimaras rejected Palamas' inconclusive synthesis but he also followed a different path from his contemporary modernists. Through a complex intellectual process, the young Christian philosopher of the twenties turned to an atheist ("indifferentist") historian in the early thirties, who gradually shifted his research focus from the history of literature to the history of ideas. This process has not yet been investigated, neither can it be discussed in the context of this chapter, but it is important that it led to a unique synthesis, which marked recent Greek historiography.

Dimaras took up Paparrigopoulos' notion of "new Hellenism," but his aim was not to single out the Greek history or culture. He rather intended to accommodate the still unstable modern Greek identity in a secure social and ideological context. This context was Europe: the Greeks were intrinsically connected with the "European people" because the values of the classical Greek civilization lie in the foundations of Enlightenment's Europe. "Through the humanism of the classics, which shaped European civilization, Dimaras sought to establish that the Greek tradition was an inseparable part of the common European tradition, in other words that the Greeks should at last realize that they were Europeans and conversely, that the Westerners should gain access to Neo-Hellenic science [sic]" (Δρούλια 1994: 19).

Dimaras was not primarily concerned with the uninterrupted continuation of the Greeks from the antiquity to his days, as was Paparrigopoulos, but he was indeed concerned with the awakening of the national self-consciousness of the "enslaved" Greeks during the last decades of the eighteenth century. This awakening occurred thanks to the contact of the Greek intellectual life with the Enlightenment. People traveling westwards and ideas traveling eastwards made the Greeks realize that they were heirs of the very same values, which flourished in the atmosphere of the Enlightenment, but could not find a proper grounding in their own society. And it was this double awareness, motivated by the paradigm of the European (particularly French) Enlightenment—the awareness of their own heritage and, at the same time, of the nonfulfillment of their historical mission because of the Ottoman rule—that activated their reflexes and led to the Greek Revolution.

As a consequence—a rather peculiar consequence—the nation state that resulted from the Greek Revolution was a "nation of the Enlightenment," as a historian recently called it (Ελεφάντης 2007). *Dimaras' major achievement was that he answered the question of modernity by merging the fate of "new Hellenism" with the fate of Europe. And the period during which this merging primarily took place was the Enlightenment.*

9.3 The Making of the Enlightenment

Dimaras was not only a skillful historian but also a capable manager. In this capacity he played a crucial role in the establishment of his historiographic scheme. As early as 1942, in a series of newspaper articles, Dimaras had suggested "the most general lines of a research project aiming at the study of the Greek literature primarily during Turcocracy [viz. the Ottoman rule]." He outlined the intellectual tasks that should be undertaken for Neo-Hellenic studies to be established and the "national census" required for the consolidation of the Greek national self-consciousness. He suggested the foundation of new institutions, the publication of new journals, and the establishment of scientific societies. The "national census" involved, among other things, biographies of the leading figures of the pre-Revolutionary era, catalogues of books and journals, records of Greek schools functioning at the period, and lists of scientific and philosophical translations that channeled the European thought into the Greek intellectual life. Above all, Dimaras stressed the need for an "Organization comprised of a big number of properly trained researchers providing all means necessary for the intensive performance of their work" (Δρούλια 1994: 17).

This organization came to life in 1959. Dimaras was instrumental in setting up the Royal (and later National) Hellenic Research Foundation, of which he was the first executive director. One of NHRF's first institutes was the Center for Neohellenic Research, devoted, as its name implies, to the study of "new Hellenism." Dimaras recruited and trained a significant number of promising historians, whose mission was to unearth and file all the documents testifying to the contact of new Hellenism with the West, and especially with the Enlightenment. As he had planned it 20 years earlier, he organized the publication of biographies and correspondences, and he edited himself or supervised the edition of the unpublished papers of the major figures of Neo-Hellenic Enlightenment. His organizational work was huge. He researched, wrote, directed, supervised, and coordinated; and, on top of all these contributions, he had a continuous presence in the press as a columnist for no less than 60 years. Taking some distance from his work and asking what was the purpose of this prolific activity, we may quite securely answer: to shape and promote the construct of Neo-Hellenic Enlightenment.

Besides administration and management, however, Dimaras was a very skillful historian. Shaping the construct of neo-Hellenic Enlightenment required a focused theoretical work, and he undertook to do much of this work himself. His most important tool was analogy. To support the idea that in the emergent Greek society of the eighteenth-century Ottoman Empire an Enlightenment took place almost simultaneously with the great French Enlightenment, he needed to establish analogies with the European societies of the time (Αποστολόπουλος 1994: 73–74). This was not an easy task, because it is hardly possible to talk of an Italian or a German society during the eighteenth century, let alone of a single and homogeneous society in countries whose social fabric was already interwoven with a multitude of colonial social contexts. Thus, Dimaras focused mostly on the French society and partly on Frederic's Prussia and Catherine's Russian Empire. What he discovered there was a society basically consisting of three groups (or classes or strata, depending on the ideological predilections of his disciples). One group was the clergy, deeply conservative and in principle anti-philosophical. The other group was connected with the state administration and the nobility. It consisted of people who, although they did not subscribe openly to the Enlightenment, displayed a favorable attitude towards its major representatives. The political expression of this group was the so called Enlightened Despotism, as it was personified by Frederick II and Catherine the Great. The third group was the emerging bourgeoisie, which enrolled the philosophers and political thinkers of the Enlightenment to create the intellectual context of its future dominance. This latter group was the actual social basis of the Enlightenment.

Speaking of "Greek society" in the context of the Ottoman Empire is also quite problematic. Throughout the eighteenth century, the Greek-speaking Orthodox populations of the Balkans not only lacked the institutional structure of a nation state; they also lacked the geographic continuity that could form the basis for a unification of their social activities. The "Greek society" consisted of a network of sites where Greek-speaking populations pursued various economic and political enterprises. Besides the Balkans, the Greek communities were dispersed along the main commercial routes of Eastern Europe, and within the most important cities of the Northern Italian peninsula, the Hapsburg Empire, and the German states. There were only two strong unifying elements which differentiated these populations from others and assigned them a certain degree of integrity: the Christian Orthodox faith and Greek-speaking education. Both were under the jurisdiction of the same authority, the Ecumenical Patriarchate of Constantinople, but both were also colored by the particularities of the various local communities. In this capacity, education and Church hosted all kinds of fermentation, negotiations, and collective pursuits concerning the emergent society's political and intellectual identity (Patiniotis 2008: 265–266). Within this loose and multifaceted formation Dimaras and his followers implanted the class system of the Western societies, however idealized.

The clergy was represented by the Ecumenical Patriarchate, conservative, anti-philosophical, and anti-scientific; with many exceptions, of course, but still on the antipodes of progress and social emancipation (Ηλιού 1988). It is highly revealing that historians working in the context of Dimaras' scheme investigate the debates about the heliocentric system in Greek intellectual space and discuss the resistance of the Church, when we know that such resistance was rare and the few debates that occurred were mostly motivated by personal antagonisms and not by the official policy of the Ecumenical Patriarchate (Αγγέλου 1988).

The nobility did not exist in the "Greek society," significantly because there was no hereditary nobility in the Ottoman Empire. Thus, it had to be invented. The Phanariots, the self-made entrepreneurs who turned their wealth into political offices, both in the court of the Ecumenical Patriarchate and in the hierarchy of the Sublime Porte, were molded to fit a historically intermediate agent demanded by the scheme of Neo-Hellenic Enlightenment. On the one hand, they formed a concise expression of the emergent Greek society's dynamism and, on the other, they cross-fertilized an intellectually dormant society with the new trends of Western philosophy, science, and literature. The political aspirations of the Phanariots, according to the received historiography, were closely tied to the tradition of the Enlightened Despotism, and many of them had the chance to implement its principles as governors of the semiautonomous regions of Moldova and Wallachia (Δημαράς 1993: 7–10, 222–224, 263–282, et al.).

The problem of the bourgeoisie was solved in a similar manner. Strictly speaking, it is impossible to locate in the eighteenth century a Greek bourgeois class comparable to the British, the French, or the Dutch ones. But there were indeed dynamic merchants, who traveled across Europe to transfer commodities from the Ottoman lands to the industrial markets of Europe and bring back other commodities or luxury items. Despite the fact that the Greek identity of these people is highly questionable (they were Balkanians rather than Greeks) and that they were generally indifferent to philosophy and higher education (Stoianovich 1960; Κατσιαρδή-Hering 1995), they were recruited by Dimaras' historiography to bring the Enlightenment to the Balkans. No doubt among them there were indeed learned men who became involved with intellectual pursuits and patrons who sponsored the establishment of schools and the publication of books. But such sporadic incidents do not suffice to make the traveling merchants representatives of the spirit of the Enlightenment. For Dimaras and his followers, this issue goes usually unquestioned (Δημαράς 1993: 27–28, 154, 310–314, et al.).

Having established analogies with the European "societies," which nurtured the Enlightenment, Dimaras needed to take one last step: to find the voice of the Enlightenment within the Greek society. This voice was a man who lived—where else?—in Paris, sympathized with the political philosophy of the Enlightenment, was an eye-witness of the French Revolution, and defended the use of common language in the Greek intellectual life: Adamantios Korais (1748–1833).

Korais was the personification of *synthesis*. As a disciple of Palamas, Dimaras was especially concerned with this concept. Synthesis epitomized the process that led to the shaping of new Hellenism through the convergence of a variety of cultural elements. Thus, as he puts it in his *History of Neo-Hellenic Literature*, Yianis Vilaras (1771–1823) and Athanasios Christopoulos (1772–1847), two major poets of the eighteenth century, *performed* synthesis not only because their work integrated a variety of dispersed literary elements, but also because their personality represented the new kind of man inspired by the ideals of freedom and national emancipation. But the person who performed synthesis in the most complete way was Adamantios Korais. Although he did not belong to the realm of literature, he was a key figure whose work and personality managed to express "all the dispersed but active proclivities of new Hellenism as we perceive it today" (Αποστολίδου 1994: 135).

The history of neo-Hellenic Enlightenment, as laid out by Dimaras and his followers, is a narrative, which naturally leads to Korais. According to this narrative, in the early nineteenth century, all intellectual currents pointing to the direction of the forthcoming national uprising emanated from Korais' sphere of influence. All the progressive forces of the Greek society were inspired by his political thought and implemented his advice to "channel" the European attainments in philosophy and the sciences into the Greek society (Δημαράς 1993: 106–119, 301–389). It is true that scientifically or philosophically Korais was not as competent as other eighteenth-century scholars. Neither was he a really representative figure of his time, as were Eugenios Voulgaris (1716–1806) or Iosipos Misiodax (1725/1730–1800). But he was indeed one of the few Greek scholars who were not ordained clergymen. He criticized the backwardness of the Orthodox Church (and was excommunicated for this reason) and insisted on the abolition of the Phanariots' power networks to free space for the political action of the emerging bourgeois groups. In Dimaras' eyes, Korais was the Greek Voltaire.

9.4 Conclusion: A Dream of Europe

The claim put forward in this paper, which is still a working hypothesis, is that Dimaras constructed and manned the Greek Enlightenment to secure the position of the Greek nation state within Europe. He gave up the leading role of the Greek culture in exchange for a steady orbit in the European firmament: Greeks are not the first anymore, but they have always been among the first by hereditary right, and the events of their recent history, *the history of new Hellenism*, show how they came to rediscover their natural position after a long period of self-alienation.

One important historiographic consequence of Dimaras' choice (although not his own innovation) is that it radically cut off recent Greek history from its Balkan and Ottoman contexts. The creation of the Greek nation state appears to be the result of a series of influences, exchanges, decisions, and debates, which took place exclusively within the Greek context. This complex process eventually gave birth to a nation state, which quite plausibly, as already mentioned, could be called a "nation of the Enlightenment." Thus, Dimaras moved beyond the problematic and inconclusive recurrence of tradition (Palamas) on the one hand and the vague and unstable estheticisation of Greekness (Generation of the 1930s) on the other. He shaped recent Greek history in a way that it persuasively appears to be a genuinely Greek matter long before the establishment of the Greek nation state. And when this state eventually came into being, it appeared to be already naturally placed in the world of modernity: Greece and Europe are intimately tied and so are their political and cultural fates.

Dimaras' Neo-Hellenic Enlightenment was the winning—sober and convincing—response to the interwar debates about the fate of Hellenism in the new world order emerging from the crisis of modernity. As a historian put it recently: "What do we owe to Dimaras? That he was the first who systematically studied the fact that ideas and principles of the movement, which in Europe became known as the Enlightenment, flowed to the area of the 'Greek Orient'; that he founded the view that the Neo-Hellenic Enlightenment was not an autochthonous phenomenon, but a branch of the European Enlightenment; that he organized the respective research in

Greece and took care to connect the Greek society with international science" (Αποστολόπουλος 2004).

There is more, however. If for a moment we change perspective and place Dimaras' answer in the broader context of the Enlightenment's historiography, we realize that his enterprise has had some more pervasive consequences. Through their attempts to establish a local version of the Enlightenment, Dimaras and his followers produced a stereotypical account of what they considered to be the Enlightenment par excellence.

- Voltaire is the paradigmatic scholar of the Republic of Letters and Montesquieu the paradigmatic political thinker.
- The main aim of the *philosophes* was to fight superstition and ignorance and their most obstinate opponent was the conservative clergy.
- The presence of science and its accompanying empiricism in a society is a measure of its cultural maturity: the more inclined to scientific thinking and the more distant from the Aristotelian scholasticism, the closer to the Enlightenment.
- The Enlightenment leads to Revolution. It happened in France, it also happened in Greece.
- And, above all, the paradigmatic Enlightenment is the French Enlightenment. Speaking of Enlightenment means to examine how and to what extent a society adopted the *civilizational* patterns induced by the great French intellectual movement.

We started with the assumption that Dimaras endorsed Cassirer's interpretation of the Enlightenment. But this is not actually Cassirer's Enlightenment. It is rather a locally produced model, which fits the Greek case and is *projected back* on the great mural of the *Enlightenments*. In fact, beyond great narratives, and the image of a well-defined movement which shaped modern Europe, the idea we have, at any moment, about the Enlightenment is the product of such local appropriations: A vague and multiply distorted reflection of the discourses produced by various social formations in their bid to become integrated into an imagined entity representing the political ideals of modernity. Dimaras' quest for Europe is a typical example of this process, highly revealing of the powers involved in the shaping of the Enlightenment.

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Chapter 10
The Non-introduction of Low-Temperature Physics in Spain: Julio Palacios and Heike Kamerlingh Onnes

José M. Sánchez-Ron

Abstract Among the many topics of interest for the history of twentieth-century science are the transmission of science from productive scientific centers to the periphery and the history of low-temperature physics. It happens that both topics met in the case of the Spanish physicist Julio Palacios, who worked at Kamerlingh Onnes' laboratory in Leiden from October 1916 until the end of the First World War. In this chapter, it is discussed how he came to apply the expertise he had acquired there when he returned to Spain. As a matter of fact, he was unable to introduce in Madrid what he learned in Holland because of the technical sophistication and exigencies of research in low temperatures, turning instead to X-ray diffraction, a promising field less technically demanding (his work and international connections there are also covered). In view of that story, it is concluded that an appropriate scientific policy for underdeveloped countries is not necessarily to send their young and most able scientists to the best research centers in the world. In the end, it was Onnes' laboratory that benefited more from Palacios's stay, because, without any expense on its part, it obtained the help of an able young physicist, at a time in which Leiden did not receive many visitors.

Keywords History of Science in Spain • H. Kameringh Onnes • Low-temperature physics

10.1 Introduction

Among the many subjects that Kostas Gavroglu has covered during his long and distinguished career as a historian of science, two are the transmission of science from the productive scientific centers to the periphery and the history of low-temperature physics.[1] Indeed, "the transmission and appropriation of scientific

[1] Gavroglu and Goudaroulis (1985, 1988, 1989), Gavroglu and Goudaroulis, eds. (1991), Gavroglu, ed. (1999), Dialetis et al. (1999), Gavroglu and Patiniotis (2003).

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concepts and practices that originated in the several 'centers' of European learning and, then, appeared (often in considerably altered guise) in regions of the European 'periphery'" (Buchwald and Gavroglu 1999: vii), was the subject of a book he edited (Gavroglu 1999). One of the places belonging to the "European periphery" that Buchwald and Gavroglu mentioned in their prologue to that book was the Iberian Peninsula, that is, Spain and Portugal, countries that, as they stated, "have not been studied systematically" in connection with the transfer of new scientific ideas, the mechanisms of their introduction, and the processes of their appropriation. Here, I discuss Spain in a case concerning twentieth-century physics.

During the nineteenth century and the first half of the twentieth, Spain clearly belonged to that "scientific periphery," at least as concerns sciences such as mathematics, physics, and chemistry. Not so, or not always so, insofar as biomedical sciences are concerned, is the case of Santiago Ramón y Cajal. Winner of the 1906 Nobel Prize in Medicine "for his work on the structure of the nervous system," Cajal's publications and laboratory became an international center of attraction for histological studies. This example, by the way, confirms another of Buchwald's and Gavroglu's assertions, namely, that "depending on the subject one is discussing—a place may at one and the same time be both center and periphery. A center may, over time, change into a periphery, and vice versa. And a single country may contain both centers and peripheries, thereby making national distinctions of dubious use" (Buchwald and Gavroglu 1999: vii).

Gavroglu also worked on the history of low-temperature physics, paying special attention to its world center, Heike Kamerlingh Onnes' Leiden laboratory, "the coldest spot on earth" during the first decades of the twentieth century, as it has been aptly denominated (van Helden 1989).

In this chapter I offer a case study at the interface of both topics discussed by Kostas, by considering the time Julio Palacios spent in Leiden and how he came to apply the expertise he had acquired there when he returned to Spain. As we shall see, Palacios was unable to introduce in Madrid what he learned in Holland, that is, research in low-temperature physics, in spite of having been fortunate to work in what was then the best Spanish physics laboratory, because of the technical sophistication and exigencies of research in low temperature. We may conclude that an appropriate scientific policy for underdeveloped countries is not necessarily to send their young and most able scientists to the best research centers in the world, but to elaborate beforehand a plan adjusted to the possibilities of the country. In the case of Palacios, it would have been better to send him to a center specialized in X-ray diffraction, the less technically demanding subject to which he finally turned when he returned to Madrid after his stay in Leiden. Would he have done that, or had those who directed the grants program from which Palacios benefited, precious time would have been gained, establishing scientific relationships with foreign individuals and centers with which Spaniards later established fruitful relationships. In the end, it was perhaps Kamerlingh Onnes' laboratory that benefited more from Palacios's stay, because, without any expense on its part, it obtained the help of an able young physicist, at a time when because of the First World War Leiden did not receive many visitors. As we discuss, this circumstance may be related to a problem that affected many scientific underdeveloped countries all throughout the twentieth century: the brain drain of their most able young scientists.

10.2 Physics in Spain in the Early Twentieth Century

Spain emerged from the nineteenth century in a precarious situation.[2] It was not only that she had gone through political revolutions, as the so-called "La Gloriosa," which began in 1868, but that she lost one of her most precious treasures: in 1898, Spain was defeated in her war against the United States, being forced to abandon its last colonies, Cuba and the Philippine Islands. That loss shook Spanish society in a way not many other events did at the time, and it happens that many Spaniards thought that the cause, or one of the main causes, of the defeat was the scientific and technological inferiority of their country. At the Cortes (Spanish Parliament), the deputy Eduardo Vincenti exclaimed on June 1899[3]:

> I will not stop saying, putting aside false patriotism, that we must follow the example that the United States has given to us. This country defeated us not only because it is stronger, but because it has a higher level of education than we have; in no way because they were braver. No Yankee has come up against our navy or army, but rather a machine invented by some electrician or machinist. There has been no fight. We have been defeated in the laboratory and in the offices, not at sea or on the mainland.

Indeed, when one looks at physics—a science that was then, as it is well known, experiencing a tremendous development—in nineteenth-century Spain, what is found is a dire situation. If we accept that a good indicator for the state of physics is the experimental facilities available, then it is relevant to quote what Antonio Gil de Zárate, director of Public Instruction, a department of the Ministry of Fomento (dedicated to the functions of what is today the Ministry of Economy), had to say concerning those facilities by the middle of the century (Gil de Zárate 1855: II, 317):

> Beginning with the buildings [. . .], they are almost destroyed, in a situation that showed the negligence both of the Government and of those charged with their conservation. The lecture rooms were obscure, dirty and without the necessary furniture [. . .] It is impossible to find in such establishments that richness of instruments and collections which form the adornment of schools where tribute is paid to the observational sciences [. . .] If in some place there was a rough and badly prepared magnet, an old unserviceable pneumatic machine or another electric instrument, such apparatus was shelved as useless and despicable. Some universities, during the last years and thanks to the interest of young rectors, had begun to buy the more precious instruments, but, for the major part, they had no idea of them; and in none could anybody find regular physics cabinets, laboratories, not even natural history collections.

[2] For more information on this point, see Sánchez-Ron and Roca-Rosell (1993).

[3] Vincenti (1916); quoted in Turin (1959, 375).

To remedy the situation, and to provide the chairs of physics and chemistry at the Philosophy Faculties with experimental facilities (the Science Faculties were not created as independent university centers until 1857), a commission was established to decide which were the more urgent necessities of scientific apparatus. A fund of 621,028 *reales* was granted for such purpose (in Madrid, at that time, the salary of a full professor was 30,000 *reales*), and the aforementioned Gil de Zárate, together with Juan Chávarri, a physics professor, in 1845 went to Paris, where, with the help of the Spanish chemist Mateo Orfila, then dean of the Medicine Faculty of Paris University, they bought materials that were later distributed in 11 Physics Cabinets.

However, in the following decades there were no new provisions in the State budget for buying scientific instrumentation, or even for the conservation of the equipment already bought. Therefore, it is not difficult to find expressions of frustration about the miserable situation. For example, Gumersindo Vicuña (1876: 39), physics professor at Madrid University, used the occasion provided by the inaugural lesson—a very formal and ceremonial act—that he delivered to open the academic course if 1875–1876 to voice his opinion about "the great delay in which the practice of physics in Spain is found, compared with that of other sciences." He further argued that one of the main causes of such delay was that "experimentation is reduced to that performed with very simple apparatus, which are usually only used to make demonstrations to the students, when instruments are not loose-jointed and broken. Delicate and expensive apparatus, instruments designed to prove complex natural relations, are not found in our cabinets, and if there is one, it is seldom used." More than 40 years later, and in a similar vein, José Rodríguez Carracido (1917: 398), professor of biological chemistry in Madrid (a chair delivered to students at the faculties of medicine, pharmacy, and sciences), complained that "during 14 years, biological chemistry was taught as if it were metaphysics, all professors opposing unanimously (irrespective of their specialties) the request to establish a most necessary laboratory." Further, the poor and underdeveloped situation of Spanish industry did not help at all in providing, as happened in other countries, instruments or opportunities to physicists.

It was against such background that a new institution, the Junta para Ampliación de Estudios e Investigaciones Científicas (Board for the Extension of Studies and Scientific Researches; JAE), was created in January 1907, when the liberal party was in power, by the recently established (1900) Ministry of Public Instruction and Fine Arts. Although this is not the proper place to study this institution in depth, something must be said about it.[4]

For Spanish standards, the Junta was a revolutionary institution, without precedent in the history of the nation. Created thanks to the efforts and influence of a small group of intellectuals related to the Institución Libre de Enseñanza (Free Teaching Institution), a private and progressive educational institution founded in

[4] For more information about the JAE, see Laporta, Ruiz Miguel, Zapatero and Solana (1987, the various articles included in Sánchez-Ron, comp. (1988), and Sánchez-Ron and García-Velasco, eds. (2010).

1876 by a few professors who had been expelled from their universities in 1867–1868, owing to their liberal ideas, the JAE—whose first president was Santiago Ramón y Cajal (he retained this post until his death, in 1934)—wanted to help renew and improve the Spanish educational system at all levels.[5] It aimed at doing so by promoting and developing not only the exact and natural sciences but also disciplines such as history, philology, law, history of art, and philosophy. Believing that one of the main problems in Spain was the lack of knowledge of what was going on in more developed countries, the JAE made a basic tenet of its policy to send abroad graduate students, as well as school and university professors. The decree creating the JAE was explicit: "A country that lives in isolation holds up progress and becomes a decadent one. Because of this, all civilized nations take part in the movement of international scientific circulation [taking place presently], which includes not only small European countries, but also backward nations, such as China and even Turkey, which has students in Germany in numbers amounting to four times the Spanish one; that is, [we are] last but two among all Europeans."

During its existence (1907–1938), the JAE received approximately 9,000 requests for grants (*pensiones*), of which more than 2,000 were granted. As to the countries chosen, 29 % of the holders of scholarships went to France, 22 % to Germany, 14 % to Switzerland, 12 % to Belgium, 8 % to Italy, 6 % to Great Britain, 4 % to Austria, and 3 % to the United States. Of the 560 university professors or lecturers who applied for grants, 73 (13 %) taught at faculties of sciences, 216 (38 %) at faculties of medicine, 53 (9.4 %) at faculties of philosophy, and 150 (26.7 %) at faculties of law. The percentages for the different disciplines are significant also: medicine, 18.6 %; pedagogy, 18.5 %; history of art, 10.6 %; law, 9.7 %; chemistry, 6 %; history, 5.7 %; natural sciences, 5 %; philology and literature, 4 %; engineering, 3.6 %; physics, 2.4 %; mathematics, 2 %; and philosophy, 1 %.[6]

These percentages might seem to indicate that physics was not particularly favored by the Junta, but what they reveal instead is that physics was not a major discipline in Spain. In this respect it is significant what José Castillejo, a law professor and the powerful and active general secretary of the Junta, wrote on 21 July 1924 to Wickliffe Rose, of the Rockefeller International Education Board, when negotiating with this philanthropic institution for its economic support for a new physics and chemistry laboratory[7]: "Physics and chemistry have been considered by the Junta fundamental studies for scientific progress. Between 1907 and 1924, the Junta granted scholarships in physics and chemistry to 66 professors and graduates for laboratory work, for one or two and on some cases for three years in different countries, viz.: in Germany 25 scholars; in Switzerland 17; in France 15; in the United Sates 10; in England 5; in Holland 2; in Belgium 1; in Russia 1; in Monaco 1."

[5] About the Institución Libre de Enseñanza, see Institución (2013).

[6] These numbers were drawn from the *Memorias* that the JAE published biannually by Laporta et al. (1987).

[7] International Education Board, 1.2, 41.577, Rockefeller Archive Center, Pocantico Hills, Tarrytown, New York, USA; quoted in Sánchez-Ron and Roca-Rosell (1993, 136). Castillejo's science policy is discussed in Sánchez-Ron (2008).

The JAE was also convinced that improving the country's scientific standing required more than sending individuals abroad. For what would happen when those individuals returned to Spain? In the opinion of those who created the JAE, the universities had no way of profiting from so many trained scientists; on the contrary, the structure of the Spanish University system, traditional and hierarchical and paying almost exclusive attention to teaching, would put in jeopardy their scientific potential. Consequently, one of the JAE's aims was to create centers of its own in which advanced research could be done. Thus, in 1910 it established two such centers: the Centro de Estudios Históricos (Center for Historical Studies) and the Instituto Nacional de Ciencias Físico-Naturales (National Institute of Physical and Natural Sciences), designed to control the laboratories and departments the Junta might support or create. The Laboratorio de Investigaciones Físicas (Physical Research Laboratory), depending on the Instituto Nacional de Ciencias Físico-Naturales, was soon (1910) created by the JAE to support physics and chemistry (and especially physical chemistry insofar as chemistry was concerned). This was not the only scientific laboratory created or supported by the Junta (there were others, dedicated, for example, to physiology or histology), but as the present chapter is dedicated to physics it is the one that interests us here.

10.3 The Laboratorio de Investigaciones Físicas

The JAE physics laboratory was put under the direction of Blas Cabrera (1878–1945). Born in Arrecife, Lanzarote, in the Canary Islands, Cabrera traveled to Madrid in 1894 with the aim of studying law but soon moved to the Faculty of Sciences, obtaining his degree (*licenciado*) in 1900. After having produced in 1901 a doctoral dissertation on a minor subject, the diurnal variations of the wind, Cabrera was revealed as a prolific researcher, publishing eight papers between 1903 and 1904 on experimental topics concerning electrolytes and elementary questions of electromagnetism. Although his scientific curriculum was quite modest by international standards, in 1905 he was appointed Professor of Electricity and Magnetism at the Faculty of Sciences of Madrid University. Moreover, in 1909 he was elected as a member of the exclusive Royal Academy of Sciences (Real Academia de Ciencias Exactas, Físicas y Naturales). If we take into account that Cabrera and Ignacio González Martí (1860–1931), also professor of physics (General Physics in his case) at Madrid University but much older than Cabrera, were then the only physicists at the Academy, the conclusion is inevitable: Cabrera was at the summit of his profession. It is not surprising, therefore, that when the JAE created a laboratory dedicated to physics and, to a lesser extent, chemistry, in Madrid, Cabrera became its director. Because he was essentially an experimentalist, interested in the magnetic properties of matter, as well as in some aspects of physical chemistry, his profile was appropriate to lead a laboratory covering physics as well as some branches of chemistry (especially, physical chemistry, a branch in which the leader was Enrique Moles). As a matter of fact, Cabrera turned out to be a very good choice, as the quality of his work improved, especially after the months he spent in Zurich during the summer of 1912, working in Pierre Weiss' laboratory. Simultaneously his international reputation also increased: in 1928, he was elected a member of the Commission Scientifique Internationale of the Institute Internationale de Physique Solvay (in the election no doubt the fact that he was a citizen of a country of the "scientific periphery" played a role); and in 1930, he became a member of the Comité International des Poids et Mesures in Paris, of which he became secretary in 1933. When John van Vleck (1978) reviewed the literature of measurements on the magnetic susceptibilities of rare earths for inclusion in his book *The Theory of Electric and Magnetic Susceptibilities* (1932), he found that many such measurements had been made by Cabrera, whose name thus appeared prominently in the book.

Initially, the Laboratorio directed by Cabrera had four sections: Metrology, Electricity, Spectroscopy, and Physical Chemistry. By 1914, its structure was almost complete with five central groups: Physics, directed by Cabrera, and dedicated mainly to rather general and miscellaneous topics as the physical properties of metals in electric and magnetic fields; Physical Chemistry, directed by Enrique Moles; Magneto-Chemistry (Cabrera); Electrochemistry and Electroanalysis (Julio Guzmán); and Spectroscopy, headed by the chemist Ángel del Campo. In 1915 another chemist turned physicist, Miguel A. Catalan, joined the last group; after his discovery, in 1922, of a multiplet structure in the spectrum of manganese while working in Alfred Fowler's London laboratory under a grant from the JAE, he became an international figure in the field of spectroscopy.[8]

No other laboratory did more to improve the poor state of Spanish physics during the first three decades of the twentieth century than the JAE physics and chemistry laboratory. As far as physics is concerned, a measure of such success is that 223 of the 303 (or 73.5 %) articles on physics published between 1911 and 1937 in the *Anales de la Sociedad Española de Física y Química*, the major physics and chemistry Spanish journal, were written by scientists affiliated with the JAE (Valera Candel and López Fernández 2001: 79). Many young physicists and chemists who were being introduced to research found in the laboratory a scientific environment that hardly existed at the few scientific facilities in Spanish universities.

10.4 Travelling to the Scientific Centers, or "the Periphery Goes to the Centers"

The policy followed by the JAE of taking the "periphery" to the "centers" through grants was used wisely by Cabrera's Laboratorio: almost all the physicists and chemists who became senior researchers, whether permanently or temporarily,

[8] Catalán (1922). About Catalán, see Sánchez-Ron (1994).

received grants from the Junta to study abroad.[9] In 1909, that is, before the Laboratorio officially opened its doors, the chemist Ángel del Campo, one of the oldest members of the laboratory, went to Paris to work with Georges Urbain. That same year, Manuel Martínez-Risco, a recent graduate (1908) in physics from Madrid University interested in optics, applied for a grant to the JAE, "to work," as he stated in the call for the application, "at the laboratory of professor P. Zeeman, of Leiden University, in Holland." We know, of course, that Pieter Zeeman was then a famous physicist, having received in 1902, together with Hendrik A. Lorentz, the Nobel Prize for Physics. However, what happened to Martínez-Risco reveals how peripheral were Spanish physicists, and how great was their necessity of establishing links with "the scientific centers"; after having obtained the grant from the Junta, Martínez-Risco went to Leiden, to find out that Pieter Zeeman did not work there. He reported the mistake to Santiago Ramón y Cajal, the President of the JAE, in a letter written on November 23, 1909, in the following terms[10]:

> Dear Sir,
>
> I have been informed at the University that Professor P. Zeeman, far from dedicating himself to physics, teaches rational mechanics, analytic geometry and descriptive geometry.
>
> The eminent physicist, discoverer of the effect which bears his name, has also the same name, P. Zeeman, but lives instead in Amsterdam. The identity of names and surnames has been the cause behind this unpleasant surprise.
>
> I believe to comply with my duty if I depart immediately to Amsterdam.

Finally, Martínez-Risco found Pieter Zeeman, under whose supervision he worked from November 1909 to June 1911 at the Natuurkunding Laboratorium he directed. There he specialized in the field of spectroscopy, working with the method of Fabry-Perot in asymmetrical triplets of spectral lines. Back in Madrid, he used the results obtained for completing his doctoral dissertation, which he submitted under the title of *Asimetría de los tripletes de Zeeman* (Martínez–Risco 1911). His academic career proceeded first as a researcher at the JAE Laboratorio, and afterwards (1914) in Saragossa University as full professor of Acoustics and Optics, a chair that he changed for the same at Madrid University in 1919.[11]

The year 1912 was particularly active for grants for members of the Laboratorio. The necessities of the Madrid laboratory were becoming clear: above all, to establish international connections to learn what was being done in the best centers

[9] The international relationships of Spanish physicists in a more general context are studied in Sánchez-Ron (2002).

[10] Junta para Ampliación de Estudios Archive, Residencia de Estudiantes, Madrid.

[11] After been active in research in Madrid during a few years, Martínez-Risco became involved in politics, an activity that consumed practically all his time and which led him into exile after the Spanish civil war (1936–1939); he then settled in Paris, where he resumed his scientific researches at the Centre National de Recherche Scientifique. Martínez-Risco's thesis is reprinted, together with the rest of his papers, in Martínez-Risco (1976: 9–79). The relationship of Martínez-Risco with Zeeman was presented in one paper with Zeeman (Martínez-Risco and Zeeman 1929; reproduced in Martínez-Risco 1976: 133–139). About Martínez-Risco, see Sánchez-Ron (2012).

for physical research. Julio Guzmán went to Leipzig to work with Carl Drucker during 1912 and part of 1913; Jerónimo Vecino spent 3 months in Paris, studying metrology at the Bureau International des Poids et Mésures, working first with J.-René Benoît, the Bureau director from 1889 until 1915, and then with Charles Édouard Guillaume, Benoît's successor. Santiago Piña de Rubíes spent 6 months at the pharmacy laboratory of Geneva University, and in Russia, taking part in an expedition to the Ural Mountains, headed by Louis Duparc, to collect different minerals. A decade later, in 1922–1923, he spent 2 months at the Bureau International des Poids et Measures, then traveled to Munich to work in Wilhelm Wien's Physikalische Institut, where he measured and interpreted the spectrum of scandium, and those of rare earths, with the help of W. Prandtl, and finally to Tübingen, to learn techniques of the Zeeman effect under Friedrich Paschen and Ernst Back. As mentioned previously, Blas Cabrera too used the grant benefits of the JAE, spending the 1912 summer in Zurich with Weiss, where he joined Enrique Moles, the head of the chemistry section of the Laboratorio, who had also been a recipient of a grant from the Junta and went to Geneva in 1915 to work with Philippe A. Guye.

In 1914, Julio Palacios (1891–1970), one of the young physicists who joined Cabrera's Laboratorio, also applied to the JAE for a grant.

10.5 Julio Palacios in Leiden

Born in Paniza (Saragossa), Julio Palacios Martínez (or Julio Palacios, as he was known in Spain; that is, omitting his second surname) studied physics and mathematics at Barcelona University, where he graduated in 1911. In 1912, he began to work at the JAE Madrid Laboratorio under the direction of Blas Cabrera. He took a course in metrology, dedicating special attention to the study of thermometers and weighing scales, a path of studies common to students who wanted to travel abroad to work at the scientific centers. Then, supported by the JAE, Palacios began working on his doctoral dissertation, supervised by Cabrera.[12] Defended on June 30, 1913, it was entitled *Determinación de las constantes ópticas de los cristales birrefringentes* (*Determination of the optical constants of the birefringent crystals*) (Palacios 1914). Soon afterwards, he became assistant professor at the Madrid Faculty of Sciences, collaborating in the course taught by Ignacio González Martí, who had been a member of the commission who judged his doctoral dissertation.[13]

[12] Until the 1950s, the only Spanish university entitled to award the doctoral degree was the University of Madrid.

[13] The other members of the commission were Bartolomé Feliú, Blas Cabrera, Francisco de Cos, and Antonio Vela.

Having a good command of French and German, on 31 January 1914 Palacios applied for a JAE grant for 1 year "to study mathematical physics with professor Laue in Zurich, with Einstein and Planck in Berlin, with Voigt in Gottingen, and with Sommerfeld in Munich."[14] Palacios's application was accepted, but ultimately he did not use it, possibly because of the uncertainty related to the beginning of the "Great War," or perhaps because he foresaw the possibility of obtaining a permanent position at the university. On 27 February 1916 Palacios applied again for a grant, this time "to study specific heats at low-temperature in Kamerlingh Onnes' laboratory in Leiden (Holland), as well as in Germany if the circumstances allow it." Furthermore, he asked for 400 francs monthly during 2 years, beginning the following October.

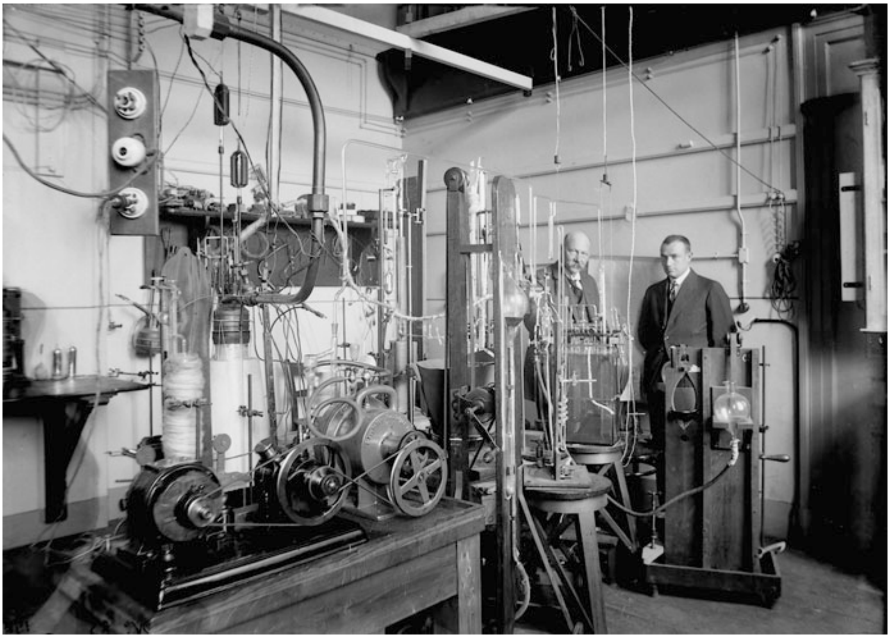
What made Palacios change his mind, from mathematical physics with Laue, Einstein, Planck, and Voigt, to low temperatures with Kamerlingh Onnes? Probably because soon after he asked the JAE for a grant to go to Leiden, on 4 March 1916, he became the holder of the chair of Termología, the name given then in Spain to the branch of physics that in due course would be named thermodynamics, at the Sciences Faculty of Madrid University, and low-temperature physics was one of the subjects with which Palacios should have been familiar as a new professor, although certainly not at the level of sophistication studied at Kamerlingh Onnes' laboratory. The process for obtaining the professorship included a competition (*oposición*) among different candidates, but Palacios must have been reasonably sure of the high probability of being successful. In fact, the members of the jury of the *oposición* included Blas Cabrera, who had accepted Palacios at the Laboratorio de Investigaciones Físicas and supervised his doctoral dissertation, Ignacio González Martí, with whom he worked as an assistant at the Sciences Faculty, and Esteban Terradas, who had been one of the professors of Palacios at Barcelona and to whom he dedicated his doctoral dissertation.[15] A revealing fact about the situation of physics in Spain is that Palacios obtained the professorship with no publications (beyond his doctoral dissertation, which following the tradition had been published, but only privately).

However, Palacios' request was granted, initially for 1 year, and he travelled by sea to Holland. On 16 February 1917 he wrote to Ramón y Cajal, the President of the JAE, to report on his work as the rules of the JAE required. He had arrived in Leiden at the end of October and had registered at the Natuurkundig Laboratorium der Rijks Universiteit te Leiden, where "after having learnt the methods of work and the ways to handle the apparatus of the laboratory," he had "determined the temperature coefficient of an aneroid barometer and practiced the use of the helium thermometer at low temperatures." Furthermore, he told Cajal that he was

[14] Junta para Ampliación de Estudios Archive, Residencia de Estudiantes Madrid. All quotations from applications to the JAE are taken from this archive.

[15] The other members of the jury were Juan Flórez y Posada (president), Alberto Inclán y López (secretary), and Alberti Inclán López; none of them did anything of scientific interest. Terradas's chair at Barcelona was of Acoustic and Optics (he held it from 1907 until 1927, when he went to Madrid).

"following Crommelin's works about the isotherms of neon, published in the 'Communications' of this laboratory," and that the director of the laboratory, Kamerlingh Onnes, had suggested to him "to extend his stay so as to be able to perform a work of some importance, probably thermometry with neon, and gain the expertise to pursue this research upon my return to Spain, as well as to offer research themes to my students."[16] Therefore, Palacios asked Cajal for an extension of 1 year. In another document he sent to the JAE officials, Palacios was a bit more specific, mentioning that he was also doing thermometry with helium, and that he planned to measure specific heats at low temperatures. His request was granted.



[16] Claude August Crommelin (1878–1965), a member of an aristocratic family, held the post of supervisor in Kamerlingh Onnes' laboratory, from 1909; previously, Willem Hendrik Keesom had occupied the post. Keesom, however, had just left the laboratory in 1917, to become teacher of physics and physical chemistry at the Veterinary School of Utrech. He returned in 1923, as successor, together with Johannes de Hass, of Kamerlingh Onnes when the great man retired. According to Kamerlingh Onnes, the obligations of a supervisor included "managing the day-to-day supervision of the junior staff, ensuring that equipment and accessories receive the proper care, [and] arranging all the supplies–in a word, the overall technical management" (van Delft 2007: 344). Besides his work with Kamerlingh Onnes, Crommelin initiated in 1928 the real foundation of the Boerhaave Museum at Leiden (see also Crommelin 1939, 1951).

Taking into account that Palacios' experience in low-temperature physics research was nil, Kamerlingh Onnes' interest in extending his stay for 1 more year must be understood in the war context that had drastically stemmed the flow of foreign visitors to his laboratory. According to van Delft (2007: 531–532): "only an occasional researcher now came to work in Leiden, too few to sustain the cryogenic laboratory's international status. Verschaffelt, an old friend, remained until 1919 to perform a series of experiments on the viscosity of liquid hydrogen, using a torsion pendulum with a ball. A.L. Clark from Toronto stayed for a few months and published some work on critical phenomena with Kuenen. Finally, Julio Palacios Martínez of Madrid stayed for two years, determining isotherms of neon, hydrogen and helium." In other words, and as stated in the introduction, it was not only Palacios, and through him, Spanish physics, who profited from his stay in Leiden. Kamerlingh Onnes' laboratory also took advantage of the Spaniard working with them. In fact, it is not only the "scientific periphery" which benefits from having access to the "centers:" "centers" also take advantage from the interaction with the periphery. Indeed, such benefits continued all throughout the twentieth century, associated with the so-called brain drain that many scientifically underdeveloped countries suffered when their most talented young scientists traveled abroad to more scientifically advanced countries to improve their knowledge, never to return.

Palacios profited from the situation: during the 2 years he stayed in Leiden, his research led to the publication of one article together with Crommelin and Kamerlingh Onnes and three with Kamerlingh Onnes.[17] Also, at the VII Congress of the Asociación Española para el Progreso de las Ciencias (Spanish Association for the Advancement of Sciences), held in Bilbao, 7–12 September 1919, Palacios presented a communication signed together with Crommelin, concerning the superconducting state of metals, that was published afterwards in the *Anales de la Sociedad Española de Física y Química*: their presentation at Bilbao offered Spaniards a splendid introduction to the discovery and attempts of a theoretical interpretation of superconductivity.[18]

Obviously, these publications, the very first in Palacios' curriculum, signed with the great man of low-temperature physics, the discoverer of superconductivity (1911), and Nobel prize winner for physics (1913), and one of his more notable

[17] Crommeling, Palacios, and Kamerlingh Onnes' article was published both in the *Koninklijke Akademie van Wetenschappen te Amsterdam* and in the *Communications from the Physical Laboratory at the University of Leiden*; also, it was translated into Spanish and published in the *Revista de la Real Academia de Ciencias Exactas, Físicas y Naturales de Madrid* (Crommelin et al. 1919a, b). Of the three articles written with Kamerlingh Onnes, two were published in the *Archives Néerlandaises*, the third being a translation into Spanish of one of them, published in the *Anales de la Sociedad Española de Física y Química*, the official journal of the Physics and Chemistry Spanish Society, founded in 1903 (Kamerlingh Onnes and Palacios Martínez 1922; Palacios Martínez and Kamerlingh Onnes 1922, 1923).

[18] Crommelin and Palacios Martínez (1920). The Spanish Association for the Advancement of Science, an organization that followed the model of the European and American Associations for the Advancement of Science, was founded in 1908. Its first congress (usually biannual) took place in Saragossa.

collaborators, were a splendid introduction card when he returned to Madrid. From then onwards, Palacios' career turned him into that of a scientific leader. Simultaneously, his social status increased significantly, especially after the end of the Spanish Civil War (1939). In 1953, for example, he became a member of the Real Academia Española, the most prestigious institution in Spain and the Spanish equivalent to the Académie Française.[19]

Palacios stayed in Leiden until the end of the First World War. Indeed, as the JAE documents related to his grant reveal, he had to postpone his return for 2 months because of transportation difficulties. Actually, he travelled in the first train that crossed the Belgium frontier after the Armistice (Cabrera 1932: 68).

10.6 Palacios and the Introduction of Quantum Theory in Spain

Besides learning low-temperature physics, Palacios benefited in other ways from his stay in Leiden. Although no documentation has survived, it seems that he followed courses by Hendrik A. Lorentz, who after retirement in 1912 continued to deliver his Monday morning lectures at the university on current problems in physics, and by Paul Ehrenfest, Lorentz's successor. It might have also been that Palacios attended some of Ehrenfest's famous colloquia, held on Wednesday evenings.[20] Probably thanks to this training, Palacios became one of the pioneers in the introduction of quantum physics in Spain.[21]

One of his activities in favor of the introduction of quantum theory in Spain was to translate into Spanish Fritz Reiche's book *Die Quantentheorie. Ihr Ursprung und ihre Entwicklung* (Reiche 1921). Palacios's translation (Reiche 1922a) appeared the following year in a series directed by Esteban Terradas, to whom, as it was pointed out before, he was closely related.[22]

Fritz Reiche (1883–1969) earned his Ph.D. with Max Planck at Berlin in 1907 and became professor of physics at Breslau in 1921. In his 1921 book he summed up the state of quantum theory.[23] In the words of Clayton Gearhart (2010): "Reiche's book was comprehensive in its coverage, clearly written, and pitched at

[19] In contrast to some of his colleagues (Cabrera, Martínez-Risco, and Arturo Duperier, for instance), and as I will explain later, Palacios sided with the victors of the Spanish Civil War, although afterwards, and because of his support of the monarchy, he had problems with the general Franco dictatorship.

[20] Ehrenfest taught regularly two courses, alternating between electromagnetic theory and statistical mechanics, dedicating most of the second semester of the latter to atomic physics and quantum theory (Klein 1989: 30).

[21] Concerning the introduction of quantum physics in Spain, see Sánchez-Ron (1987).

[22] Terradas was another pioneer in the introduction of quantum physics in Spain (Roca i Rosell and Sánchez-Ron 1990, and Sánchez-Ron 1987).

[23] About Reiche, who emigrated to the United states in 1941, see Bederson (2005).

a comparatively elementary level." It was quickly translated into English by Henry L. Brose, who had translated Arnold Sommerfeld's *Atombau*, and Henry S. Hatfield, and published in 1922 (Reiche 1922b), the same year in which Palacios' Spanish version appeared. In the same year the translations appeared an anonymous reviewer characterized the book in the following terms: "This is an admirable account of the whole field of quantum theory [. . .] the literature is very predominantly German, and it is customary in Germany to permit the publication of much more speculative ideas than is usual in other countries. The great merit of the present book is that it brings together all the threads of the argument and criticizes them, so that a just view can be obtained of the whole theory" (*Nature*, February 1922; quoted in Gearhart 2010). Remarkably, both the German and English editions are still in print, but not the Spanish. Thus, Terradas and Palacios's choice was a sound one; moreover, Reiche's book was the first comprehensive work discussing the quantum published in Spain.

Besides the translation, following his election to the Real Academia de Ciencias Exactas, Físicas y Naturales Palacios delivered a lecture (8 April 1932) in which he addressed quantum physics. Published in the same year (Palacios Martínez 1932), Palacios's lecture was a well-documented and quite rigorous review of most of the quantum topics of the time, including Heisenberg's, Schrödinger's, and Dirac's formulations of quantum mechanics. At the time, very few works had been published by Spaniards on the topic of quantum theory, because one of the characteristics of Spanish physics research during the period was its experimental bent. No theoreticians existed, or at least, no theoreticians who published original work. It is an open question if this characteristic was common to countries from the periphery which tried to participate in the quantum revolution that shook physics during the first third of the twentieth century.

10.7 Back in Spain: From Low Temperatures to X-Ray Diffraction

Once back from Holland in 1918, the natural choice for Palacios would have been to apply the knowledge obtained in Leiden and introduce low-temperature physics in the Madrid Laboratorio de Investigaciones Físicas, which, as mentioned previously, he joined again, at the same time that he assumed the duties of his university professorship. Indeed, Palacios wanted to take some glassware back to Madrid (van Delft 2007: 532); nevertheless, because the raw materials came from Germany, the Dutch instrument manufacturer told him that this would cause problems and advised him to take the glass along as hand luggage instead of sending it by ship. However, low-temperature physics was very demanding from the material and technological aspects, and there was no possibility of continuing working in that field in Madrid. Blas Cabrera made this clear when he received Palacios as a new member of the Royal Academy of Sciences (Cabrera 1932: 68): "After staying two years in Holland, Palacios [...] returned to his chair and to the Laboratorio de Investigaciones Físicas, where he had to follow new directions as he lacked the appropriate material to continue with the same type of experiments which he learnt in Leiden." The fact that Palacios had to give up low-temperature physics, the field to which he had been awarded a grant by the JAE, put him in a different class from most physicists who were supported by the JAE during its existence (1907–1938). Most of those whose scientific careers were established on a more or less permanent basis followed the path initiated abroad.[24] For example, the months Blas Cabrera spent in Zurich with Pierre Weiss in 1912 were crucial to his scientific career, not only because most of his research thereafter concerned the study of weakly magnetic substances, but also because he would join forces with Weiss in trying to prove the existence of the "Weiss magneton," which was, according to the French professor, the natural unit of molecular magnetism.[25] Something similar could be said about Manuel Martínez-Risco concerning what he learned with Zeeman. Therefore, Palacios's grant to Leiden was a failure in the science policy of the JAE.

The publications of Palacios immediately after he returned to Madrid concerned subjects related to the meniscus of mercury (Palacios 1919, 1920a, b, c; Palacios and Lasala 1922). As a matter of fact, these were topics familiar at Leiden, where considerable attention was paid to the variation of surface tensions with temperature using several cryogenic fluids. Thus, in a paper written by van Urk, Keeson, and Kamerlingh Onnes (van Urk et al. 1925: 958), we read:

> *Method and apparatus*. In order to determine the surface tension of liquid helium in contact with its saturated vapour we used, as did Kamerlingh Onnes and Kuypers in the case of hydrogen, the method of the capillary elevation in a narrow tube. To diminish irregular pressure differences at the open ends of the capillaries these were made comparatively short. Also it appeared desirable to have an immediate control as to whether or not the measured rise was the true rise corresponding to the temperature used. It often happened that gas bubbles rose in the narrow tube, causing the meniscus to oscillate for a considerable time and then to remain stationary for some minutes at an entirely wrong height.

However, surface tensions and meniscus were not a very promising topic, at least when studied at rather high temperatures, and consequently Palacios looked for other problems for his research. Thus, the possibility that he made good use of what he had learnt at Leiden was ephemeral, and no permanent research program in low-temperature physics was implemented in Madrid, despite the expectations of the JAE with their policy of grants. It is in this sense that we can say that Palacios'

[24] The commentary "in the cases in which those careers were established in a more or less permanent basis" is because in a number of cases those careers became stagnant: thus, Juan Cabrera, Blas's young brother, went (March–November 1922) to Paris with a grant from the JAE, to work with Maurice de Broglie in X-ray spectroscopy. However, he did not follow that line of research back in Spain as he became professor of Acoustics and Optics at the Science Faculty of the University of Saragossa, where laboratory facilities were almost inexistent. Something similar happened with Jerome Vecino, who also obtained a professorship in Saragossa.

[25] Weiss introduced the magneton in 1911 (Weiss 1911). For a study of the Weiss magneton, see Quédec (1988); referring to Weiss and Cabrera, Quédec (1988, 360) states: "Their joint combat on behalf of the magneton would last nearly thirty years."

stay in Leiden was a something of a failure, although certainly it helped him to become a better physicist. If he did not find in low-temperature physics a convenient site for a research program, he found one in X-ray diffraction.

A clue to the reasons behind the choice of X-ray diffraction was disclosed in the JAE biannual report for the period 1922–1924 (Memoria 1925: 178):

> *Works about X-ray diffraction and crystal structures*: The enormous importance acquired recently by this type of works, and the necessity to rely on the analysis of crystalline structure to complete Cabrera's researches on rare earths, justifies the necessity to establish in this Laboratorio an installation dedicated to X-ray spectroscopy, adequate to apply the methods of Laue, Bragg and Debye-Scherrer to the resolution of such problems. The necessary instruments are already on their way; but in the meantime, and with the purpose of getting the necessary experience in their manipulation, a provisional installation has been set up.

Indeed, Cabrera was instrumental in introducing X-ray diffraction in the Laboratorio. He had shown quite early that he knew and valued the new contributions of the Braggs in the use of X-rays to the study of crystal structures, publishing in the *Anales de la Sociedad Española de Física y Química* a detailed review article on that topic (Cabrera 1915). As a matter of fact, Cabrera was not the first in Spain to pay attention to X-ray diffraction: soon after the foundational articles of the Braggs, Francisco Pardillo, a professor of crystallography and mineralogy at the Sciences Faculty of Barcelona, had published an article in the *Boletín de la Real Sociedad de Historia Natural* in which he discussed the Braggs' achievement (Pardillo 1913).[26] As X-ray diffraction offered Spaniards versatility (not only physicists were interested in it) and technical accessibility (the instruments needed were not as sophisticated and expensive as those of low-temperature physics), it is not surprising that it was selected by the physicists of the JAE Laboratorio: in 1922, a Section of X-Rays was established at the Laboratorio de Investigaciones Físicas, with Julio Palacios as director. However, it was not until 1927 that he published his first article on this topic (Palacios 1927).

10.8 Bringing the Scientific Centers to Madrid, or "the Centers Come to the Periphery"

It is not the subject of the present article to study Palacios' scientific contributions to X-ray diffraction physics, but there is an aspect of how he promoted it in Spain that deserves to be considered because it reveals an important change in the science policy of the JAE.

In 1912, a group of Spanish emigrants who had settled in Argentina created in Buenos Aires an association named Institución Cultural Española (Cultural Spanish

[26] For more information about the introduction of X-ray diffraction in Spain, see Mañes Beltrán (not dated) and Sánchez-Ron (2013); a few details are also included in Ewald (1962: 502–503).

Institution).[27] At the time Argentina was experiencing a period of enormous prosperity: in 1914, her income per capita, which had been increasing about 6 % every year since the last decades of the nineteenth century, was the same as that of Germany and higher than that of Italy, Switzerland, or Spain. This situation attracted emigrants, and not surprisingly the Spanish colony reached about 1 million people. Economic prosperity paved the way to the promotion of culture, and nostalgia for their home country led Spaniards to create an organization whose main purpose was to invite to Argentina some of the most eminent Spanish intellectuals and scholars for short periods of time. This move was, of course, a way to keep contact with their mother country, and simultaneously, to help promoting their own cultural status as viewed by other Argentinians, inasmuch as the eminence of the visitors could help raise the respectability of their country.

To comply with the first article of the Institución statutes ("The Institución aims to raise awareness in the Republic of Argentina of the researches and scientific and literary studies made in Spain"), the main activity of the association was initially to invite, following the advice of the JAE, eminent Spanish scholars to spend several weeks, or even months, in Buenos Aires, delivering courses and lectures. Among the scientists invited (there were also scholars from the social and human sciences) were the mathematician Julio Rey Pastor (1917), the physiologist August Pi i Sunyer (1919), Blas Cabrera (1920), the neurologist and psychiatrist Gonzalo Rodríguez Lafora (1923), the chemist José Casares Gil (1924), the histologist Pío del Río Hortega (1925), Esteban Terradas (1927), and Enrique Moles (1930), the second authority, after Cabrera, at the Laboratorio de Investigaciones Físicas.

In 1922, one decade after its foundation, following the retirement of Santiago Ramón y Cajal, the Institución Cultural Española decided to enlarge its activities, with the creation of a chair, the "Cátedra Cajal," to support scientific research in Spain. This chair enabled inviting distinguished foreign scientists to spend extended periods of time in Madrid, helping Spaniards to learn and develop the new techniques mastered by visitors. Again, the JAE would be in charge of selecting foreign scientists.[28]

An extremely interesting document kept at the JAE archives is a letter sent by José Castillejo, JAE secretary, to Avelino Gutiérrez, the president of the Institución Cultural Española, on January 7, 1926. Castillejo explained to Gutiérrez who were the invitees considered so far.[29] The first was Richard Willstätter, the organic chemist and 1915 Nobel Prize of Chemistry, who refused, not surprisingly, on the grounds that he could not abandon his research. The next invitation was sent to Ernest Fourneau, the great name of the French therapeutic chemistry, working at Pasteur Institute. When Castillejo wrote to Gutiérrez, negotiations were still taking place, but the hopes that he would accept were few. The American biochemist Donald van Slyke was also being considered.

[27] About the Institución Cultural Española, see Roca i Rosell and Sánchez-Ron (1990, Chap. 4), Formentín Ibáñez and Villegas Sanz (1992), and Lago Carballo (2008).

[28] The announcement (December 1, 1922) and conditions are reproduced in Anales (1948: 339–376); also in Sánchez-Ron (2013: 106–108).

[29] This document is reproduced in full in Sánchez-Ron (2013: 136–143).

Physics was chosen following the failure of the former invitations. The *Anales* of the Institución Cultural Española corresponding to the period 1926–1930 (Anales 1953: 662–664) justified this choice not only on the grounds of "the number and fundamental character of discoveries in physics during the last 30 years, but because they originated transformations in the conceptions men have of the world which we live in." Among the various branches of physics, X-ray diffraction was selected because "the recent development (1913) of high-frequency spectroscopy (X-rays) had revolutionized crystallography and unveiled a new world of detailed knowledge about the molecular and atomic structure of solids, and Spain had already a group of professors and researchers with works on the field, thus justifying the subject's promotion."

Instead of the 3 years initially suggested, Cajal proposed a 6-month contract per year, and to associate each foreign scientist with a Spaniard working on the same specialty. Julio Palacios was the Spanish specialist chosen, and Paul Scherrer, of the Zurich Eidgenössische Technische Hochschule, the foreign scientist. Scherrer, who spent October and November 1928 in Madrid, coming back in March 1930, was a good choice: he established deep and extended relationships with the Madrid physicists, helping in the establishment of what would become in due course a rather prosperous school of X-ray diffraction and afterwards of solid-state physics in Spain.[30]

Actually, Scherrer was not the only X-ray specialist who visited Madrid. In 1930, Axel Lindh (an assistant of Manne Siegbahn, who later succeeded his mentor as professor of physics at Uppsala) came to teach the spectrographic techniques used by Siegbahn; and Jean Thibaud, who had worked since 1924 in Maurice de Broglie's laboratory and who was known specially for his measurements of X-ray wavelengths by means of a ruled grating, came to teach the application of X-ray diffraction to the study of grass acids structures. In 1932, Raimund Wierl and J. Hengstenberg, both members of Herman Mark's laboratory at the I.G. Farbenindustrie A.G. (Ludwigshafen) also visited Madrid. Wierl stayed 2 weeks to explain some topics of electron diffraction, and Hengsterberg stayed for 6 months. In the Instituto Nacional de Física y Química (National Institute of Physics and Chemistry), the successor of the old Laboratorio, built with the help of the Rockefeller Foundation and officially opened in 1932, he helped technicians in the construction of a Weissenberg camera, delivered lectures, and collaborated with Spaniards in several projects related to the determination of crystal structures through electron diffraction. Palacios was among those who profited from such interactions (Hengstenberg and Palacios 1932; Palacios et al. 1933). In March and April 1933, William Lawrence Bragg visited Madrid for 2 weeks, delivering several lectures; one of his collaborators in Manchester, J. West, stayed 6 months (January–June 1933).[31]

[30] See, in this regard, Sánchez-Ron (2013). Palacios wrote an article with Scherrer (Scherrer and Palacios 1928).

[31] A resumé of one of Bragg's lectures was published in the *Anales de la Sociedad Española de Física y Química* (Bragg 1933).

10.9 The Spanish Civil War and Its Consequences: Interactions Within the Periphery (Spain and Portugal)

Soon, however, in 1936, a civil war began in Spain that lasted 3 years. Julio Palacios was one of the few physics professors of the Sciences Faculty who stayed in Republican Madrid, although he never sided with the republicans. Actually, after the war he claimed that he had been an agent of the secret information agency in the Republican territory, the Servicio de Información y Policía Militar, “Information Service and Military Police,” of the Franco forces. As a distinguished scientist who had refused to support the republicans, Palacios was offered important positions by the victors. In March 1939 he was named vice-rector of Madrid University, a post which he occupied until March 1944, when Franco’s government confined him to Almansa, a small town in the southeast of Spain, after he signed a manifesto in favor of don Juan, the heir of Alfonso XIII, who had left Spain after the establishment of the Second Republic in 1931. Palacios, a fervent monarchic, favored the reestablishment of the monarchy in Spain and the return of don Juan.

His confinement ended in December 1944. In view of the difficulties he faced in Spain, Palacios decided to accept the invitation forwarded by Francisco de Paula Leite Pinto in 1947, on behalf of the Portuguese Instituto para a Alta Cultura (Institute for High Culture), an institution created in 1936. Dependent from the Ministério da Educaçäo Nacional [Ministry of National Education]), it aimed at the promotion of high culture, scientific research, and foreign relations in cultural matters, as well as the dissemination of Portuguese language and culture.[32] Palacios opted for Portugal not only for political reasons but also because he was married to a Portuguese woman, Elena Calleya, who had spent the war years in Portugal together with their three daughters. Moreover, since January 1944, he had established scientific relations with Portuguese scientists, having spent 8 days delivering a short course at the Sciences Faculty of Lisbon, and lecturing also in Oporto. The visit was a success: the Spanish consul at Oporto informed the Spanish Foreign Ministry that Palacios’ visit contributed to the prestige of Spain.[33]

The dossier held at the Madrid Sciences Faculty containing the administrative details of Palacios’ university career shows evidence that his relationship with Portugal were increasing: on 26 July 1946, he was authorized “to travel to Portugal during the present vacations”; finally, on October 28, 1947, he obtained permission to spend in Portugal 1 year, so as “to deliver a course at the Lisbon Sciences Faculty, upon invitation of the Portuguese government.” Such authorization was

[32] Francisco de Paula Leite Pinto (1902–2000) was a multifaceted man: in Lisbon he studied mathematics and geographic engineering, in Paris civil engineering and astrophysics, a subject in which he earned a doctoral degree. Back in Lisbon, he obtained (1940) a chair at the Technical University, of which he would became rector in 1963–1966. He was minister of Education between 1955 and 1961.

[33] Quoted in Rodríguez López (2002, 375); this reference contains (pp. 366–378) a good exposition of Palacios’ situation in Spain after the end of the civil war. See also Villena (1985).

renewed every year until 1961, when he retired from his Madrid professorship, which he had been allowed to retain throughout all these years.[34] As a matter of fact, the Portuguese invitation was welcomed by the Spanish authorities, as it allowed them to get rid of monarchic Palacios in a "civilized" manner.

During the years he spent in Lisbon, Palacios directed the Center for Studies of Physics of the Sciences Faculty, the section of Metrology of Radiations of the Portuguese Institute of Oncology, the Laboratory "Lopes Rego" of Applications of Radioactive Isotopes, and the Laboratory of Atomic Physics of the Portuguese Commission of Nuclear Energy Studies. He published a series of articles on topics such as radioactive isotopes, galvanic batteries, ultrasound, and electrostatic energy of atomic nucleus, some in the journal of the Lisbon Sciences Faculty and in the Bulletin of the Sociedade Portuguesa de Radiologia Médica (Portuguese Society of Medical Radiology), a society founded in 1931.[35] Therefore, he abandoned the field of X-ray diffraction.

Several elements contributed to that change. First, in spite of the official prominence given to Palacios in Spain after the war, he was not welcome in the new organizational structure for physics research. The laboratory of the JAE in which he had worked had been located since 1932 in a new building, the Instituto Nacional de Física y Química, and was dependent now on the Consejo Superior de Investigaciones Científicas (Higher Council of Scientific Researches; CSIC), the institution which was created in 1939 to replace the old Junta. It was then dedicated mainly to physical chemistry. Representative of what the authorities of the new CSIC thought about Palacios's work is the statement voiced by its powerful General Secretary, José María de Albareda, a soil scientist and member of the influential Catholic group Opus Dei. He wrote in a private note: "Of little interest [*van muy trilladas*] are Palacios' crystalline lattices."[36] Even so,

[34] After 1953, he shared his time between Madrid and Lisbon, spending a half month in each of the two locations. In the Palacios' dossier at the Madrid Sciences Faculty, there is an interesting letter that Palacios sent to the dean of the Madrid Faculty, Maximino San Miguel: "Since I went to Lisbon invited by the Institute of High Culture," he wrote, "at the end of every academic year the question of my return to my chair in Madrid has been brought up, and always has arisen the difficulty of interrupting [in Portugal] the works in progress, or of completing the training of my collaborators till they could substitute me. For this reason, and answering to the request of the Portuguese government, our Ministry of Education has agreed that my contract with Lisbon University be extended" (September 11, 1953). However, Palacios went on, "during my recent travels to Madrid, both the Ministry and our Rector suggested that I resume my courses in Madrid." To that end, it was proposed that "benefitting from the facility of communications between Madrid and Lisbon, I remain in each place the half of every month and deliver in that period the lessons corresponding to the full month." "That solution," he added, "has been endorsed by the Portuguese authorities, and therefore you can count with my services at the Faculty for the next academic year."

[35] For Palacios' list of publications, see González de Posada (1993: 33–53). According to this reference, Palacios published eight papers in the journal of the Lisbon Sciences Faculty, and two in the Bulletin of the Portuguese Society of Medical Radiology. The Portuguese scientists who co-authored papers with him were A. M. Moreira, F. Barreira, and A. M. Baptista (with the last he published nine papers, not confined to Portuguese journals).

[36] Quoted in Sánchez-Ron (1992a: 68).

Palacios was allowed to work with a small group of collaborators in a section of the CSIC, the Instituto Alonso de Santa Cruz, which shared with other departments the building of the former JAE Institute (the group was reorganized when Palacios went to Portugal).[37] By then, however, he had lost interest in X-ray diffraction, being attracted by the application of physics to medicine, at least since 1930. In fact, he wrote a booklet *Complementos de Física para medicos* (*Physics Supplements for Physicians*) (1930) to help medicine students in the physics course, and which appeared in a new extended version in 1942, with the title *Física para medicos* (*Physics for Physicians*) (Palacios 1930, 1942). This shift may have played a role in his election as member of the Real Academia de Medicina (Royal Academy of Medicine). He delivered his inaugural lecture on 15 March 1945, on *De la miopía y de la presbicia nocturnas* (*On the nocturnal myopia and presbyopia*; Palacios 1945), a topic in which physics (optics) and medicine were well mixed.

As in the case of other physicists (notably Francis Crick), his medical-biological interests were furthered when he read Erwin Schrödinger's book *What is Life*? (Schrödinger 1944). In a little book published in 1947 with the significant title of *De la física a la biología* (*From Physics to Biology*), in the preface Palacios (1947: 9) wrote: "This publication has been suggested to me by reading the book entitled *What is Life*?, written by Schrödinger." As Palacios acknowledged, Schrödinger "was well known among Spanish physicists, because he had been our guest in different occasions and has given us information on his publications." The creator of wave quantum mechanics visited Spain for the first time in the summer of 1934 and delivered a course on "The new wave mechanics" (Schödinger 1935) at the Summer University of Santander, and the following year he returned, this time to Madrid, where he met several physicists of the JAE, including Palacios.[38]

However, Palacios was unable to take the route from physics to molecular biology or bacteriophages as had other physicists, such as Crick, Max Delbrück, or Leo Szilard, inspired, at least in part, by *What is Life*? He could, however, profit from his previous knowledge of X-rays and apply it to problems of protection against radiation, a subject on which he published, while still in Madrid, a short work (35 pages) in collaboration with Carlos Gil y Gil titled *La protección en el manejo de los aparatos de rayos X y sustancias radiactivas* (*The protection in the use of X-ray machines and radioactive substances*) (Palacios and Gil y Gil 1947). He also delved into radioactive substances, one of the fields to which he dedicated himself when he settled in Portugal.[39] His ability to move to metrology of radiation,

[37] See Martínez Ripoll (2009: 228–229).

[38] Schrödinger's relationships with Spain are explained in Sánchez-Ron (1992b). Palacios' case adds more evidence to what Gunther Stent (1992: 3) wrote: "Shortly before the end of World War II the great Austrian physicist, Erwin Schrödinger, then living as an anti-Nazi emigré in Ireland, wrote the little book *What is Life*?, which was to draw wide attention to the dawn of a new epoch in biological research."

[39] See, for instance, Palacios (1949).

radioactive isotopes, and nuclear energy, fields in which he had not specialized in Spain, suggests that in countries of the scientific periphery, recognized scientists, and physicists in particular, can move from one field to another more easily than in scientifically developed countries, where specialization is enforced. Additionally, in the specific case of Palacios, he might have abandoned X-ray diffraction as he thought that he could not compete adequately with foreign groups. In the report that Charles Mendenhall, delegate of the Rockefeller International Education Board, sent the Board, after having visited Madrid in March 1926, to inform then about the situation of the JAE Laboratorio de Investigaciones Físicas (the Junta authorities had asked the Rockefeller Foundation to fund a new laboratory), he said[40]: "[Palacios] is working in the study of the structure of crystals through X-rays, [but] part of his equipment is rather inadequate. It seemed to me that he is not aware or able to follow the novelties in his field as [Miguel] Catalán." In the end, when we look at Palacios' whole career, we realize that his move from one area to another, from low-temperature physics to X-ray diffraction to medical physics, was often the result of various sorts of constraints, be these political or the restricted access to adequate scientific instruments.

10.10 Conclusions

The case of Julio Palacios illustrates the many ways in which original scientific research in countries belonging to the scientific periphery can be promoted, the difficulties to be faced, and the choices that may eventually lead to avenues with no future, as happened with Julio Palacios choosing to work in Leiden on low-temperature physics with Kamerlingh Onnes. At the same time, the Spanish-Palacios case shows with particular clarity how necessary it is for peripheral countries to rely on well-selected foreign help. Good science policy, provided in this case by the Junta para Ampliación de Estudios, an institution that did not survive the Spanish Civil War, reveals itself as particularly necessary.

There are, however, other aspects of the interactions between the peripheries and the centers that should be emphasized. It is not only the peripheries, the scientifically underdeveloped countries, which benefit from the centers: centers also profit from the peripheries. In the Palacios case, and as it was pointed out already, we may even claim that perhaps Kamerlingh Onnes' laboratory benefited the most from Palacios' stay. Without any expense on its part, the laboratory gained the workforce of a promising young physicist fundamental for the progress of Kamerlingh Onnes' research at a time when Leiden did not receive many visitors as a consequence of the war. The so-called brain drain of scientists that affected (and still affects) many

[40] Charles Mendenhall, "Report of Visit... in Madrid," 24 March 1926, pp. 1–2, International Education Board 1.2, 41.579, Rockefeller Archive Center, Pocantico Hills, Tarrytown, NY, USA.

peripheral countries all throughout the twentieth century is closely connected with similar situations.

Finally, Julio Palacios' career also illustrates an issue not yet much studied: the scientific interactions between different peripheral nations, exemplified in this case by the interaction between Spain and Portugal, two neighbor countries whose scientific status was similar in many respects. It is exceedingly important to enrich the history of science with more case studies that enable us to unveil the dynamics and effects of such interactions.

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Chapter 11
Beyond Borders in the History of Science Education

José Ramón Bertomeu-Sánchez

Abstract In this chapter, I explore the interactions between the new history of science education and the research agenda of the group "Science and Technology in the European Periphery" (STEP). While reviewing the contributions made by STEP members to this field, I discuss some missed opportunities and challenges faced by peripheral contexts in mainstream narratives of the history of science education. Many authors have called for cross-national studies and the application of a comparative approach to the history of science education, but studies of this kind are few. They require researchers to use sources written in several languages and to master a wide range of local studies and highly fragmented secondary literature. Multinational groups such as STEP are promising forums for promoting a project of this kind, but many barriers, and not only national borders, still persist. In this paper, I consider two interrelated problems: the tensions and connections between different bodies of scholarship in the history of science education, and the problems facing the construction of a global, decentred narrative in the history of science.

Keywords Historiography • Centers and peripheries • History of science education • Science textbooks

11.1 Introduction

Historical studies of science education have expanded enormously during the past two decades. Dramatic changes have taken place in relationship to narratives, protagonists, problems, sources, frameworks, and intended audiences. In 1975, in a review of a large selection of English-language studies, William Brock reported that most of the publications focused on educational institutions, state legislation, and education policies, and that many others were concerned with the lives of famous scientists in classrooms, either as professors or students. Many of these narratives were "dry as dust" (Olesko 2006), and most of them were written to

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celebrate the birth of famous scientists or the creation of academic institutions. Thirty years later, John Rudolph's review painted a completely different picture. The scope of the research topics had broadened beyond recognition to include issues such as the emergence of school disciplines, educational literature, school architecture and classroom design, material and visual culture, the changing biographical profile of professors and students, methods of evaluation (for example, oral and written examinations), and the role of political and economic powers in shaping education systems. In 1975 Brock had affirmed that the study of scientific manuals was a "potentially fruitful subject," and more than 30 years later Rudolph could claim that this area had been transformed into "a healthy sub-field of scholarship." Many recent publications confirm the growth and diversification of topics, sources, protagonists, and contexts.[1]

Focusing on a series of studies published during the past decade, I explore the connections between the new history of science education and the STEP project (Science and Technology in the European Periphery). While reviewing the contributions made by STEP members to this field, I also mention some missed opportunities, barriers, and challenges faced by peripheral contexts in mainstream narratives of the history of science education. In spite of the growing number of studies on this topic, most of the international literature about the history of science teaching focuses on traditional scientific centres.[2] Many authors have called for cross-national studies and the application of a comparative approach to the history of science education, but studies of this kind are few and far between; they require researchers to use sources written in several languages and to master a wide range of local studies and highly fragmented secondary literature. Multinational groups such as STEP appear to be promising forums for promoting a project of this kind, but many barriers, and not only national borders, persist. In this chapter, I present two interrelated problems: the tensions and connections between different bodies of scholarship in the history of science education, and the problems facing the construction of a global, decentred narrative.

One of the novelties in recent scholarship is the collaboration between historians of science and historians of education. During the past two decades, many members of these communities have taken up the challenge of crossing the disciplinary border, an academic migration that has been encouraged by new trends in both areas. The new social and cultural history of science has brought to the fore many unknown historical actors (teachers, publishers, readers, and audiences), while introducing (or expanding upon) topics such as material culture, translations, spaces, and scientific performance, which are closely related to educational practices. Many of the new studies have emerged from collective international projects. One of the first, launched by the European Science Foundation in the 1990s,

[1] See Brock (1975) and Rudolph (2008), quoted on p. 68. For other reviews see Bensaude-Vincent (2006), Olesko (2006), and Warwick and Kaiser (2005).

[2] A recent *Isis* focus on "Textbooks in the sciences" (Vicedo 2012) exemplifies this narrow geographic and chronological scope, which contrasts sharply with the aim to broaden and diversify the range of historiographic questions. The editor of the volume aims to "expand our vision of textbooks further" but three of four papers discuss only twentieth-century education in the USA.

involved more than 30 historians studying the development of chemistry textbooks between the last years of the eighteenth century and the first third of the twentieth, in a wide range of countries including Sweden, Spain, France, the UK, Germany, and Hungary, which stimulated a particularly productive comparative analysis (Lundgren and Bensaude-Vincent 2000). Many other projects on aspects of scientific education have been developed in recent years, producing special issues or collective volumes, but as yet we still lack a big picture.[3]

Historians of education have also paid attention to new issues, not only institutions and curriculum changes but also textbooks, teaching and learning practices, popular science, school disciplines, material culture, and educational spaces. The new studies have also included topics related to science and technology, traditionally ignored by historians of education, who have focused their research predominantly on humanistic disciplines. Unfortunately, as Rudolph remarked in 2008, the interaction between these studies and research in history of science has faced many barriers. The reasons are multiple, but one of the most important is the contrast between the national scale of many traditional studies of history of education and the aspiration for universality of traditional narratives of history of science (but which, in fact, covered just a reduced canonical group of scientists, institutions, settings, and problems).[4] These two traditional trends have been challenged by international collaboration and the introduction of comparative studies in the history of education, whereas historians of science have taken an increasing interest in local settings, practices, and circulation. Many STEP projects have offered new opportunities in this regard by bringing historians of science into contact with local groups working in the history of education, promoting the circulation of local studies across linguistic barriers, and encouraging comparative studies. But, as in many other fields of historical scholarship, the vast number of new contexts, actors, and problems, in conjunction with the micro-historical scale adopted in many of these studies, have restricted the emergence of a new master narrative based on recent research. This limitation may well be one of the main challenges facing scholars in STEP projects in the coming years, and not only in the history of science education. In the following pages, I suggest some possible areas for confronting these challenges described in studies carried out under the auspices of the STEP project. First, I show that the received hierarchy between centres and peripheries recalls the assumption of a top-down relationship between research and teaching. The next two sections review the two main areas of historical scholarship concerning science education: studies of science education for future scientists (a topic mostly studied by historians of science) and for the general public (mostly studied by historians of education). The last section offers some suggestions for

[3] See Kaiser (2005b), Bertomeu-Sánchez et al. (2006), and Vicedo (2012) and Simon (2013a, b) (on textbooks); Heering and Witje 2011 (on scientific instruments in classrooms); and Simon (2013c) (on cross-national and comparative studies in science education).

[4] On old and new transnational studies, see Turchetti et al. (2012). See also Simon (2012) and the new STEP group on "Cross-National, Comparative and Transnational History of Science, Technology and Medicine" in http://bdrupal.hicido.uv.es/?q=node/85

bridging the gap between the two areas. Finally, I discuss the significance of the history of science education to current debates about the role of science in classrooms.

11.2 The Laboratory, the Centre, and the Rest

Traditional images of the diffusion of knowledge have depicted peripheries as passive recipients of the research produced in the centre. Many STEP research projects and other studies have revisited this diffusionist view, but it nonetheless remains powerful both inside and outside academic circles. A similar conclusion could be reached concerning teaching, which has usually been regarded as a second-level activity compared with research: classrooms are portrayed as unexciting spaces in which anonymous teachers give more or less watered-down versions of the creative, exciting science produced at laboratory benches to bored students. "Moving pedagogy from the periphery to the centre" was the title chosen by David Kaiser in his introduction to one of the most influential collective volumes on this issue. No doubt the title is just an unintended coincidence, but it captures very well the neglect suffered by both classrooms and peripheries in the history of science. The issue is addressed more explicitly by Bernadette Bensaude-Vincent in her foreword to one of the collective STEP volumes on science education, in which she affirmed that "the subject of this volume is resolutely on the periphery of everything," not only in geographic terms but also because science education is not "usually considered central in scientific activity." For early historians of science, educational contexts were of interest only "in so far as they emphasize the divorce between science creators and the anonymous crowd of science transmitters," that is, between "creative science and expository science,"[5] just as peripheral local studies were used in past times to highlight the contrast between passive peripheries and creative centres. Thus, a project on peripheral scientific classrooms faces a double challenge in the form of the assumed hierarchies between the centre and the periphery and between research and teaching.

Similar to peripheral settings, science education is no longer pictured in terms of passive transmission of knowledge but as one of the chief spaces in which scientific knowledge is produced and transformed. This picture emerged during the 1990s thanks to several detailed studies of the areas most commonly examined by historians of science: Central Europe, France, Britain, and the US. One of these pioneering works was a detailed study by Kathryn Olesko of the teaching of physics in nineteenth-century Germany. She described science teaching as a "space where economic, social and political forces more strongly rush into the structure and function of scientific knowledge."[6] Moving away from old simplistic views, the

[5] Kaiser (2005b), p. 1; Bensaude-Vincent (2006), pp. 667–668.

[6] Olesko (1991), pp. 15–16.

new studies portray science teaching as a complex activity located at the intersection between scientific knowledge and education, always under strong social, economic, and political pressures, dependent on local factors, and changing over time and space. In another pioneering essay, John Christie and Jan Golinski (1982) pointed out that there is no linear relationship or established hierarchy between these ingredients; science teaching was conceived as a sort of non-Euclidean space with highly complex trajectories of interaction.

This complexity contrasts with the superficiality of the approach adopted in many studies on science education, which is still regarded as a low-profile topic by many professional historians. For instance, when discussing scientific instruments, historians and curators prefer to study and preserve "research" rather than "teaching" objects (a trend which is criticized by modern studies such as Heering and Wittje 2011, 2012). Many studies of scientific literature focus on academic journals in which the creative side of science is expected to be found. Apart from the most famous cases, many science teachers and textbook writers remained "illustrious unknowns" (Bensaude-Vincent et al. 2003). Even Adolphe Ganot (1804–1887), author of the most popular nineteenth-century physics textbook, which was translated into all major European languages and played a major role in shaping physics in the nineteenth century, has only recently been considered sufficiently important as to merit a biography (Simon 2011).

Writing biographies of teachers who carried out their work on the periphery and whose textbooks were never translated into major European languages is an even more challenging task. Only a very few textbooks (those of Lavoisier or Mendeleev, for example) have been analysed in depth, whereas many others, in spite of their historical interest, remain unexplored. Thus, as in the old-fashioned narratives of scientific discovery, the famous textbooks are portrayed as pathbreaking models and contrasted with earlier "pedagogically handicapped" textbooks lacking "a coherent structure."[7] These images are generally based on a combination of a condescending attitude towards past educational practices and an unconscious acceptance of the alleged dichotomy between the creation and reception of science. All these ingredients encourage the writing of narratives similar to those based on the centre–periphery dipole, in terms of novelties emerging in a select group of places and spreading to the "scientifically backward" peripheral countries lacking the "coherent structure" of scientific institutions (that is, those common in the centres). The STEP project offers an excellent opportunity for revisiting these questions, not only by foregrounding a new range of actors, contexts, and educational environments and taking advantage of collaborative work, the cross-national dimension, and a comparative approach to history, but also by enlisting historians in the common task of subverting received dipolar views of the centre and peripheries and of the hierarchies between research and teaching.

[7] The quotation is taken from Gordin (2012), p. 96, when comparing Mendeleev's *Principles of Chemistry* with a textbook written by an illustrious unknown, the French chemist Auguste Cahours. Many other examples could be mentioned. See Bertomeu-Sánchez et al. (2002, 2003).

11.3 Teaching Science to Scientists

Many studies of the history of science education have been inspired by the works of Ludwig Fleck, Thomas S. Kuhn, and Michel Foucault and their analyses of the emergence of "styles of thought," "paradigms," or "disciplines." In developing these ideas, new studies have enriched, nuanced, and, in many cases, contested the views of these authors on many aspects of the training of scientists and the reproduction of scientific communities. Kuhn, for instance, regarded textbooks as uncontroversial vehicles of "normal science" because they "define the legitimate problems and methods of a research field." But many later studies have revised this idea of textbooks and have shown how contemporary debates were included in many of these works (Bensaude-Vincent et al. 2003). Further examples are provided by one of the STEP edited volumes, which includes a section on textbooks and the chemical revolution in Portugal, Spain, and Italy. These papers highlight the different perceptions of the novelties (as radical revolutions, significant improvements, or just minor changes in a larger theoretical corpus), the selective appropriation of particular aspects, and the rejection of many others producing a creative adoption of novelties in the classrooms. What some historians might regard as opposed or even "incommensurable" theories were reconciled in late eighteenth-century Iberian textbooks. The study of Italian textbooks showed how different authors perceived the scope and value of the novelties and reflected their reactions, which ranged from acceptance to resistance and rejection. The background and interests of teachers and students both become relevant (Bertomeu-Sánchez et al. 2006).

Just as diffusionist studies on centres and peripheries, historical narratives of science education have tended to ignore the perspective of the recipients. Most of the research has focused on the teaching itself rather than on the various practices of learning: note-taking, sharing notebooks, learning by heart, performing home experiments, reading textbooks and encyclopaedias, managing visual culture, or preparing examinations. These different practices of learning, reading, and experimenting are elusive to historians, but many sources, for instance, students' notebooks, remain largely unexplored in peripheral archives.[8]

Studies on research schools offer further examples of long-term attention to science education. Historians have analysed how theoretical and practical knowledge is transferred from master to pupils, not only in university classrooms but also in practical work with colleagues, seminars, and informal discussions at the laboratory bench. These issues have been well analysed in France, Germany, or England but there may be much more to be said about other contexts and periods. A study of the emergence of biochemistry in Portugal shows the role of foreign scientists as charismatic leaders, the strategies for gaining local support and international authority, the interactions with the different institutional settings (universities and

[8] On students' notebooks see Waquet (2003), García-Belmar and Bertomeu-Sánchez (2010a), Brutter (2008, 2011).

research institutions), and the broad range of permanent or occasional collaborators producing a variety of structures at the different research schools (Amaral 2006). In a recent study of the Laboratory of Physics at the University of Lisbon from 1929 to 1947, the authors review how particular individual interests, local controversies, and politics shaped the development of research schools. They claim that these issues are more visible in the fragile institutional settings on the periphery, although they are by no means absent in the development of other research schools (Gaspar and Simões 2011).

One of the most recent STEP clusters is centred on the universities. A large amount of institutional history has been written in local languages, and the challenge for future historians is how to transform local studies into a general narrative while avoiding received views concerning the spread of "Napoleonic" or "German" institutional models. This subgroup aims to discuss the multiple roles played by universities (the education of intellectuals, legitimisation of academic disciplines, provision of social services and expert advice, and so on) by paying attention to new historical sources such as students' notebooks or lecturers' autobiographies.[9] Another recent publication concerning the emergence of higher technical education in Portugal also questions the idea of a passive, linear reception of institutional models by stressing the role of local conditions and preexisting academic traditions, which offers a new view of the origins of higher education in the European periphery in the early nineteenth century (Carolino 2012).

Further studies are also likely to show the complex transit of educational models (not only those supposedly coming from the centres) and how foreign institutions were represented and valued in different ways and selectively appropriated in different contexts, under the pressure of political agendas, imbalances of power, and economic constraints. In his recent study on Western-style higher education in Meiji Japan, Yoshiyuki Kikuchi (2013) adopts the idea of "contact zones" to escape from diffusionist narratives on the spread of Western science, while aiming to capture the unequal power relationships both at the international level (between imperial centres and colonies) and inside the classrooms (between teachers and students). He also brilliantly analyses many issues that have been part of the STEP research concerns during the past decade: spaces, translations (in both linguistic and cultural terms), and travels of learning.[10]

11.4 Science for the General Public

In contrast to historians of science, historians of education have focused primarily on the most elementary levels of teaching. Many studies have been devoted to the development of national systems of education, particular institutions, educational

[9] Further details in http://bdrupal.hicido.uv.es/?q=node/9 (accessed on 31 January 2014).

[10] On travels of learning, see Simões et al. (2003). Further discussion on these points is provided in Simon (2012).

policies, and the changes in curricula (more in relationship to humanities than to science). In recent years, the scope has been enlarged with the introduction of new topics such as spaces, material culture, and school disciplines, including what Rudolph (2008) calls "the collateral aims or purposes of science education:" the role of science in maintaining the social order, the changing relationships between science and religion, and the role of science education in creating the cultural boundaries of science and supporting the authority and credibility of scientists in many areas of the life of a society. Rudolph has offered a brilliant analysis of the different political, economic, and pedagogical factors which shaped the change of school curricula in the first half of the twentieth century in the United States, from the "life-adjustment curriculum" and "everyday physics" to a different model based on academic disciplines, which was encouraged by the political context of the Cold War years. The issues at stake were substantially different in the reform of the secondary school curriculum in Denmark during the late 1960s, when the main discussion was centred on the relationships between science and humanistic education. However, the study shows how "cartographies of science and education [. . .] were embedded in the social and political goals" of the protagonists.[11] Both these studies explore how school science, as is science popularization, was "involved in redefining the hegemonic ideology" in diverse ways in different places (Gavroglu 2012b).

Secondary education has received little attention from either historians of science (focusing on higher education) or historians of education (who have focused mostly on primary school science). And yet, secondary schools were crucial in the development of scientific education during the nineteenth century.[12] The growing authority of science in liberal education provoked a broad range of reactions among nineteenth-century intellectuals. Science might be associated with economic progress and cultural modernization, but it was also perceived as a threat to the traditional humanistic curriculum or to religion. In many cases, science lectures were used to naturalize ideas about society, to promote social control, and reduce political upheaval in the working classes (Donnelly 2002). In secondary schools, teachers faced the tensions between academic and school science, between the different goals, contents, and practices related to the teaching of science for future scientists or the general public. Secondary education was also a major force in the emergence of modern nations in the nineteenth century. Many studies have focused on the role of history, geography, and other humanistic disciplines in this process, but much remains to be explored concerning the role played by science and technology.[13]

[11] Lynning (2007), p. 506.

[12] For some exceptions to the general rule, see Belhoste (1995), Belhoste and Hulin (1996), Olesko (1991), Rudolph (2002, 2005), Shapiro (2012), Simon (2013c).

[13] The topic is scarcely mentioned in Harrison and Johnson (2009), but see Withers (2001), pp. 134–142 and 158–182.

Secondary education is also an ideal context for a discussion of the differences, exchanges, and tensions between academic and school disciplines. Historians of science have paid attention to the role of pedagogy in the emergence of new academic disciplines such as experimental physics or chemistry, but have largely neglected the ways in which these disciplines were accommodated inside the school environment or, as happened in many cases, how new areas of study emerged in schools, sometimes with few connections with specific academic disciplines. School disciplines are another example of the constrained creativity that took place in pedagogical environments. These disciplines emerged, developed, and sometimes disappeared in particular school settings but were generally shaped by regional or national regulations (curricula, degrees, academic schedules, educational spaces, etc.), which were also very influential in defining the biographical profile of teachers and students at different educational levels. In this context, school disciplines are the result of a broad range of unequal exchanges between different actors, inside and outside the educational institutions, sometimes inspired by academic disciplines or foreign educational experiences. These features make school disciplines a very interesting topic for comparative and transnational studies (Simon 2012, 2013c).

The continuities, tensions, and divergences between school and academic disciplines can also be analysed by paying attention to the overlaps between teaching and the popularization of science. When introducing the differences between esoteric and exoteric circles, and between journal, vademecum, and textbook science, Fleck (1981) also remarked on the exchanges and interactions among the different types of scientific literature. But, similar to many other studies, Fleck focused on the producers more than on the users. In fact, these genres of scientific literature imply shared assumptions that are locally dependent, changing over time and renegotiated by the protagonists who creatively challenge regular practices and introduce new uses and conventions. In his study of the many lives of Adolphe Ganot's books on physics, Josep Simon described the broad range of readers, from teachers and students to researchers, instrument makers, or just occasional leisure readers. These readers used Ganot's books for a broad range of purposes: as school manuals, as study aids for examinations or self-instruction, as general introductions to science, as symbols of French physics, as catalogues of scientific instruments, and even as ideological tools against the evils of scientific materialism. These appropriations contributed to the circulation of Ganot's book in esoteric and exoteric circles, blurring the borders between research, teaching, and the popularization of science (Simon 2009, 2011).

Not only books circulated in this continuous space between research, teaching, and popularization of science, but also specimens and instruments, visual aids such as diagrams, models, and wall charts, and experimental demonstrations combining oral explanation and performance. Inspired by the works of Foucault, historians of education have discussed how the contexts shaped (and were themselves shaped by) the practices and protagonists of schools. Historians of science have also produced a large number of studies on the spatial features of laboratories and science museums but, rather surprisingly, less attention has been paid to educational contexts. The geographic turn provides new opportunities for bridging these gaps and again the STEP network offers access to unexplored locations, sites, and historical sources for future research.[14]

The STEP focus on circulation and appropriation provides a suitable perspective to explore the connections and tensions between academic disciplines, school science, and different forms of popularization. Research, teaching, and the popularization of science are activities with imprecise and ever-changing borders, which, in most cases, share protagonists, texts, spaces, objects, and visual culture. For instance, the recent interest in popular science books written for children overlaps with studies of the history of science education in primary schools (Eddy 2010; Kohlstedt 2010; Tailrach-Vielmas 2011). Scientific performers and their audiences face similar problems both inside and outside classrooms (Morus 2010). The same can be said of material or visual culture: dramatic electric sparks, coloured fumes, exotic specimens, anatomical models, and optical wonders took their place alongside other visual displays in nineteenth-century classrooms as well as in popular lectures or international exhibitions. Models, graphics, maps, and other forms of visual representations are also common in manuals, popular books, classrooms, lecture halls, and science museums (Bensaude-Vincent and Blondel 2008; Berkowitz 2013). By exploring the changing forms and meanings of instruments in research, teaching, and popularization, historians can obtain interesting clues about the role played by experiments in these overlapping contexts (Keene 2007; Heering and Wittje 2011, 2012).

The examples show how recent studies of the popularization of science (Topham 2009; Papanelopoulou et al. 2009; Nieto Galan 2012) can provide valuable methodologies, fresh perspectives, and as yet unexploited sources for the analysis of both school and academic science. The reverse is also true, because the foregoing examples show that both the teaching and the popularization of science conveyed different images of the value and authority of science, for instance, a hierarchical relationship between science and technology, or the expected role of scientific experts in political decision making. Both practices could also serve to reify and naturalize gendered images, religious beliefs, hegemonic ideologies, and political ideas about social order (Rudolph 2008; Gavroglu 2012b).

11.5 The Point Is to Change It

The foregoing discussion has highlighted that the STEP group provides an ideal environment for future research on the history of science teaching. One reason is that "the issues related to local conditions [. . .] play such a dominant role" when

[14] See, for instance, Livingstone (2003) and Finnegan (2008). For a recent study on pedagogical spaces see Lourenço and Carneiro (2009), García-Belmar and Bertomeu-Sánchez (2010b) and Kikuchi (2013), Chap. 5. An ongoing project on "sites of chemistry" is http://www.sitesofchemistry.org/ (accessed on 12 June 2013).

concerned with school science and science popularization (Gavroglu 2012b). The STEP environment offers access to the materials needed for the enterprise: historical sources, case studies, unknown protagonists, interdisciplinary approaches, collective debates, and comparative perspectives with a promise of the emergence of a new large picture in the near (or maybe not so near) future. STEP can also bridge the gaps between different specialized histories of similar topics while at the same time taking the opportunities offered by crossing the blurred borders between research, teaching, and the popularization of science.

This review has shown that historians have interpreted the world of science education in various sophisticated ways, but the crucial point now is to change it. The conclusions of these studies shed light on the political debates on the future of science education currently taking place in countries all over Europe. Some of the participants in these debates are moved only by the goal of reducing public spending, in spite of the dramatic consequences of this policy in terms of social justice and cultural loss. But those who care about these issues and want to discuss the future of science in universities and schools would do well to introduce fresh historical perspectives in the debate. Moreover, a new history of science education could provide new opportunities for revisiting the role of the history of science in schools. Its pedagogical uses have been centered mostly on reconstructions of classical experiments (as a way to understand an imagined scientific method), scientific biographies (as models or encouragement for prospective scientists), the evolution of scientific ideas (sometimes as a way to dispel students' misconceptions), and, more recently, to appreciate the interactions between science and society. However, the uses of history of science in classrooms also have a history that has been sadly neglected and which could provide valuable clues about how the concepts of center and periphery had been reified, legitimated, and employed for many collateral purposes.

The studies of the history of science education reviewed here could be advantageously used as a starting point for the discussion of many political issues concerning scientific education; for instance, the kind of knowledge about science that the members of a democratic society should have. The history of science education can also provide clues about the tensions between "science for the scientists" and "science for the general public," particularly in secondary schools. Avoiding diffusionist and dipolar models, new STEP projects can also discuss the interactions between local pedagogical cultures and the transnational circulation of objects, ideas, and people. These projects can unveil the political agendas which are "formulated in terms of scientific entities or concepts that appear neutral in order to lend legitimacy to the politics involved" (Gavroglu 2012b).

To enter the debate requires new master narratives on the history of science education that are accessible to nonspecialist readers living outside the small community of historians of science. These narratives should avoid the standard dipolar models and hierarchies concerning either centres and peripheries or research and teaching. Adopting this approach, historians should record both local idiosyncrasies and general trends while paying attention to appropriations and circulations across disciplines and nations. The revised account should include famous teachers but also obscure scientists, teaching practices and learning technologies, exams and curricula, visual and material culture, instruments and models, and many other protagonists and ingredients described here. I hope this new picture will push forward the study of science education at STEP and will help to move STEP protagonists, problems, and contexts to the center of the history of science.[15]

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Part III
History and Philosophy of Science

Chapter 12
Probable Reasoning and Its Novelties

Ian Hacking

Abstract The historian A. C. Crombie identified six styles of reasoning in the history of the sciences. Crombie's idea, motivated by history, suggested to me a philosophical programme for thinking anew about Scientific Reason. My interest in the present paper is not to defend the older parts of the styles of reasoning project, but to develop a further aspect of the programme. In particular, this paper turns to the fifth style in Crombie's list, probable reasoning (or statistical inquiry). I shall show that one of the novelties connected with probable reasoning, is a new kind of object, the *population*. At the end of this paper I shall suggest many a novelty that accompanied the evolution of probability and statistics as a way of reasoning and finding out.

Keywords Styles project • Scientific reason • A. C. Crombie • Probable reasoning • Statistical inquiry • Concept of population

12.1 Introduction

I first got to know Kostas Gavroglu at a wonderful conference that he organized on the Island of Corfu in June, 1990. There I presented a stage in what I call my "Styles Project" (Hacking 1994). It seemed fitting, in this celebration of Kostas's life and work, to offer a paper that continues that theme. A more widely circulated version of the talk I gave at Corfu appeared as Hacking (1992a). The latest update on the project is Hacking (2012). Since that last paper recapitulates—with modifications—the project from the very beginning (Hacking 1982a), I won't do so here. I shall state the mere minimum before I turn to probable reasoning.

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12.2 Where It All Began

The styles project began when I heard the historian A. C. Crombie give a talk at a conference in Pisa, in 1978. Here is the central quotation:

> *The active promotion and diversification of the scientific methods of late medieval and early modern Europe reflected the general growth of a research mentality in European society, a mentality conditioned and increasingly committed by its circumstances to expect and to look actively for problems to formulate and solve, rather than for an accepted consensus without argument. The varieties of scientific method so brought into play may be distinguished as:*
>
> (a) *the simple postulation established in the mathematical sciences* (for short, mathematical)
> (b) *the experimental exploration and measurement of more complex observable relations* (for short, experimental exploration)
> (c) *the hypothetical construction of analogical models* (for short, hypothetical modelling)
> (d) *the ordering of variety by comparison and taxonomy* (for short, taxonomic thinking)
> (e) *the statistical analysis of regularities of populations and the calculus of probabilities* (for short, probable reasoning, or statistical inquiry)
> *and*
> (f) *the historical derivation of genetic development* (for short, historico-genetic or H-G for short)
>
> *The first three of these methods concern essentially the science of individual regularities, and the second three the science of the regularities of populations ordered in space and time.* (Crombie 1981, 284 (reformatted by the author))

Crombie usually called these six methods "styles of scientific thinking in the European tradition" (Crombie 1994), but they can be described as styles of reasoning (my initial choice), styles of thinking and doing (to emphasize that the reasoning is often a matter of doing something), genres of scientific inquiry, or simply ways of finding out (in the sciences). All of these modes of investigation are used in all the sciences, but they seem to me to be fundamentally different. They certainly have distinct historical trajectories.

I regard Crombie's list as a template, and do not claim it to be definitive, or that he characterized his European styles in the best possible way. For style (*a*) he emphasizes postulation, while I think the fundamental discovery was proof. I think the essential element in the event loosely characterized as the "scientific revolution" was the invention of the laboratory, which needs both experimental exploration and measurement, (*b*), and hypothetical modelling, (*c*). These are the two great intellectual revolutions singled out by Kant:

> In the earliest times to which the history of reason extends, *mathematics*, among that wonderful people, the Greeks, had already entered upon the sure path of science. [. . .] the transformation must have been due to a *revolution* . . . .
>
> Natural science was very much longer in entering upon the highway of science. [. . .] In this case also the discovery can be explained as being the sudden outcome of an intellectual revolution. (*Critique of Pure Reason* B, x–xii, original emphases)

I shall not dwell on the point, but Kant, who seems to have introduced the idea of a revolution in science, also saw rather clearly that the highway of science required that experimental exploration and hypothetical modelling must be combined, to form what we might call the laboratory style of thinking and doing.

12.3 From History to Philosophy

Crombie's words, motivated by history, suggested to me a philosophical programme for thinking anew about Scientific Reason. That is a topic in the great tradition that runs through Aristotle, Descartes, Leibniz, and Kant—and on to the present. I asserted that the theory of styles is at bottom conservative; it recycles ideas that have been in circulation for ages. Note how I have just used Kant.

Yet from time to time the upshot sounds quite radical, for example, I have proposed all along that the basic types of reasoning enumerated by Crombie answer to no standards other than their own. They are "self-authenticating": they set the canons of what we have come to call good reason. Many find this doctrine unsettling, and believe it introduces relativism; on the contrary, I hold that it is at the root of the kind of objectivity for which science strives.

My interest in the present paper is not to defend the older parts of the project, but to develop a further aspect of the programme. In particular, like most philosophers interested in the sciences, I have said a good deal in various places about Crombie's first group of three methods, but very little about members of the second group. This paper turns to method (*e*), probable reasoning (or statistical inquiry). Of course I have notoriously written too much about probability ("hacking his way through the statistical jungle" as Daniel Dennett once put it in a philosopher's dictionary). But I have said nothing about it in the context of Crombie's template. This is an occasion to do so. Some of my merely historical statements in what follows are virtually copied from one or another of my books, and I shall unabashedly not repeat the arguments and the evidence here.

I also claimed, in the paper prepared for the 1990 Corfu meeting, that each of Crombie's six genres of scientific thinking and doing comes together with a great many novelties, which may include new *types* of:

object
evidence
sentence, new ways of being a candidate for truth or falsehood
law
possibility
explanation

I used to say that each style "introduces" new kinds of evidence (etc.), but that was inapt. To speak that way suggests we have first the style and then the novelty. When Kostas introduced me to Aristides, first there was Kostas among my friends, and then there was Aristides. But it is not the case that first there was a new way to find out, and then there was a new kind of evidence. The new kinds of object, evidence etc. are integral to the style of scientific thinking and doing. I shall show here that one of the novelties connected with probable reasoning, is a new kind of object, the *population*. At the end of this paper I shall suggest many a novelty that accompanied the evolution of probability and statistics as a way of reasoning and finding out.

12.4 Differences Between the Two Groups of Styles of Scientific Thinking in the European Tradition

Crombie said, in the passage quoted above, that his first three styles involve individuals, while the second three involve populations. I do not strictly disagree but I want to change the emphasis and to broaden the panorama.

I will not bother to quarrel with someone who says that the first group—mathematics, experiment, hypothetical modelling and the laboratory—concern individuals while the second group, taxonomic, statistical and historical derivation—concern populations. I do have one small difficulty that I shall soon explain. My problem is that the concept of a population is not a freestanding concept in its own right. It is a companion of Crombie's style (*e*), probable reasoning. It is odd to characterize a difference among two groups of styles by means of a concept that essentially matured only with one of them.

Before explaining that, I want to insist on a fundamental difference between the two groups of ways of finding out, far more important than individuals/populations. It concerns truth. When I mention self-authentication, one naturally thinks of the sentiment that the ultimate test of a style is that it should lead us to the truth, which should be independent of how we find out about it. So self-authentication is a travesty! If the primary enterprise of the second group of styles is not truth, self-authentication will not arise in quite the same way, or may not be so surprising when it does.

Why does truth play a different role in the second group than the first? I shall exemplify with taxonomy and historico-genetic reasoning before proceeding to statistics. I should re-emphasize that each of Crombie's six is inherently different in nature, and that one cannot generalize from one to another. Each has its own personality, which is one of many reasons that I take them to be truly different.

12.5 Truth in Taxonomy

One aim of the mathematician, the experimenter, and the theorist, is to find out the truth. Someone who is classifying plants wants the right classification, but classifications are not usually said to be true or false. They are correct or informative or memorable or explanatory. I like to quote Darwin: "All true classification is genealogical." He immediately added: "But I must explain my meaning more fully. I believe that the *arrangement* of the groups within each class, in due subordination and relation to the other groups, must be strictly genealogical in order to be natural" (Darwin 1859, 420). A good classification is not a true proposition but the right arrangement.

Once a taxonomy has been established, there can be truths parasitic upon that arrangement. A child may think that jackrabbits and hares are rabbits. But that is not true; a helpful parent will teach that hares and rabbits are of different genera.

Having become a know-it-all adolescent, the same person may opine that there are two genera, one for rabbits and one for hares and jackrabbits. False again. Rabbits form a family of seven genera. But all that is a secondary use of truth, relative to an agreed system of classification.

Thus at the first level, a taxonomy is not favoured because it expresses true propositions. At a second level, there may be true or false statements relative to an agreed taxonomy.

The adjectives true and false are used in all discourse, and hence they are used in taxonomic thinking. I say only that taxonomic structures as a whole are not judged as true or false, but as better or worse, or, in Darwin's terminology (derived from Linnaeus) "natural".

12.6 Truth in Genesis

A similar observation holds in connection with historico-genetic reasoning. Consider theories on the origin of the Earth and how it got to be the way it is. Philosophers, including Leibniz and Kant, contributed to that body of speculation. If push comes to shove we can say that what they wrote is false, but that is not what we do say. We say they were imaginative, sometimes on the right lines, but in any event totally superseded. We do not say that the Mayan creation tales are false. One of them features a universe destroyed and our present one created out of the remnants of the old. It has four trees at each corner of the universe, and another, a tree of plenty, at its centre. It would be stupid to call that false. We do not say that *Genesis* is false either.

Confronted by an Intelligent Designer who thinks that Darwin was an evil influence, one may be forced to say that *Genesis* is not literally true. Truth and falsehood are not, however, the standards that are relevant. What I myself say is that the Theory of Natural Selection is an extraordinarily good explanation of the Origin of the Species and *Genesis* is simply too uninformative to be an explanation of anything.

We accept and teach natural selection because of the fine detail in which it explains how we got here, and constantly turns up new problems whose resolution teaches us even more about origins and processes of evolution. Of course Darwin aimed at the truth about the species. I say only that the philosopher's fetish of truth should not force us to categorize everything as true or false, when much richer kinds of evaluation are available in ordinary language.

We may make parallel observations about H-G reasoning in other fields. The origin of languages has been a very rich field of inquiry. Adamicism was once rife. It was a highly suggestive research program for tracing present languages back, presumably though Hebrew, to Adam's ur-language. Modern philology is a descendant of that project. Yes, we can, if forced, say that, "It is false that all human languages are descended from the language spoken by the First Man to the First

Woman." But that is blind regimentation: we do not have to force every thought into the straitjacket of truth or falsehood. We can do that, but why should we?

12.7 Truth and Probability

The relationship of probability to truth is more convoluted. One use of probability statements is to assess the credibility of other statements, that is, to say whether or not they are likely to be true. At the basic level of assertion, statements of what is credible are not assessed by their truth value, although as always we can jump a level and say that it is not true that so and so is credible.

Another aspect of probability complicates matters still further. The statement that this coin is fair is the statement that the probability of getting heads is one half. That can be true or false. The duality of probability is a mare's nest that we must shortly enter, but for the present we can be sure that the relationship of probability to truth is very different from anything we encounter in Crombie's first group of three genres of thinking and doing.

We do have criteria by which we judge statements of probability. Indeed for one kind of probability statement, the criteria are themselves probability statements so those statements are all too self-authenticating. For another class of probability statements, criteria are purely internal coherence, which again suggests we have over-stepped the bounds of respectable self-authentication. Probability starts to look suspiciously circular. We shall presently return to the details of this point.

All this has consequences for my most outrageous proposal, that styles of scientific thinking and doing are self-authenticating. I meant that a style does not answer to any criteria of truth except its own. But if a style of reasoning is not directly or primarily in the business of finding out the truth, then this radical claim fizzles out. But of course reasoning is not just for finding out the truth about something. We reason, for example, in order to make sense of something, to explain it. That is the primary route of historico-genetic inquiry

12.8 A New Type of Object: The Population

I thought it too quick to say that styles in the second group study "regularities of populations ordered in space and time." The word "population" is not innocent. Population is itself a novelty engendered within the statistical style. I am not about to preach a dull sermon on anachronism, but rather to use this as an example to illustrate the idea that styles are accompanied by novelties. I do insist that there was not first probable reasoning, and then the concept of a population. They evolved together from inchoate anticipations. A population is an example of a statistical object, no less than the mean and the variance. It seems counter-intuitive to propose that the very idea of a population was a novelty that arose within statistical reflection, so I shall pause to explain. This will also serve as a sneak preview of the statistical style of thinking and doing, which I also call probable reasoning. I confess that in what follows I lean on assertions to be found in Hacking (1975) and (1990), and I shall not repeat evidence for them here.

12.9 The Very Words, Population and Statistics

"Statistics" was a word that came into being to refer to collections of primarily economic facts about the state. Statistics = state-istics. These facts included climate, produce of crops, manufactures, and the sheer number of people. French and English historical dictionaries give us first recorded usage of the words *statistique* and "statistics" as 1785 and 1770 respectively. I can do better than that. German *Statistik* is much earlier. It named an academic subject, often written up in Latin. One of Leibniz's favourite older correspondents and mentor was Hermann Conring (1606–1681). Conring too was a polymath, one of whose fields of great knowledge is what we would now call political economy. He is often referred to as the founder of "University statistics." Thanks to his influence, German Universities became a hotbed of this branch of political economy. Hence a century later, a *Political Survey of Europe*, published in England in 1787, refers to "This science, distinguished by the newly-coined name of Statistics, is become a favourite study in Germany" (The *OED* cites this from a book by one A. W. Zimmermann). Goethe traveling in Italy in 1786 speaks of living in these "statistically minded times" (See Chap. 3 of Hacking 1990).

The English word "population" is of course Latin and involves people and peopling, but its meanings wandered about. They began to coalesce on the modern sense only at the time that the word "statistics" was coming into use. Dr Johnson's celebrated *Dictionary* of 1755 shows where the word was going. "The state of a country with respect to numbers of people." Only much later did it come to mean, as the *OED* puts it, "A totality of objects or individuals under consideration, of which the statistical attributes may be estimated by the study of a sample or samples drawn from it." The *OED* dates that as 1877, and the citation is Francis Galton, the very man who gave us regression towards the mean and the correlation coefficient.

12.10 Populations

Of course there had always been some loose idea of a population of people around. How could it be otherwise? As long as there has been counting, and so long as people have lived in sizeable social units, there has been an idea of how many people are in the group. The fourth book of the Pentateuch is called *Numbers*, and is full of enumerations. The very word "census" is Roman. Augustus extended the census to the Empire. That was why Jesus was born in Bethlehem. "There went out a decree from Caesar Augustus [...] And all went to be taxed, every one unto his own city" (*Luke* 2.1, 2.3).

The Roman censuses do not report the simple number of people in the population. We take that as a first question to ask, but have done so only since the maturation of statistical thinking. Despite the dense information that social historians extract from Roman censuses, it is very hard to infer the actual number of people. Very much the same thing is true of all the other large administrations for which censuses are known: ancient China, Egypt, or Persia. Early enumerations were for taxes and military recruitment. That was the purpose of old censuses: taxation and the army.

The census of a population, as we now understand the word, was in the first instance mostly a matter of modern colonial administration. Thus New Spain, New France, Iceland, and Virginia were the sites of early censuses. In the first instance, the Spanish, French and English censuses were to count colonists, not everyone, not natives. New France was much better counted than New Spain. Yet even much latter a distinguished French political economist—a conservative one—was saying that it made no sense to talk about the number of inhabitants of France itself because it was not a fixed number and people were always moving across borders. But you could count the number of young men, because they had to carry papers concerning military service about their person!

12.11 Togetherness

In short, our present concept of a population itself is a distinctly modern one, firming up in the eighteenth century due to changes in political administration and to statistical technologies. Population becomes a coherent concept only grace of statistical analysis and the calculus of probabilities. Conversely, statistical analysis becomes practicable by using the concept of population.

You may think this is a horrible chicken-and-egg problem. Instead it is a tidy example of what I shall call the *togetherness* phenomenon. The statistical style grew together with the concept of a population.

Historically, the European word "population" first referred to the inhabitants of a city or a state (be it a nation state or a province of one). Statistics originally meant the study and analysis of numerical facts about a state and its population. The two words, "statistics" and "population" travelled together, although each went at its own pace. They began with the state, but gradually applied to more and more varied types of groups, until the word "population" became an abstract term meaning *any* definitely specified collection of anything. Statistical mechanics, a mid-nineteenth-century science, is the theory of populations of molecules. Population genetics, which emerged in the 1920s, analyses how evolutionary processes such as natural selection, mutation, genetic drift, and gene flow affect a population such as a species.

I hope this long aside on populations has been a relatively painless lead into the hateful topic of statistics. These remarks about populations should also be a powerful indicator that styles are not just styles of *analysis* or *reasoning*, but also of *doing*. The potted summary of the past few pages illustrates the ampersand in my master label, styles of scientific thinking and doing in the European tradition. I did not say a word about what anyone thought, reasoned, or analysed. I did point at many things that political administrations *did*.

12.12 Janus-Faced Probability

Probability had two faces from 1650 on. They were once called subjective and objective. The former looks to credibility and degrees of belief. The latter looks to stable relative frequencies of events. Formally there is no difference between the two, for they satisfy exactly the same axioms. The credibility idea is nowadays often called Bayesian. Bayes' rule, or "theorem," is named after Thomas Bayes, whose contribution was published posthumously in 1763. It is a trivial consequence of any plausible calculus or axiomatization of probability. It offers a valuable model for changing degrees of belief in the light of new evidence. That is why people who adopt the credibility stance find it natural to call their approach Bayesian.

Many have said that there are two concepts of probability. Early on, philosophically-minded authors said we should specify two names for the two concepts. None have stood the test of time. Everyone forgets which name means which concept. Carnap unintentionally parodied the situation by proposing probability$_1$ and probability$_2$. I cannot for the life of me ever remember which is number one and which is number two.

I began as a frequency man but I have become eclectic. I have learned from all schools of thought. In my elementary textbook (Hacking 2001). I found it best to speak of the frequency attitude (or stance) and the belief attitude, or stance. There have always been and always will be frequency-dogmatists who say that the only useful or scientific meaning of probability has to do with frequency. There have always been and always will be belief-dogmatists who say that the only useful or operational meaning of probability has to do with degree of belief. But even the dogmatists of each school have a good deal of difficulty pinning down exactly the shade of meaning that best suits their dogma.

In a great many situations probability is simply neutral, not equivocal but neutral, between the two attitudes. Case in point: there is not much probability of winning the lottery with only ten tickets in hand. Do I mean that the relative frequency with which people holding ten tickets win, is very low? Or do I mean one should not expect that ten tickets will solve your financial problems? I mean both. Or I mean neither. I mean probability.

It is symptomatic of a non-issue that it can be made a point of ideology. For 150 years Moscow was an epicentre of research in mathematical probability. A novice quickly learns about the Kolmogorov axioms, Markov chains, and the

Chebycheff inequality. In the Soviet Union, a subjective or belief attitude to probability was bourgeois idealism and hence unprintable. But the brilliant mathematics produced in Moscow was avidly acquired by Western scholars of all persuasions, regardless of ideology.

12.13 A Radial Concept

Probability can be thought as a radial concept, an idea that originates in cognitive psychology. It starts with Eleanor Rosch's prototype theory. She suggested that many concepts, such as *bird*, are best characterized by a few prototype exemplars of birds. In her part of the world, Northern California, students enrolled in first-year psychology are more likely to give the robin than any other bird as a best example of a bird. Obviously Greek students would give a different example, and even if British students gave robins, it would be an entirely different bird (Erithacus rubecula) from the robin of California (Turdus migratorius)—half the size, to begin with. Other types of birds radiate out from the local centre. Ducks and owls are not central for Rosch's students, but they are good enough examples of birds. Ostriches are far out. The examples radiate in different directions, ducks and gulls in one direction, eagles and falcons in another. You see the picture.

Probability fits the picture pretty well. Coins, lotteries, dice, drawing balls from an urn. These are the paradigmatic examples with which you learn the probability calculus. Each is an artificial randomizer, so made that there is a fundamental set of equally probable alternatives. That means that each alternative is, in the long run of repeated trials, obtained as often as any other. It also means that it is, on the stated information, no more or less credible that one alternative should occur than any other. Frequency dogmatists insist on the one way of talking, while belief-dogmatists insist on the other, but in real life what is important is that both apply equally well. That is precisely why we use them as prototypes for teaching probability ideas.

12.14 Away from the Centre

When we turn to applications one or the other attitude may strike us as most natural. Because of the different roles associated with Bayes' rule in belief and frequency thinking, many of the formal manipulations and applications look rather different when presented one way or the other. Occasionally there are substantive differences in results, but usually it makes no significant practical difference which attitude you deploy in the analysis of data or making decisions.

For a specific example, consider the taxonomic problem of choosing among phylogenetic trees consistent with available data. (Probable reasoning applied to a taxonomic problem, a reminder that styles of thinking and doing interweave.) I am studying, let us say, the bumble bee, a remarkable genus that has adapted to almost every environment on earth. Almost as well as humans, but by physiological rather than cultural adaptation. There are what seem to be innumerable species scattered around the earth, from Greenland to the Amazon. Most of the species of bumble bee are in the highlands of Yunnan and Tibet. They can fly higher than Everest, which seems to defy the laws of physics. They are vastly more interesting than the honey bee, of which there are only seven species.

Molecular biology enables us to delimit the number of species to about 250. There are many ways to draw up an evolutionary tree of the bumble bee. The most recent and perhaps definitive work does so by Bayesian analysis, which presents us with the most credible phylogenetic tree (Cameron et al. 2007).

Next consider the problem of genetic anthropology pioneered by Luca Cavalli-Sforza using blood groups, and enormously enhanced by molecular genetics (Cavalli-Sforza et al. 1994). The task is to produce a graph of population movements across the face of the earth, and once again the question is who is related to whom. Does an indigenous group in Yucatan, say, descend from the same people as a group in Florida? It happens that Cavalli-Sforza began the statistical analysis of his data in Cambridge, England, where the great frequency dogmatist R. A. Fisher held sway. One of Fisher's pupils (Anthony Edwards) became Cavalli-Sforza's colleague, and until at least very recently frequency analysis has prevailed in that field. In particular what are called maximum likelihood methods, first devised by Fisher, predominate.

For purely accidental reasons the analysis of the ancestral roots of native Yucatan people employs (frequency-style) maximum likelihood methods, and the phylogenetic analysis of bumble bees found in Greenland employs Bayesian methods. But there would have been no biologically or genetically significant difference in the results of analysis if the contingencies had been reversed, and Cavalli-Sforza had been supported by a student of his fellow countryman and great belief dogmatist, Bruno de Finetti. It just happened that Cavalli-Sforza went to Cambridge and not to Rome.

12.15 Criteria for Accepting and Rejecting Statements of Probability (Frequency Version)

Probable reasoning is not primarily in the business of making true statements. It aims at sorting what is probably true and what is not. We nevertheless make statements of probability. The chance of rain tomorrow is 0.3, the likelihood of a plane crash by a scheduled carrier is minute. What are the criteria for the acceptance or rejection of such statements?

All of a sudden it makes a difference whether we are in the frequency framework or the belief framework. Does that not show that my valiant eclecticism is for naught? Not at all. To ask for criteria of assertability is to ask a meta-question, it is to engage in semantic ascent. We have to stipulate precise meanings, even though in both ordinary discourse and much scientific practice we coast smoothly along without pedantry.

Karl Popper, with his enthusiasm for what he called objective knowledge, derided the “subjective” credibility approach. He postulated what he called the propensity account of probability, where the probability of an event is a theoretical property that manifests itself in the relative frequency with which the event would occur in repeated trials. Or as C. S. Peirce more succinctly put it, the “would-be” of a die. But Popper had a problem. Statements are scientific only if refutable. Dispositional statements are not obviously so. The propensity for two dice to fall ace simultaneously may be 1/36, but if we throw only once and get snake’s eyes, we have not refuted the propensity statement. Popper’s students said we should be satisfied if there are clear criteria for the acceptance or rejection of a statement of probability. So they invoked the theory of statistical testing developed by Jerzy Neyman and Egon Pearson in the 1930s. It harks back to the ideas of Jacques Bernoulli published in 1713. It is also at the heart of C. S. Peirce’s theory of probable inference (Hacking 1980).

Neyman and Pearson divided possible data resulting from an experiment into two classes, “reject the hypothesis” and “accept the hypothesis.” They designed the test using two probabilities, one of which maximized the efficacy of the test. The other was a number such as 5 % which stated the probability that you wrongly reject the hypothesis under test by using the method in question. In effect, a probability was assessed by the probability of error using this type of test procedure.

The bottom line is: *The criteria for the acceptance or rejection of such statements of probability are given by statements of probability.*

From the frequency point of view, statements of probability are self-judging in a rather trivial way. Or, to parody my earlier analyses, they are all too easily described as “self-authenticating” (Hacking 1992b).

12.16 Criteria of Internal Coherence for Statements of Probability (Belief Version)

The strong version of the credibility approach to probability treats statements of probability as assessments of my own personal degree of confidence in a given proposition. This was first clearly set forth in a short but classic talk given by F. P. Ramsey in 1926.

If probability statements are mere expression of personal opinion, how can there be any public criteria for their correctness? Ramsey showed that there are principles of internal consistency, now usually called coherence after a later and independent paper by Bruno de Finetti. Both men set out operational criteria for determining a person’s degree of confidence over a field of propositions. The shallowest but easiest to understand is in terms of the rates at which a person is willing to bet on a range of options. A set of betting rates would be foolish if an astute bookmaker could bet with you in such a way as to make a profit no matter what happens. The necessary and sufficient conditions for preventing that are that that betting rates satisfy the probability calculus, or standard axioms for probability.

Thus *the belief approach to probability invokes its own internal criteria of coherence*. It answers to no other standard than the probability calculus itself.

That is, there is a rather trivial way in which from the belief point of view, statements of probability are self-sealing, or, to parody my earlier analyses, "self-authenticating."

12.17 A Fearful Symmetry

There is a curious symmetry between the conclusions of the preceding two sections. This should not be surprising, since we are talking about basically the same concept dressed up in very different clothes.

In the concluding chapters of my elementary textbook I asked how the two main approaches to probability could be applied to the traditional philosophical problem of induction, first put in a clear modern form by David Hume. Both the Neyman approach and the Ramsey approach have been taken to vindicate induction. At the end of my textbook (Hacking 2001). I showed that both approaches have a gaping hole. In the case of the confidence approach, Peirce had already made plain that the vindication relied on a moral maxim that he called faith, hope and charity. I showed that from the belief point of view the vindication works only if backed by a parallel moral maxim, that one stay true to one's beliefs, lacking reason to change them. Both vindications of induction rely on a moral principle—very different in the two cases, but a question always of values, not facts.

12.18 A New Type of Object: And a New Ontological Disputation

Finally I turn very briefly to the novelties that are introduced by (or grow together with) the style of probable reasoning. I hope I have softened you up with the idea that the modern idea of a population is an instance of a new kind of object, characterized only by statistical techniques. People did not seriously start to acknowledge a new type of object until Quetelet gave us the "average man" in 1844. Is the average man a mere construct, or is it a property of human (or social) nature? This is the normal form of an ontological debate, and it is one generalization that works fairly well across all six of Crombie's styles. As new objects of study arise as a style evolves, some thinkers maintain that the objects are "real" while others maintain they are mere "instruments".

Galton gave us correlation coefficients in 1875. A disputation followed. Are these real properties of a population, or mere summaries?

These questions sound very technical, until we recall the modern rhetoric of politics with the unemployment rate, or the Gross Domestic Product. Indeed in my own lifetime an entirely new statistical object came into being without anyone noticing it: the Economy. More generally in the modern Risk Society, as Otto Beck calls it, we are surrounded by quantified risks, all taken as real characteristics of the world around us. There are and continue to be what I call ontological disputations as to whether quantities of this sort are real or merely summaries of data. These continue today.

12.19 New Classifications Generate New Kinds of People

One of the most remarkable and little-noticed phenomena connected with statistical practices, starting with the censuses, is the way in which classifying in order to count causes people to think in terms of those classifications. I argue that this happened with early efforts to control the abuses of the industrial revolution. Factories were inspected, and categories of workers were defined. Employers and workers alike began to think of themselves as being of such and such a type, a boiler-maker or a riveter. I sketched this idea in Hacking (1982b). This has been most extensively studied by Alain Desrosières (1998) in his book *The Politics of Large Numbers*.

12.20 A New Type of Law

The novelty due to the emergence of probable reasoning, and the taming of chance, that has had the greatest effect on modern thought, is the statistical law. It arose after people noticed remarkable stability in human behaviour, such as the suicide rate in a given region of Paris. My favourite aphorism, on this score, comes at the end of a long fascination with such social statistics. Emile Durkheim wrote that these statistical quantities and regularities are "as real as cosmic forces." The idea was transferred by quite specific lines of transmission—the persons who on the surface transmitted the ideas can be traced—to physics. First came statistical mechanics, with a new kind of population—a population of molecules in the first instance—subject to a new kind of law. The triumph was the indeterminism introduced by Planck and cemented in 1926–1927. We came to live in, as Peirce put it, a Universe of Chance.

12.21 New Kinds of Explanation

Hand in hand with the new laws came new ways to explain events. There are many questions about statistical explanation, but mostly they are for philosophers. We explain events all the time in terms of statistical regularities. For example, the claims about sexual abuse as a child having severe consequences on the adult are now reduced to statistics, and have changed criminal defences in trials in industrial countries. The person on trial says: I am a victim of my childhood, and do not deserve severe punishment for the crimes I have committed. That is the explanation of my errors, and since it is a causal explanation, it mitigates the crime.

12.22 Conclusion

Crombie's style (*e*), *the statistical analysis of regularities of populations and the calculus of probabilities* (for short, probable reasoning, or statistical inquiry), possesses, in its own way, many of the characteristics of the other five methods of investigation that he listed in his template. It is rather trivially self-authenticating. It brings with a whole host of novelties, including new objects such as the mean of a population. There continue to be ontological debates about these entities, starting with whether the mean is real or merely a summary of information. We also have a new type of law, which has changed how we interact with the world around us, and a new type of explanation, which changes our understanding of the natural, social, and personal world. Probable reasoning pursues its own trajectory, and continues to be a living, evolving, genre of inquiry.

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Chapter 13
Reductionism and the Relation Between Chemistry and Physics

Hasok Chang

Abstract The relationship between physics and chemistry is one of the perennial foundational issues in the philosophy of chemistry. It concerns the very existence and identity of chemistry as an independent scientific discipline. Chemistry is also the most immediate territory that physics must conquer if its "imperialistic" claim to be the foundation for all sciences is to have any promise. I wish to enhance the anti-reductionist position concerning the chemistry–physics relation with three arguments inspired by the works of some leading twentieth-century chemists. (1) The very foundation of quantum chemistry is classical, and its roots go back to the organic structural chemistry of the 1860s. (2) Chemists exploit for their own purposes the conceptual resources provided by physics; this may or many not involve deducing chemical theory from physical theory. (3) Even physics itself is much more disunified than it may seem, and therefore constitutes a dubious basis for reduction as it is normally envisaged. I will also suggest that a careful consideration of the physics–chemistry relation points to some productive ways in which we can move beyond the reductionism debate as it is traditionally construed in philosophy and science.

Keywords Reductionism • Reduction • Quantum chemistry • Structural chemistry • Linus Pauling • Schrödinger's equation • Integration

13.1 Introduction

In this paper I wish to re-visit the relation between chemistry and physics, and use this case to throw some new light on the general philosophical and scientific debates on reductionism. The nature of the chemistry–physics relation is not a new issue, and it has indeed been a central concern in the philosophy of chemistry. There is much existing work on the topic, and I do not pretend in this brief paper to give a comprehensive survey of it, nor even engage with all of what I consider the

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best work.[1] So I must begin by asking myself what I may hope to add to such a well-rehearsed debate.

My sense that there might be something useful to say originates from my study of Kostas Gavroglu's work on the history of quantum chemistry, much of it in collaboration with Ana Simões, which is conveniently collected and synthesized in their impressive recent volume *Neither Physics Nor Chemistry: A History of Quantum Chemistry* (Gavroglu and Simões 2012).[2] What Gavroglu and Simões's work has shown me is that the reductionism debate can still be significantly enriched and refreshed through a renewed attention to the history of science. So this article owes to their work not only much of its detailed content about quantum chemistry, but also the inspiration to take a more general integrated historical–philosophical look at the reductionism debate.

Before I enter into specific arguments, it makes sense to emphasize why the chemistry–physics relation is such an important issue. In the realm of scientific practice, the ambition of reductionism has most often been expressed in the form of "physics imperialism"—the attempt to take all other sciences as applications of physics. Ernest Rutherford has notoriously expressed his disdain for sciences that could not be ultimately reduced to physics: "All science is either physics or stamp collecting" (Birks 1962, 108). It may be considered fitting punishment that he was given the Nobel Prize in *chemistry* in 1908. "Biology is nothing but applied chemistry, which is nothing but applied physics" and such thoughts constitute a familiar refrain of reductionists at the lab bench and in the schoolroom.

Serious philosophers, too, have entertained exactly such thoughts. For example, in their classic reductionist manifesto, "Unity of Science as a Working Hypothesis", Paul Oppenheim and Hilary Putnam (1958, 9) express their expectation that the science of social groups will be replaced by the science of the individual multicellular organisms that constitute such groups, that the science of individual multicellular organisms will be replaced by the science of the cells that constitute such organisms, and so on, eventually down to the science of the elementary particles that ultimately make up everything. In this "imperialist" project, physics will not get anywhere if it cannot conquer the territory traditionally covered by chemistry, which may be seen as the next-door neighbor of physics in the geography of scientific disciplines.

Gavroglu and Simões remind us that some of the most important architects of quantum physics and chemistry did concern themselves with this task of reduction. Already in 1923, *before* the establishment of what we now know as the standard forms of quantum mechanics, Max Born thought he could see the way forward: "we have not penetrated far into the vast territory of chemistry; yet we have travelled far enough to see before us in the distance the passes which must be traversed before

[1] For a recent survey of the literature, see Hendry (2012); an earlier and more concise view is given in Hendry (2008). Most notable individual views include Scerri (2008), sec. A; van Brakel (2000), Chap. 5; and Hettema (2012).

[2] For my own and other reviews of this book, see Chang et al. (2013).

physics can impose her laws upon her sister science" (quoted in Nye 1993, 229). Some even considered it already achieved in principle almost as soon as Schrödinger's quantum mechanics was formulated. Walter Heitler, just after starting his pioneering work in early quantum chemistry published in 1927, declared in a letter to Fritz London, his collaborator in that work: "We can, then, eat Chemistry with a spoon" (quoted in Gavroglu and Simões 2012, 22). I take it that the sense of this image was that chemistry would offer so little resistance that not even a knife and a fork were required. In Paul Dirac's memorable conceit expressed in 1929: "The underlying physical laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known, and the difficulty is only that the exact application of these laws leads to equations much too complicated to be soluble" (quoted in Gavroglu and Simões 2012, 9).

From the viewpoint of chemistry, its alleged reduction to physics is even more serious business. For the physicist, any actual failure of reduction is not so troubling: it does not threaten physics itself, and can easily be blamed on the imperfection of our mathematical techniques or the particular complexity of given systems of interest. For the chemist, however, a successful reduction of chemistry to physics would threaten to remove the very *raison d'être* of chemistry as an independent scientific discipline. But since even those quantum chemists who seem to base their work entirely on physics can happily work in chemistry departments that operate very independently from physics departments, the alleged reduction may not be such a dire concern in practice. For the philosopher of chemistry, however, there is not even this practical comfort. If chemistry really is reducible to physics and there is no independent chemistry in the intellectual sense, then there is no real need for a philosophy of chemistry. Moreover, there would not even be practical inertia pushing for its perpetuation, since philosophy of chemistry lacks the kind of institutional and industrial backing that keeps chemistry itself going. All this may explain why there *is* so little philosophy of chemistry (compared to philosophy of physics or philosophy of biology), and why most of what there is leans towards a sort of defensive anti-reductionism.

13.2 Benefits of a Historical Perspective

As announced above, one key inspiration that I have received from the work of Gavroglu and Simões is to take history more seriously in thinking about the chemistry–physics relation. Similar inspiration also comes from the work of Bernadette Bensaude-Vincent and Jonathan Simon (2012, esp. Chap. 8). The immediate insight that a look at the longer-term history gives us is that the relationship between chemistry and physics has not been a fixed thing. Knowing something about the longer-term history of this changing relationship will help us contextualize our current situation and put it into better perspective. It is a familiar point among historians of science that disciplinary labels are fluid. "Chemistry" has been a changing and evolving category, and "physics" even more so. Even when we consider periods during which each of these two disciplines was well enough defined, it is important to recognize that the boundary between them has been shifting. Some topics that are very important for the identity of each discipline have shifted between the two. Two hundred years ago heat and electricity were clearly chemical subjects (and even chemical substances), even though they were also treated by physics to the extent that there was such a thing as "physics". Atoms had to be made respectable in chemistry first through a long struggle in the nineteenth century, before physicists could begin to find useful ways of engaging with them.[3] Such boundary-shifts make it harder to say that chemistry as such can be reduced to physics as such; at best we may be able to argue that some particular phases of chemistry have been reduced to particular phases of physics.

When we take such a particularist look, at least as many non-reductive moments as reductive ones are seen in the chemistry–physics relation. The early triumphs of physics, such as those achieved by Newton, had little relevance to chemistry. Successful notions of attraction in chemistry, usually under the rubric of "affinity", were conceived without regard to any "underlying" physics; the best reductive attempt, by Claude Berthollet around 1800, was respected but abandoned by most chemists, despite Berthollet's high prestige. John Dalton's thinking was firmly grounded in physics when he proposed his atomic theory, but most chemists who took up atomism made a "chemical" version of it, dropping Dalton's notions about the sizes and shapes of atoms and the physical forces that affect their combinations. Gilbert Newton Lewis's use of electrons and electron-pairs in the explanation of chemical bonds might seem like an exemplary instance of the application of physics to chemistry, but Lewis made almost no use of the physical properties of electrons, and had no physics with which to explain why electrons would form pairs. Addressing an even more important instance of alleged reductive success, Eric Scerri (2007, 248) declares: "It is indeed something of a miracle that quantum mechanics explains the periodic table to the extent that it does at present. But we should not let this fact seduce us into believing that it is a deductive explanation." And orbitals, deemed to be unreal by any rigorous quantum mechanical reckoning, continue to have enormous importance in theoretical and experimental reasoning in chemistry. These are just a few of the most important non-reductive moments in the relatively recent history of the chemistry–physics relation.

A closer look at the history also makes it plain that the standard framework of the reductionism debate in the philosophy of science is not very helpful in thinking about the chemistry–physics relationship (or the relationship between *any* empirical sciences, for that matter). I will discuss this matter in more detail in Sect. 12.4 below, but for now consider the canonical "correspondence" view of reduction by Ernest Nagel (1961, 1974) and others. Here is one of Nagel's early formulations (1953, 541): "The objective of the reduction [of one science to another] is to show that the laws or general principles of the secondary science are simply logical

[3] On the futility of early atomic theories, see Chalmers (2009).

consequences of the assumptions of the primary science." As will become clearer in the discussion to follow, the full academic relationship between scientific disciplines is one between sets of practices with different objectives and values, not only (or even primarily) between sets of propositions as standard accounts of reduction would have it. Consider, for example, organic synthetic chemistry: what are its "laws or general principles"? Even if we could articulate such a set of propositions, merely stating those propositions would simply miss the point of the endeavor. Even if we look at much more theoretical and physics-friendly areas of chemistry, it is not clear that the main use of physics for chemistry has been in the deduction of chemical propositions from physical propositions.

13.3 Three Points on the Modern Chemistry–Physics Relation

The preliminaries stated above will now help me focus, reinforce and reframe three anti-reductionist points that can already be found in extant literature.

13.3.1 Quantum Chemistry Is Founded on Classical Chemistry

Anyone who wants to challenge the reduction of chemistry to physics must tackle the case of quantum chemistry head-on. Whatever the historical situation has been, it may seem that we now have a clear verdict that chemistry has been reduced to quantum physics. Modern physics tells us what atoms are, and how they combine with each other—isn't that all we need to know in chemistry? More specifically, the Schrödinger equation specifies how electrons distribute themselves around atomic nuclei, and that is the basis of all the information that chemistry requires. So the remaining challenge of chemistry is the solving of the very complicated Schrödinger equations of multi-electron systems. While chemists' actual research practices involve much else and much more than the Schrödinger equation, many chemists still pay lip-service to the idea that all of their knowledge is ultimately founded in the Schrödinger equation.

Against that naïve and dogmatic reductionism, the first and most powerful anti-reductionist point concerning quantum chemistry is that it cannot be practiced without making use of the knowledge gained in pre-quantum chemistry. Gavroglu and Simões's discussion of Linus Pauling in this context is particularly instructive. Pauling, whose pioneering and significant role in the development of quantum chemistry nobody would deny, thought of this field as a direct continuation of nineteenth-century organic structural chemistry, dubbing it "modern structural chemistry".[4] He declared in 1970: "The theory as developed between 1852 and 1916 retains its validity . . . . It has been developed almost entirely by induction (with, in recent years, some help from the ideas of quantum mechanics developed by the physicists). It is not going to be overthrown" (Gavroglu and Simões 2012, 251).

What Pauling points out in such statements is that the quantum-chemical calculations are built on the presupposition of molecular structures, established for chemical reasons long before there was quantum mechanics, rather than vice versa. Two steps need to be distinguished in order for us to appreciate this point fully. First, as R. Guy Woolley among others have observed for many years (Sutcliffe and Woolley 2012, and references therein), the typical method of quantum-mechanical treatment of molecules begins with the Born–Oppenheimer approximation, which separates out the nuclear wavefunction from the electronic wavefunction ($\Psi_{\text{total}} = \Psi_{\text{nuclear}} \times \Psi_{\text{electronic}}$). Additionally, it is assumed that the nuclei have fixed positions in space. In this "clamping-down" approximation, the atomic nuclei are treated essentially as classical particles; as Olimpia Lombardi (2013) points out, this picture is non-quantum in a very fundamental way as the simultaneous assignment of fixed positions and fixed momenta (namely, zero) to them violates the Heisenberg uncertainty principle. But without such classical scene-setting, the quantum calculations are quite impossible.

The difficulty here is not only about the practicalities of calculation, and the clamping-down of nuclei is not merely an approximation. Aside from assuming that the nuclei are fixed, it is necessary to know *where* exactly the nuclei in question should be placed. Otherwise it is not possible to specify the potential function, which needs to be inserted into the Schrödinger equation, whose solution determines the wavefunction of the electrons in the molecule. In other words, without knowing the locations of the nuclei in the molecule, it is impossible even to *set up* (not to mention *solve*) the Schrödinger equation. So we must ask: on what basis do quantum chemists make their initial assumptions about the positions of the nuclei? The answer is: the molecular structures determined, from purely chemical reasoning, in classical structural chemistry! Initially assuming that the nuclei are fixed in their "classical" places, chemists are then able to use quantum mechanics to calculate further aspects of the molecules such as precise bond lengths and energies, and also reason about the cases when nuclei are not exactly fixed. This is the kind of progressive development that I have called "epistemic iteration", which I think is quite common in science (Chang 2004, Chap. 5). To my knowledge, there are no cases in which an ab initio solution of a multi-electron and multi-nuclei molecular Schrödinger equation has yielded the structure of any molecule with

[4] This is what he calls the subject in the subtitle of Pauling (1960). See also Gavroglu and Simões (2012, 77).

any complexity to speak of. And we should not expect such an achievement, because protons and neutrons, which make up nuclei, do not even obey the Schrödinger equation.

The use of the "nucleus-clamping" approximation is a well-established point, among those who have considered the reductionism question in the context of quantum chemistry. I would like to stress an additional and slightly different fact: the very structure of Schrödinger's quantum mechanics is classical in a similar kind of way, and this is *not* something that arises because of the need for approximations in a complex multi-particle system. Look at the basic time-independent Schrödinger equation for one particle, which is the starting point of all further work in wave mechanics:

$$-\frac{\hbar^2}{2m}\frac{d^2\psi(x)}{dx^2} + V(x)\psi(x) = E\psi(x)$$

Already striking is the fact that this is an equation for just one particle, not for the interaction between two particles. This is in contrast to the basic equations of Newtonian gravitation, Coulombic electrostatics, etc., whose fundamental laws tell us *explicitly* about interactions between two bodies. In the Schrödinger equation, the interaction that the particle in question has with any other particle is only expressed indirectly through the potential function V (which forms part of the Hamiltonian H).

In classical mechanics or electrostatics, it is clear what the potential function signifies, and where its values come from: first we calculate the force on a test body due to the interactions according to the relevant force law, and integrate the force function over distance in order to obtain the potential energy. But if we ask where the potential function V inserted into the Schrödinger equation comes from, the answer is curious. Those who have the experience of suffering through basic quantum mechanics will remember things like the square potential well (infinite or finite), which are pedagogical devices for showing how the equation might be solved, not anything intended to describe the properties of real systems. And then we get the one real system whose Schrödinger equation is exactly solvable, namely the hydrogen atom. In this case V is taken to be simply the Coulomb potential that we know from classical electrostatics!

$$\mathrm{V(r)} = -\mathrm{e}^2/4\pi\varepsilon_0\mathrm{r}$$

The fundamental assumption giving rise to that potential function is not only that the force involved is the good old Coulomb electrostatic attraction, but also that the nucleus is a point-particle fixed in one position. So one might say that Schrödinger's quantum mechanics, right from its very first use for a real-life system, was *born* with the nucleus-clamping assumption. It should be stressed again that this is not something that arises from the need for approximation, but something woven into the very fabric of elementary quantum theory. The theoretical framework of Schrödinger's wave mechanics does not allow any scope for theorizing about the state of the nucleus at all.[5]

13.3.2 Physics Itself Is Not So Reductionist

My second main point about the modern chemistry–physics relation concerns the nature of modern physics itself. I will argue that physics itself is not so reductionist, and therefore it is not a suitable basis for the reduction of chemistry (or anything else). "Physics imperialists" should put their own house in order first, before trying to take over any other sciences. The first pertinent observation here is that there are many branches of physics itself that are not reduced to the most fundamental theories of physics.[6] In most areas of condensed matter physics, mid-level concepts such as Cooper pairs or phonons have been necessary in order to explain important novel phenomena such as superconductivity and semiconductivity. It has also not been trivial to apply the advanced theories of microphysics to mundane macroscopic phenomena; consider, for example, the task of giving a quantum-mechanical account of the absorption of an electron by a metallic surface.

We should also not forget that the most fundamental theories of physics are also not unified with each other. Quantum gravity is still a project very much in progress, with no clear promise that it will be concluded successfully. I am not claiming that the ultimate unification of physics will never be successful. No one can be confident in such predictions about the long future of science, and concerning the plausibility of an ambitious goal (such as the grand unification of all fundamental physics), the burden of argument is on those who are actually pursuing that goal. The point is not to *presume* that unification will be successful, and not to reason entirely on the basis of such a presumption. In the here-and-now of physics, it is important to note that many important and successful uses of physics draw from various parts and levels of physics, which have not been unified with each other in general. A good example illustrating that point is the global positioning system (GPS), which draws from Newtonian mechanics (for satellites), quantum mechanics (for atomic clocks), and special and general relativity (for the correction of atomic clocks), without any attempt to unify those theories on the whole.

Returning to the chemistry–physics relation more specifically, a key point to note is that chemistry does *not* use the most fundamental and up-to-date theories of physics. Trying to use quantum chromodynamics or superstring theory to do

[5] Incidentally, it is also important to remember how *parochial* the original remit of the Schrödinger equation was. It is a grave mistake, committed by Schrödinger himself among others, to imagine that it is some sort of a fundamental law that can tell us about all of the universe. That is where the philosophy of physics in the twentieth century went seriously wrong.

[6] A classic statement of anti-reductionism by a renowned working physicist is Anderson (1972).

chemistry would be absurd, and even trying to use quantum field theory would be quite futile. The greatest benefit for chemistry comes from using the physics of a century ago. Somehow, Bohr in 1913 and Schrödinger in 1926 (and not Heisenberg in 1925) hit the right notes for chemistry. In this context, Schrödinger's theory is not simply an approximation to Dirac's, Feynman's, or Yang and Mills's, or any other more advanced theory. In fact we do not have sufficient evidence that using more advanced physics for chemistry would give us better results, even if we could handle the mathematics. And it is also not the case that Schrödinger's theory is at an emergent level of ontology in relation to more fundamental theories of physics. It deals with the same level as the more advanced quantum theories, at least when it comes to electrons.

13.3.3 Chemists Exploit Physics

My third main point concerning the modern chemistry–physics relation, already hinted above, is that chemists *use* physics in their practice but they do not surrender or submit to it (contrary to what some physicists may imagine). This is the case even for many of the chemists who pay lip-service to the fundamentality of quantum mechanics for their enterprise, and who may subscribe to reductionism in their more philosophical moments. I would even say that chemists *exploit* physics (by which I do not mean that they necessarily exploit *physicists*.)

A longer-term historical perspective is helpful here. For at least two centuries chemists have made good use of physics, without thereby turning into physicists or turning their subject into physics: we can think about Lavoisier's use of weights, Davy and Berzelius's use of electrostatic theory to explain chemical combinations, Dulong and Petit's use of specific heat measurements to help determine molecular formulas, organic chemists' use of melting and boiling points to help them distinguish similar substances, and so on.

When we consider twentieth-century chemistry, Linus Pauling again gives us a very useful clue.[7] Pauling's great success in shaping the early directions of work in quantum chemistry was due to his ability to *use* quantum-mechanical ideas to help him do better *chemistry*, rather than turning chemistry into physics in any real sense. It may have been the focus of some early pioneers of quantum chemistry, such as Heitler and London, to treat chemical systems as exercises in quantum physics. But it is important to note that this was typically not the viewpoint of the later and more successful quantum chemists. Gavroglu and Simões tell us that Pauling's emphasis on the tradition of structural theory was due to its continuing

[7] In addition to the work of Gavroglu and Simões, I have benefited greatly from the informative article by Martha Harris (2008) on Pauling, Lewis and the chemical bond.

importance in organic chemistry, and its newfound usefulness in the applications of chemistry in biology and medicine—one is reminded of his great success in elucidating the alpha-helix structure of proteins and his role in the race to solve the problem of DNA structure. In the "integration" of the sciences that he advocated, to be achieved through the sharing of tools and methods, Pauling saw chemistry, not physics, as occupying the central place (Gavroglu and Simões 2012, 119). It is significant that Pauling dedicated his masterpiece *The Nature of the Chemical Bond* (Pauling 1960) to Lewis, whose pioneering work on electron-based conceptions of chemical bonding (the cube/octet and the pairing of electrons) were based on no detailed theories of physics and created before the arrival of full-fledged quantum mechanics (see Arabatzis 2006, Chap. 7).

Pauling's anti-reductionism concerning the chemistry–physics relation was shared by Charles Alfred Coulson, the leading pioneer of quantum chemistry in Britain, who said in 1970 that "one of the primary tasks of the chemists during the initial stages in the development of quantum chemistry was to escape from the thought forms of the physicists" (Gavroglu and Simões 2012, 1). While clearly deploying theoretical resources borrowed from quantum mechanics in his chemical work, Coulson thought it was important not to think entirely like the physicists who developed those resources. Generally speaking, while most twentieth-century chemists accepted that the momentous developments in physics had some serious implications for the practice of chemistry, there were a variety of ways in which that relevance of physics to chemistry was understood and developed.

A general note might be useful here, about the notion of "fundamentality". It is a mistake to think that being "fundamental" implies having higher status or importance—literally, it should mean being lower! Seriously, and less metaphorically, what does it mean to be "fundamental"? That is not easy to state unequivocally, but one main sense is that if A is fundamental to B, then A is necessary for B. There are many forms of "necessity": A may be a material requirement for B to be possible (say, the foundation of a building in relation to the rest of it); A may be a necessary tool to enable B (say, the calculus for Newtonian celestial mechanics, or the hammer for hanging a picture on the wall); or A may be something one must know before one can learn B (say, arithmetic for algebra). In all these cases, B may be the really important thing or activity, supported by A. That was probably the notion that Pauling and others had about chemistry (B) in relation to physics (A). In Korea, where I grew up, there is a traditional saying that the farmer is the foundation of heaven and earth (of society, less grandiosely). That does not at all mean that traditional Korean society actually gave high status or paid much respect to farmers; it only meant that the rulers recognized clearly that farming was fundamental and without it the whole society would collapse. That is not quite the image that physicists have when they boast that physics is a more fundamental science than chemistry. When reductionists say that physics is fundamental to chemistry, they tend to make an unspoken assumption that physical theory is sufficient to tell us everything that chemical theory says. If we look at the practice of chemical theory and experiment, it becomes obvious that physical theory is necessary but not sufficient for performing what chemists want to achieve.

13.4 Beyond Reduction: Philosophical Viewpoints

I would now like to take a slight step back from the specific points that I have made about the modern chemistry–physics relation, and articulate some general philosophical viewpoints that will render the points made so far both more convincing and more deeply meaningful.

13.4.1 Aims of Chemical and Physical Activities

Implicit in my discussion so far has been the inclination to look at what chemists *do* with physics, rather than focusing on the logical relationship between the propositions contained in physics and the propositions contained in chemistry. It is my general philosophical ambition to understand scientific practice in a fuller sense, rather than just focusing on the propositions involved in it and scientists' belief in them. (This is one of the general ways in which I try to improve on standard Anglophone philosophy of science of the last several decades.) Practice consists of various epistemic activities, and these include the activities that constitute theorizing. But *theorizing* (as opposed to *theory*) is a very different thing from simply believing or not believing certain propositions, and involves a lot more than the postulation of propositions and the deduction of propositions from each other.

One of the most important things to examine when one looks at scientific practices, and what one misses by only paying attention to propositions and beliefs, is the aims that scientists are trying to achieve in their epistemic activities. Looking at the relation between chemistry and physics, we must ask whether chemists and physicists have significantly different aims in their practices. If so, chemistry and physics do not at all become the same thing even if their practices should involve exactly the same propositions. My view is that the aims of physics and chemistry are diverse and overlapping, but not identical enough to warrant a true merger of the two fields.

Chemistry has a broad range of interrelated aims, which do not seem to map neatly on to the aims of physics. One point that is very often noted, at least ever since Marcellin Berthelot's famous statement that chemistry "creates its objects", is that *making* has been a preoccupation of chemistry (Bensaude-Vincent and Simon 2012, Chap. 6). This is not only about the synthesis of new substances. On the one hand it also involves making substances that are already well known, and on the other hand there is also a focus on the creation of new processes and reactions. As well as synthesis, let's not forget about the venerable old aim of *analysis*, or more broadly the *identification* of substances, whether it is done by means of chemical reagents, nuclear magnetic resonance, or anything else; the other side of the analytical coin is the study of the properties of the identified substances, which remains a bedrock activity of chemistry. And the *classification* of the substances so characterized has been an important aim of chemistry over the centuries, even though it is now widely considered a straightforward and pedestrian business.

Do the aims of physics differ radically from these aims of chemistry? This is a complicated question. The usual perceptions of the difference here are based on an extreme and impoverished view of what physics practice is really all about, such as the following by Steven Weinberg (1994, 6): "Our present theories are of only limited validity, still tentative and incomplete. But behind them now and then we catch glimpses of a final theory, one that would be of unlimited validity and entirely satisfying in its completeness and consistency." Even though there is certainly that desire for grand unified theories among at least some physicists, physics also very much shares the aims of chemistry that I have discussed above. However, we should also note that there are some important differences in how the aims of physics and chemistry are concretely manifested, because of the differences in subject matter, methodology, and emphasis in the two fields.

13.4.2 Reduction as an Aspect of Inter-System Relation

The question of reduction should be understood as one aspect of a broader and more complex question concerning the relationship holding between two systems of practice.[8] The starting point of my discussion here will be a recognition that there are different ways in which two systems can relate to each other. Looking at the various types of inter-system relations, we can ask which of those relations may incorporate something like reduction.

Elsewhere, in my discussion of pluralism in science (Chang 2012, Chap. 5), I distinguished three modes of interaction between co-existing systems of practice: integration, co-optation, and competition. These are real and distinct and beneficial modes of interaction, which should be considered as seriously as the reductionist holy grail of unification. And within the category of what is colloquially called "unification", there are different types of events, which I might call "merger" and "acquisition". In a merger, two systems come together on an equal footing and make a new system that is more general. A good example would be how the work of Michael Faraday and then James Clerk Maxwell brought together the previously separate sciences of electricity and magnetism. In what I call acquisition there is an unequal relationship that takes place between two systems; one is dissolved, and absorbed into the other. This is what happened, for example, when Kepler's astronomy of planetary orbits got absorbed into Newton's gravitational physics. Unification, either by merger or acquisition, is different from what I mean by "integration" (akin to what Otto Neurath called the "orchestration of the sciences"), in which the interacting systems remain intact in themselves but they are brought

[8] What I mean by a "system of practice" is defined in Chang (2012, Chap. 1), and the notion provides an analytic framework for the entire work.

together in joint application to address a specific problem. The construction of the global positioning system, mentioned above, is a good example of integration. In co-optation, isolated elements from one system are adopted by another for its own purposes. For example, Lavoisier adopted Priestley's production of de-phlogisticated air and Cavendish's production of water from the combustion of inflammable air, and turned those results from phlogiston chemistry into key components of his own system. In the relation of competition, the competing systems affect each other's behavior without adopting specific content from each other. We might say that the competition between catastrophism and uniformitarianism in geology exhibited this pattern.

Now, having distinguished the five modes of inter-system relation, we can ask which modes describe best the relation between chemistry and physics. The traditional view might be that physics has now acquired chemistry. I think the various points I have made and referred to so far are sufficient to discredit this view; all in all, the epistemic activities of chemists remain very distinct from those of physicists, and it is not at all the case that chemistry has become part of physics as a practice. I think Martha Harris (2008) is correct in saying that in the early to mid-20th century quantum chemistry has created a synthesis (or, hybrid) of chemistry and physics (rather than reduction). But in what exactly did this synthesis consist?

Pauling's remarks quoted above suggest that what happened in the development of quantum chemistry was a case of chemistry co-opting elements from physics. There is also a serious question to be raised, regarding whether there was a co-optation of results from chemistry by physics; it could be argued, for example, that the development of the concept of spin in physics owed a good deal to the co-optation of Lewis's electron-pair idea. But the other direction of co-optation is more obvious. If we look back at Lewis's practice it is quite obvious that he takes the discovery of the electron from experimental physics, and makes use of it in his chemistry in ways that did not relate at all to any credible physics of the electron.

Concerning the more mature phases of quantum chemistry, however, my view is that what we have is a case of integration, between classical structural chemistry and the Schrödinger formulation of quantum mechanics. It is more than co-optation (of physics by chemistry), as the kinds of questions addressed in quantum chemistry go well beyond the kinds of questions raised in classical chemistry. It is not acquisition (of chemistry by physics), because of the fact that classical structural chemistry remains a separate and independent system of practice (though no longer an active research field), and provides those essential nuclear positions in the molecules that we wish to study using quantum mechanics. Rather, what we have is the bringing together of the two systems in order to handle a particular class of problems. And "quantum chemistry" is a bit of a misnomer, because it suggests that this field of study treats all of the chemical questions conceived in the quantum mode. But if we consider other areas of chemistry to which quantum considerations might be relevant, for example electrochemistry and solution chemistry, it is evident that we require a different synthesis, at least because thermodynamics has to enter the picture in an essential way. This is, of course, only a reminder that I should never have conceived my topic so broadly as to claim to comment on "the relation between chemistry and physics"!

Finally we can come back to the question of reduction: does the relation of reduction hold within these modes of inter-system relation? Here I am staying with the classic correspondence view of reduction, and asking if the key theoretical propositions of one system can be deduced from the key theoretical propositions of the other system. I will accept, without pedantic questioning for the moment, that when we are tempted to call a development "unification" (merger or acquisition) there is reduction being achieved. If what we have is acquisition, then the propositions of the acquired system would be deducible from the propositions of the acquiring system. In merger, the propositions of either system would be deducible from the propositions of the new system formed by merger. At the other end of the spectrum, there would be no reduction expected within competition. There would also not be wholesale reduction in integration or co-optation, because the two systems remain separate and independent. However, interesting questions can be raised regarding the possibility of partial reduction. In the case of integration, can a significant portion of the propositions (at least in some specific version) in either of the two systems be deduced from propositions in the hybrid system? In that case it could be considered that a certain truncated version of the initial system has been reduced to the hybrid system. A similar question can be raised for co-optation, too: can the co-opted elements (if they are propositional) be deduced back from the propositions of the co-opting system? These questions provide subjects for future research.

13.4.3 Reduction Versus Replacement

As the discussion above should make clear, I am not interested in making a dogmatic claim against the possibility or actuality of reduction in the relation between various scientific systems of practice. In fact I do think that reduction, when it happens, should be considered an achievement. This is because the various inter-system interactions that may incorporate reduction have their benefits. These benefits are conceptualized in the context of my arguments for pluralism in science, which have been elaborated elsewhere (Chang 2012, Chap. 5, esp. 279–284). However, we can only make a proper appreciation of reduction if we get rid of one common major misconception. This is the assumption that when one theory has been reduced by another, the reduced theory should now be discarded. There is also a related assumption that when two systems of practice have been unified, there should be only one system remaining as a result.

One overall point that my perspective on the issue should make clear is that the logical relation between two theories (or sets of propositions) is not at all the same as the broader relation between the systems of practice in which the theories are respectively embedded. So, even if a reduction relation holds and the reduced theory may be deemed redundant in its content, there may well be good reasons not to abandon the system of practice that incorporates the reduced theory in question. One clear reason would be that the different systems of practice involved fulfill different kinds of aims.

But there are also reasons to retain a system of practice associated with a reduced theory even if the two systems pursue the same kind of aims. It can easily happen that different systems of practice can satisfy the same kind of aim, but in different ways. For example, there are different formulations of classical mechanics (Newtonian, Lagrangian, Hamiltonian, Hertzian) that express the same basic content and address the same kind of physical situations, but that are suited for different types of problems and that give us different kinds of intuitive understanding. This is also the case for different formulations of quantum mechanics (Heisenberg, Schrödinger, Dirac, Feynman). And such alternatives can develop in very different and differently fruitful ways. For example, the Newtonian formulation of classical mechanics could not have provided a formal framework suited for quantum mechanics the way the Hamiltonian, and then the Lagrangian formulation did. So it would have been a very foolish thing to discard Hamiltonian mechanics just because it can be reduced to Newtonian mechanics (or vice versa). When we do have really quite different systems, it may still happen that they satisfy the same aim to similar degrees. They can also set up a competition that is beneficial, keeping up a critical awareness of alternatives and preventing a sense of complacency.

These considerations lead to a more fail-safe recommendation: when reduction happens, there should be no presumption that the reduced theory ought to be discarded. Instead, we should ask what functions the reduced theory has served, and if those functions can all be served by the reducing theory. Beyond that, we should ask if it is clear that the reduced theory has no hope of being used in hitherto unknown systems of practice that will deliver new benefits. And we should also ask if we can be reasonably certain that all the actual and potential systems associated with the reduced theory will not stimulate other systems positively by competition. If the answer is “yes” to all these questions, then we may consider discarding the reduced theory *if* it is too costly to maintain it.

This stance is consonant with a position that I have called “conservationist pluralism”: retain previously successful theories and paradigms (systems of practice) for what they were (and are still) good at, and *add* new ones that will help us make new and fresh contacts with reality (Chang 2012, 218). The main idea here is that if a system of practice was once adopted as a good one by a group of serious, honest and self-critical scientists, then it is likely to continue to serve the useful functions that it once served, unless nature actually changes in a radical way. (This is the same practical assumption we make in everyday life, even though we know about the problem of induction.) Recall Pauling’s confidence: “The [structural] theory as developed between 1852 and 1916…is not going to be overthrown” (quoted in Gavroglu and Simões 2012, 251). The point is that we should not discard something as good as nineteenth-century structural theory, even though we have the option to do so. A good system of practice is difficult to create; we should not hastily throw it away when another one comes along, because the old and the new are not likely to do exactly the same work, so keeping the old is not a waste of effort and resources. This is why scientists have retained Newtonian mechanics in physics and orbitals in chemistry, etc., despite paying ideological lip service to reductionism and saying that Newtonian mechanics is really incorrect and orbitals do not really exist.

13.5 Concluding Thoughts

Briefly in conclusion: I think it is time to get beyond the reductionism debate, without dismissing it as unimportant or irrelevant. If we recognize and cultivate reduction as merely one of the possible productive modes of interaction between different fields of study and between different systems of practice within a given field, we will see that there is a great deal of philosophical, historical and scientific work to do—in recognizing different varieties of those interactions, studying how specific cases of interaction worked out, and how they might be improved.

I also hope that this paper can serve as an instance of the idea that paying proper attention to chemistry may help us gain a new philosophy of science. I think it is especially useful to look back at physics from the new perspective gained from our consideration of chemistry, as it will help us go beyond some distortions that were introduced to philosophy of science when a particular perspective on physics became dominant within it. This is in accord with Gaston Bachelard's proposal for a re-orientation of philosophy of science as "metachemistry", remedying the shortcomings of "metaphysics".[9]

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[9] See the exposition and development of this idea in Nordmann (2006); see also Chang et al. (2010).

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Chapter 14
The Internal-External Distinction Sheds Light on the History of the Twentieth-Century Philosophy of Science

Gürol Irzik

Abstract Drawing on the recent revisionary scholarship regarding logical positivism and its relation to the early post-positivism, I display and question the standard historical understanding of the analytical philosophy of science from the late 1920s to the mid-1970s. I then propose an alternative account based on the internal-external distinction. I conclude by showing some advantages of my alternative narrative that does more justice to the logical positivism than the standard understanding and suggest some further lines of research that it opens up.

Keywords Logical positivism • Postpositivism • Internal-external distinction • Constitutive a priori

14.1 Introduction

The history of the analytical philosophy of science from the late 1920s to the mid-1970s is often told in the following way: Logical positivism (or logical empiricism) was the dominant philosophy of science until the 1960s. Both the image of science and the way philosophy of science was practiced, which we owed to logical positivists, went through a revolution in the hands of such early postpositivist philosophers as Thomas Kuhn, Imre Lakatos, Paul Feyerabend, Norwood Hanson and Stephen Toulmin. As a result, a completely new image of science and a new (historical) way of doing philosophy of science emerged. In this account Karl Popper's falsificationism is seen as occupying a middle position in between, perhaps closer to logical positivism than to early postpositivism. Let me call this the standard account.

However, recent revisionist work on the history of logical positivism by a number of historically oriented philosophers of science such as Alberto Coffa, Richard Creath, Michael Friedman, Alan Richardson, George Reisch, Thomas Uebel and myself has shown that the standard account above will not do for several

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reasons: First, it hides some important similarities and continuities between the logical positivist and the early postpositivist pictures of science. Second, it treats logical positivist/empiricist philosophy as too monolithic, simplistically and, ironically, ahistorically. Finally, it completely misses the neo-Kantian roots of the logical positivist movement—a crucial point whose omission results in a distorted history. We now know that the logical positivism is a much more sophisticated movement incorporating different strands within it, that its most original contribution is to develop an alternative to the Kantian philosophy by taking into account the revolutionary scientific discoveries of the period, and that many theses attributed to the early postpositivist philosophers of science were also held by some of the leading logical positivists.

These considerations suggest that we need a better historical account than the standard one—an account that would incorporate the findings of the recent revisionary scholarship in a coherent whole. How should we then tell the story of the development of the analytical philosophy of science in the past century, roughly from the late 1920s to the mid-1970s? What should be the guiding questions, themes and issues that would enable us to see both the continuities and the ruptures and to discover new relationships among different actors of the logical positivism and of early postpositivism? The aim of this chapter is to propose an alternative narrative, or more correctly delineate *a thread* that would help us answer these questions. In Sect. 13.2, I describe the standard account, criticize and reveal its limitations. In Sect. 13.3, I explain what I mean by the internal-external distinction. In Sect. 13.4, I discuss the views of major actors of logical positivism and early postpositivism and reclassify them in the light of this distinction. In the final section (Sect. 13.5) I display some advantages of my alternative narrative that does more justice to the logical positivism than the standard account, and suggest some further lines of research that it opens up.

14.2 The Standard Account and Its Problems

The standard account consists of basically three components: a claim about the general philosophical perspective, a claim about the style of philosophizing, and a claim about the picture of science. Its advocates, such as Brown (1977), Giere (1988), Hacking (1981), McGuire (1992), Shapere (1966) and Suppe (1977), argue that the discipline of philosophy of science went through a revolutionary transformation in the 1960s and early 1970s in that the positivist perspective was abandoned, the old style of philosophizing about science, known as "the logic of science", was replaced by a historical approach, and as a result, the old positivist picture of science was dismantled and replaced by a new one that is totally incompatible with it.

According to the standard account, logical positivists were crude positivists or empiricists in the tradition of Hume, Comte and Mach. Their major novelty was to inject modern logic into the traditional positivism/empiricism. For them philosophy is nothing but the logic of science which operates entirely within the context of justification and is concerned with the form rather than the content of scientific statements in general and the formal structure of theories, laws, explanations, evidential relationships between observations and theories in particular. It is essentially a meta-linguistic activity of rational reconstruction and logical analysis of terms and statements such as “law”, “theory”, “explanation” and “evidence E confirms (or disconfirms) theory T”.

Early postpositivists replaced the logic of science with the historical approach. They dispensed with logical analysis completely, adopted a non-formal approach to science and derived their image of science by focusing on the actual practice of science historically. According to the standard account, these two styles of philosophizing about science have produced diametrically opposed pictures of science (cf. Hacking 1981):

1. **Realism:** Logical positivists believed, and early postpositivists rejected, that science aims to discover objective truths (in the sense of correspondence) about one real world.
2. **Cumulativeness:** Whereas logical positivists believed that science progresses through incremental accumulation of facts, early postpositivists claimed that there are radical ruptures and discontinuities in the development of science.
3. **Foundationalism:** Whereas logical positivists believed that observation provides secure grounds for theories, early postpositivists claimed that science has no foundations and that all observational claims are fallible.
4. **Observation/theory dichotomy:** Logical positivists endorsed a sharp double distinction between theory and observation: they argued that there is a fixed and categorical distinction between observational and theoretical terms on the one hand and observable and unobservable entities on the other hand. Early postpositivists rejected such a distinction in both senses.
5. **Meaning:** For logical positivists meanings of observational terms are fixed by observations untainted by theoretical commitments. For early postpositivists all meaning is theory-laden. While the latter endorsed semantic holism in the sense that the meaning of term is given by the theoretical context in which it occurs, the former held no such view.
6. **Structure of theories and explanations:** Logical positivists thought that both have a deductive structure. For them scientific theories are axiomatizable, deductively connected sets of statements. They endorsed the deductive-nomological (D-N) model of explanation. Early postpositivists tended to either reject both or remain uncommitted to neither.
7. **Testing:** Logical positivists claimed, and early postpositivists denied, that scientific theories can be tested in isolation. For the latter testing of theory is always a holistic affair, which is possible only when certain auxiliary hypotheses are added.
8. **Underdetermination:** Whereas logical positivists held that logic and empirical evidence uniquely determine theory choice, early postpositivists claimed that scientific theories are always underdetermined.

9. **Incommensurability:** Early postpositivists argued that rival scientific theories such as classical and relativistic mechanics are incommensurable in that (a) there are no universal or shared criteria on the basis of which they can be compared and (b) there are certain statements in one theory that cannot be translated into the other without loss of meaning. For that reason, advocates of rival theories often talk past each other and communication between them breaks down. Incommensurability was anathema to logical positivists.
10. **Rationality:** For logical positivists science is a rational activity guided by explicit methodological rules common to all scientific disciplines. Rival theories can be rationally compared in terms of universal criteria. By contrast, because of incommensurability in the sense (9a) above, early postpositivists argued that theory choice is always influenced by social and personal (subjective) factors.
11. **Context of justification-context of discovery:** Whereas logical positivists endorsed a sharp distinction between these two contexts and argued that philosophy is concerned only with the former, early postpositivists rejected these claims.
12. **Unity of science:** For logical positivists all sciences are unified. Less developed sciences are reducible to more developed ones, ultimately to physics. Early postpositivists claimed that the sciences are plural and disunified.

The advocates of the standard account do not of course claim that the 12 theses attributed to logical positivists were all held by all of them. Nor do they even claim that they can be attributed to a single philosopher. They do however believe that "they form a useful collage not only of technical philosophical discussion but also of a widespread popular conception of science" for which logical positivists are largely to be blamed (Hacking 1981, 2). All the same, the standard account will not do. First of all, as the recent revisionary history of the philosophy of science has revealed, with the exception of 6, 11 and 12, 9 out of the 12 theses attributed to early postpositivists were also held by at least one logical positivist. Let me review them very quickly. While Reichenbach and Schlick were realists, Carnap and Neurath were not. Carnap, Reichenbach and Frank all rejected accumulationism. After the famous protocols debate of the early 1930s, logical positivists rejected foundationalism and became fallibilists about even the simplest observation statements. Neither Carnap nor Reichenbach believed in a sharp distinction between theory and observation. A form of semantic holism, incommensurability in the sense of untranslatability and a moderate holism with respect to testing can all be found in Carnap's mature writings. For that reason, underdetermination was not anathema for Carnap at all, and Neurath used it to explain how social factors creep in scientific theory choice. Finally, Carnap's conception of rationality is much closer to Kuhn's than to Popper's.[1]

[1] For these points, see Coffa (1991), Creath (1990), Friedman (1999, 2001), Hardcastle and Richardson (2003), Irzik (2003), Irzik and Grünberg (1995), Richardson and Uebel (2007), Reisch (1991, 2005) Uebel (2011) and the literature cited therein.

None of this is meant to deny the existence of a deep divergence of views between logical positivists and early post-positivists with respect to 6, 11 and 12. The latter were hostile to deductivism in matters related to explanation and theory structure. They rejected the D-N model of explanation and the notion of a theory as a deductively organized set of statements, replacing it with a semantic conception. Kuhn in particular adopted the Sneed-Stegmüller view of scientific theory according to which a theory is a model-theoretic structure consisting of a set of distinct applications which all share the same basic law or laws and of a set of constraints binding the applications together.

The standard account is of course right when it claims that a break between the two camps occurred with respect to the style of philosophizing about science. Indeed, the early post-positivists rejected the logic of science as a way of understanding scientific activity and instead turned to history of science as a resource for deriving their theses about science. However, even here the story is more complicated than the standard account suggests. Philipp Frank, a major representative of logical positivism especially after WWII, believed that a logico-structural analysis of science was not sufficient for a deep understanding of science and must be supplemented by a pragmatic-historical one. He thus emphasized the importance of history of science for doing philosophy of science and often appealed to historical cases such as the Copernican revolution for insight into scientific development. He paid particular attention to the issue of theory acceptance and noted, much as Kuhn did, that theory acceptance is not just a matter of match between theory and observational facts since no theory ever agrees with all facts completely, but only more or less. In addition to agreement with facts, he also underlined the importance of the criterion of simplicity (understood both as mathematical simplicity and as simplicity of the overall theoretical discourse) and drew attention to the problem of how to weigh these two factors in choosing among rival theories, an issue that Kuhn dealt with especially in his influential 1973 article "Objectivity, Value Judgment and Theory Choice," reprinted in Kuhn (1977).[2] This is not to deny of course that early post-positivists employed history of science much more widely and were themselves engaged in detailed historical case studies that informed their picture of science.

As a final point, I must emphasize that the standard account also goes wrong when it completely ignores the neo-Kantian roots of logical positivism/empiricism. This point is brought home in particular by Michael Friedman's *Dynamics of Reason* (Friedman 2001). What follows owes much to this pioneering study.

[2] For a fuller discussion of the similarities and differences between Frank's and Kuhn's views, see Nemeth (2007) and Reisch (2005, 229–233).

14.3 The Meaning of the Internal-External Distinction

In his *The Theory of Relativity and* A Priori *Knowledge*, Reichenbach noted that Kant had distinguished between two senses of the a priori: that which is universal, necessary and unrevisable and that which is constitutive of the objects of experience. He argued that in the light of the revolutions in physics during the first quarter of the twentieth century the first sense of the a priori must be abandoned but that the second sense could be retained and applied to the theory of relativity. Friedman (2001) took up Reichenbach's idea and developed it into a full theory of the development of science. He argued that every physical theory consists of empirical, a posteriori elements on the one hand and constitutive a priori principles on the other hand. The former, he argued, were made possible by the latter; more specifically, the latter made the former's testing possible. He further argued that the constitutive a priori principles changed through major revolutions. He showed what these principles were in the context of classical Newtonian and relativistic physics and how they functioned and changed.

The idea of historically changing constitutive a priori principles naturally suggests a distinction between two kinds of scientific activity: that which is carried out within a given framework in which constitutive a priori principles are in place and that which transgresses and disrupts it, resulting in a radical transformation in those principles. This is what I mean by the internal-external distinction and propose to employ it as a resource for a suggestive narrative as an alternative to the standard account. While I do not pretend that a single distinction can do justice to the richness and complexity of a 50-year long history, I do claim that it provides a perspective that captures an important part of it.

Internal scientific activity is typically guided by a set of principles that are constitutively a priori which either (a) make problem solving and the testing of other (empirical, a posteriori) claims possible or (b) partially constitute the meaning of primitive terms that occur in those principles. Once such principles are adopted by the scientific community, they are not allowed to be refuted by isolated experiments. Nevertheless, though a priori, they do change historically as science progresses. Thus, unlike Kant's, constitutively a priori principles are at the same time historically relativized and thus historically a priori. By contrast, external scientific activity is revolutionary activity that challenges, disrupts and transgresses the existing framework and gives rise to new constitutively a priori principles.

14.4 The Internal-External Distinction Applied

Now, I argue that while Reichenbach, Carnap, Kuhn, Lakatos and Putnam endorsed the internal-external distinction in one form or another in their philosophies of science, Neurath, Popper, Feyerabend and Quine rejected it, thus breaking with the neo-Kantian heritage of which the notion of constitutive a priori is a descendant.

This gives a novel classification which cuts across the standard logical positivist-postpositivist divide. Naturally, the second group is, doctrinally speaking, less unified (and therefore more heterogeneous) than the first one since they are defined negatively (in terms of the denial of the internal-external distinction).

As already mentioned, constitutively a priori principles occupy a central place in Reichenbach's *Relativity Theory and* A Priori *Knowledge*. The term Reichenbach used was "coordinative axioms". By mediating between abstract mathematical structures and concrete experience, such axioms made the testing of scientific hypotheses possible. As Friedman pointed out, since such principles make testing possible, they themselves cannot be tested during ordinary scientific activity, but they can be revised in a wholesale fashion, a revision that takes place only during scientific revolutions.

That Kuhn based his entire picture of science on this distinction in his seminal book *The Structure of Scientific Revolutions* needs no argument even though he used a different terminology, namely, the normal vs. revolutionary science distinction. Here I will be content to point out what Kuhn says about symbolic generalizations: They "function in part as laws but also in part as definitions of some of the symbols they deploy" (Kuhn 1970, 183). This is precisely what constitutively a priori principles do. No wonder then for Kuhn they are not open to refutation by isolated experiments during normal science, but can only be rejected wholesale during a revolution. Indeed, Kuhn says "I currently suspect that all revolutions involve, among other things, the abandonment of generalizations the force of which had previously been in some part that of tautologies" (ibid. 183–184).[3]

We find a similar view in early Putnam as well, a philosopher who was one of the staunchest critics of the post second world war logical positivism as it was understood in the USA. In his well-known paper "The Analytic and the Synthetic", Putnam introduced the notion of "framework principles". According to him, Newton's second law of motion and the postulates of Euclidean geometry are examples of framework principles. Regarding them Putnam wrote:

> '[F]ramework principles' ... have the characteristic of *being so central that they are employed as auxiliaries to make predictions in an overwhelming number of experiments, without themselves being jeopardized by any possible experimental results*. This is the classical role of the laws of logic; but it is equally the role of certain physical principles,

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[3] In his post 1980 writings in which he reformulates his earlier views in terms of taxonomic lexicons, Kuhn makes explicit reference to the constitutive a priori. He writes: [My] structured lexicon resembles Kant's a priori when the latter is taken in its second, relativized sense [in the sense of 'constitutive of the concept of the object of knowledge']. Both are constitutive of possible experience of the world, but neither dictates what that experience must be. Rather, they are constitutive of the infinite range of possible experiences that might conceivably occur in the actual world to which they give access (Kuhn 1993, 331). And again: "By now it may be clear that the position I am developing is a sort of post-Darwinian Kantianism. Like the Kantian categories, the lexicon supplies preconditions of possible experience. But lexical categories, unlike their Kantian forebears, can and do change, both with time and with the passage from one community to another (Kuhn 1991, 12).

e.g., '$f = ma$,' . . . the laws of Euclidean geometry, and the law '$e = mv^2/2$', at the time when those laws were still accepted. (Putnam 1975, 48–49)

Putnam's framework principles function just like Kuhn's symbolic generalizations and are no different from constitutively a priori principles in the sense I have introduced (see Tsou 2009). Putnam discusses the framework principles in the context of scientific change, and the discussion of his example regarding the kinetic energy $e = m\mathrm{v}^2/2$ clearly indicates that the framework principles only change during scientific revolutions. Thus, Putnam seems to be saying that there are two kinds of scientific activity, the normal and the revolutionary in Kuhn's sense, or the internal and the external in the sense I have defined it.

Consider now Carnap's views about scientific change. As I have argued elsewhere (Irzik and Grünberg 1995; Irzik 2003), much like Kuhn, Carnap too distinguishes between normal scientific activity within a framework characterized in part by physical postulates and revolutionary activity that transgresses it. The latter is characterized in terms of a radical change in the framework rules or a change in the theoretical postulates (Carnap 1963, 921). Again, echoing Kuhn, Carnap holds that these postulates have both a factual and a semantic function. They not only serve as laws, but also introduce primitive theoretical terms and partially interpret them. Thus, though they are synthetic, they do not change their truth-value during "normal" science:

To be sure, this status [analyticity in a language] has certain consequences in case of changes of the second kind [changes in the truth values of indeterminate statements], namely, that analytic sentences cannot change their truth-value. But this characteristic is not restricted to analytic sentences; it holds also for certain synthetic sentences, e.g., physical postulates and their logical consequences. (Carnap 1963, 921)

This suggests that the theoretical postulates behave just like meaning postulates, forming a part of the linguistic framework. There is further textual evidence for this. For example, consider the following passages from *Studies in Inductive Logic and Probability*: "The basic laws of the theories are incorporated into the language Lt as theoretical postulates". (Carnap and Jeffrey 1971, 51); ". . .laws of nature (which may sometimes be taken as B-postulates)" (ibid., 94), where a B-postulate is a basic assumption accepted generally for any language L, not just for a special investigation (ibid., 80). Finally, look at this passage in Carnap's reply to Putnam in the Schilpp volume:

It seems to me that a basic role should be assigned both to the primitive magnitudes of the theoretical language and to the [theoretical] postulates and correspondence rules. Since I regard d.c. [degree of confirmation] as always relative to a language, the influence of both factors on the values of d.c. is in accord with my conception. (Carnap 1963, 988)

Though Carnap did not use the term "constitutive a priori", I think there is ample evidence that his physical postulates (or what he later calls B-postulates) function very much like constitutively a priori principles since they partially constitute meanings and make confirmation possible.

Finally, take Lakatos's methodology of scientific research programs (Lakatos 1970). A scientific research program (SRP for short) consists of a hard core, a positive and a negative heuristic. The hard core consists of fundamental laws or principles, such as the three laws of motion, in a Newtonian SRP. The positive heuristic contains suggestions as to what to do when the results of observations or experiments are inconsistent with the SRP. The negative heuristic basically says that the hard core should never be held responsible for such inconsistencies; hence, it has the methodological function of protecting the hard core from refutation. Thus, we can say that the negative heuristic is methodologically a priori if not constitutively so. Accordingly, for Lakatos a scientific revolution occurs when one SRP is replaced by another only when it degenerates. This suggests that we can interpret Lakatos's views about scientific development in terms of the internal-external distinction. Scientific activity within a SRP is an internal one, and activity which results in abandoning the SRP's hard core is an external one, giving rise to scientific revolution.

This concludes my argument to the effect that Carnap, Kuhn, Putnam, Reichenbach and Lakatos all either explicitly or implicitly endorsed something like the distinction between scientific activity within a framework and activity outside it. By contrast, there is no room for either this distinction or the notion of constitutively a priori principles in Neutrath's, Popper's, Feyerabend's and Quine's philosophies. In particular, the first three find anything like "normal science" or "scientific activity within a framework" too dogmatic, conservative, or stagnating for scientific progress.

14.5 The Fruits of the Internal-External Distinction

This is interesting, one might say, but is it enough to generate an entire revisionist historiography? Certainly not, but there is more. The internal-external distinction is often accompanied by the endorsement of the notion of analyticity. Again, we find the first group of philosophers (with the possible exception of Lakatos) endorsing it, and the second group rejecting it. This is not surprising since "analytic" is often defined as "analytic-in-L", relative to an already accepted framework. Carnap's heroic efforts to define analyticity are well known. What is perhaps less well known is the fact that Kuhn too appealed to that notion in his post-1980 writings once he introduced the notion of a scientific lexicon: "At this point I will seem to be introducing the previously banished notion of analyticity, and perhaps I am. Using the Newtonian lexicon, "Newton's second law and the law of gravity are both false" is itself false. Furthermore, it is false in virtue of the meanings of Newtonian terms 'force' and 'mass'" (Kuhn 1990, 317, n. 17). In a similar fashion, Putnam affirms the analytic-synthetic distinction while rejecting the analytic-synthetic dichotomy, as Tsou (2009) put it.

As for Reichenbach, it is possible to argue that he too held that there are analytic truths since he believed that "there is synonymy, there are equivalent descriptions" (Glymour 2012). Once we have synonymy, of course, there is truth in virtue of meaning, and hence analytic truths.

Moreover, those who employ the internal-external distinction also take questions of meaning seriously. For analyticity is often defined in terms of truth in virtue of meaning, and to the extent to which framework principles contribute to the constitution of the meaning of terms that occur in them, there is a tight connection between them and analytic truths. It is no wonder then Carnap, Kuhn and Putnam devoted considerable attention to the constitution of the meaning of scientific terms and embraced a form of semantic holism. By contrast, Neurath, Popper and Quine (but, admittedly, not Feyerabend) shun all questions of meaning.

Finally, those who endorse the internal-external distinction believe that the rationality of science should also be treated similarly. In other words, according to the first group, standards which apply to the evaluation of internal scientific activity do not in general apply to the external scientific activity. By contrast, the second group denies this and employs more or less the same standards across the board. Relatedly, Carnap, Kuhn and Lakatos in the first group see internal activity as accumulative but take external activity as discontinuous and deny that science progresses toward truth, where truth is understood as correspondence between statements and a mind-independent reality. Not surprisingly, the members of the first group tend to be anti-realists of sorts.[4]

Thus, the internal-external distinction sheds considerable light on a number of issues that have divided philosophers of science in much of the twentieth century, and the ensuing division is very different from the one that emerges on the basis of the standard historiography. The perspective I suggest for a new historiography of the philosophy of science, of course, needs to be supplemented by a detailed examination of specific fault lines that are equally divisive. These include the debates around the theory-observation dichotomy, theory structure, the nature of scientific explanation, the unity of science, and the context of discovery- context of justification distinction that have preoccupied philosophers of science from the 1950s to the mid-1970s and form the basis of real disagreement between logical positivists/empiricists and the early post-positivists. We need to understand more deeply how these debates shaped our understanding of logical positivism and became decisive in undermining it. We would also do well if we paid more attention to the "lesser" figures such as James Conant, Philipp Frank, Herbert Feigl, Ernest Nagel and Carl Hempel as these philosophers of science played pivotal roles in both creating the standard image of logical positivism/empiricism and shaping the agendas in the 1950s and 1960s.

The perspective provided by the internal-external distinction that I suggested in this chapter through which we can rewrite a considerable portion of the twentieth century analytical philosophy of science has a number of advantages over the standard account. First, it does justice to the recent revisionist work on logical positivism/empiricism and highlights the already discovered similarities between logical positivists/empiricists and early postpositivists. Second, it unearths new continuities between the two movements. Third, it enables us to link the history

[4] The exception to this of course is Reichenbach who is a scientific realist.

of philosophy of science to the general history of philosophy in a more revealing way; thus, contrary to the standard historiography that places logical positivism/ empiricism exclusively within the empiricist-positivist tradition of Hume, Comte and Mach, my proposal enables us to see the lineage from Kant to Kuhn through Reichenbach and Carnap as well as to appreciate the reactions against it by Popper, Quine and others.

A final advantage of my proposal is that it broadens the scope of our narrative to include certain aspects of the so-called (contemporary) continental philosophy of science as various forms of the internal-external distinction are also central to the views of science of such philosophers as Bachelard, Canguilem, Foucault, and Althusser. All of these philosophers take the development of science as discontinuous, as marked by radical epistemic ruptures. In particular, there is a distinct parallel between Foucault's notion of "the regime of truth" and Kuhn's paradigm, and it is revealing that both the term and the notion "historical a priori" is central to Foucault's philosophy of science. These are themes that I believe are worth investigating. In this way, we can open up new areas of research and contribute to the bridging of the "analytical" and "continental" traditions.

Some Personal Remarks

It is a great honor for me to contribute to this volume dedicated to Kostas Gavroglu. I first met Kostas in 1996 or 1997 when he and several other members of Athens University visited the Philosophy Department of Boğaziçi University, where I was then teaching, for a possible collaboration between the two institutions. I was instantly drawn to his warm and charming personality, accentuated by his resemblance to Einstein. This marked the beginning of a lasting friendship that I will cherish as long as I live.

The meeting was a huge success, which led to the signing of a memorandum of understanding between Boğaziçi and Athens Universities. Accordingly, a series of biennial conferences alternating between Athens and Istanbul was planned, and the first of these was successfully held in Athens in March 1998, the second one in Istanbul in May 2000, and so on for a number of years. At the end of each conference all participants gathered to decide the topic of the next conference, and it is in these meetings that I had the chance to witness closely Kostas's gentle leadership–constructive, creative and fair to all. It was mainly due to his efforts that these conferences came to welcome graduate students, who benefited from them greatly, and to expand toward topics outside of but relevant to HPS. Kostas was also a skilled facilitator who was able to ease the tensions that occasionally arose among scholars from different disciplines and bring their diverse interests into harmony, without compromising the common goal of these conferences.

He is no doubt one of the most distinguished historians of science of our times. Anyone who is familiar with his works knows his superb scholarship even in the toughest areas of history of science, encompassing insights from philosophy, sociology, social and cultural studies of science. But those who know him well also know that he is a shrewd intellectual who is well versed in literature, art and politics. He closely follows the developments in these areas in Turkey as well and always surprises me by talking about works whose existence I did not even know about but should have or by offering fresh perspectives on a topic that I thought I knew well.

I remember the workshop on "The Idea of Nature in the Culture of Tanzimat" organized by the eminent Turkish sociologist Şerif Mardin of Sabanci University in Doğanbey near Söke, Izmir in the summer of 2002. It brought together leading Turkish experts in history, philosophy, social sciences, and literary theory and was held in an unusual, cozy environment (a large wooden house in an old, small village to be exact), which contributed to the success of the meeting immensely. Kostas presented a paper that emphasized the importance of adopting "the appropriation model" rather than "the transfer" one in understanding the dissemination of the new scientific ideas especially in multicultural societies of the European periphery like the Ottoman Empire. As is well known, the Ottomans missed the seventeenth century Scientific Revolution and other scientific developments following it, and this created severe anxiety among Ottoman and Turkish scholars. The dominant attitude has been either lament ("if only so and so was not translated into Ottoman so late") or a forced attempt to show that "Ottomans had that idea in some form or another too". Kostas of course knew this and finally said, "look, what do you make of a geography book written in French in 1797 by a Turk living in England and translated into Turkish by a Greek living in Vienna? Can you still make sense of this case as simply one of transmission of ideas and knowledge transfer"? The audience broke into laughter, and the discussion took an entirely different direction from that point on.

Kostas worked hard, perhaps harder than anybody, for the development of history of science in countries in the periphery of Europe and produced exemplary works that are embodiments of the universal within the local. Few scholars of Kostas's stature are as modest and as generous as he is. He is always there for any one –colleagues and students alike–who needs his help. He has a great talent in stimulating and inspiring others. I am privileged to know him and proud to call him "my friend".

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Chapter 15
Concepts Out of Theoretical Contexts

Theodore Arabatzis and Nancy J. Nersessian

Abstract In this paper we take as our point of departure Kostas Gavroglu and Yorgos Goudaroulis's insight that, in the process of describing and explaining novel phenomena, scientific concepts are taken "out of" their original theoretical context, acquire additional meaning, and become relatively autonomous. We first present their account of how concepts are re-contextualized and, in the process, extended and/or revised. We then situate it within its philosophical context, and discuss how it broke with a long-standing philosophical tradition about concepts. Finally, we argue that recent developments in science studies can flesh out and vindicate the "concepts out of contexts" idea. In particular, historical and philosophical studies of experimentation and cognitive-historical studies of modeling practices indicate various ways in which concepts are formed and articulated "out of context."

Keywords Concepts • Contexts • Experimentation • Meaning • Models

15.1 Introduction

In 1989 Kostas Gavroglu coauthored, together with Yorgos Goudaroulis, a philosophical history of the development of low temperature physics (Gavroglu and Goudaroulis 1989) (hereafter, G&G). It was a significant book for both of us. It came out in a series edited by Nancy Nersessian, and its intriguing ideas contributed to Theodore Arabatzis's decision to switch careers from engineering to history and philosophy of science, and subsequently to our introduction to one another. Three of those ideas we find particularly important. First, concepts are formed and articulated via problem-solving:

> The meaning of concepts is not only due to their definitions or to the fact that some are derivable from others. That is, their meaning is not merely determined from their position in

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> the conceptual "hierarchical structure" of a particular theory. The role of concepts during . . . the problem solving process is also a determining factor. (G&G, 27)

Second, during the problem-solving process concepts are taken "out of" their original theoretical context, acquire additional meaning, and become relatively autonomous: "This autonomy has been achieved due to the excess meaning the concepts acquired from their use in attempting to [describe or] explain . . . unexpected phenomen[a]" (ibid.). Thus, when they are used to account for unexpected phenomena, concepts expand. Three, when concepts are taken out of their context(s) paradoxical situations arise, the resolution of which results in fundamental conceptual change.

These were ground-breaking insights at a time when many philosophers of science, bewitched by Kuhn's and Feyerabend's historical philosophy of science, saw the meaning of scientific concepts as fully determined by the theoretical framework in which they are embedded. In what follows, we will sketch G&G's analysis of how concepts are re-contextualized and, in the process, extended and/or revised. We will situate it within its philosophical context, and discuss how it bears upon (and has enriched) our own work. We hope thereby to support and extend further G&G's insights.

15.2 The Primacy of Theory

When *Concepts out of Contexts* was published it was common to consider scientific concepts as theory-laden. A lot of philosophical ink had been spilled on spelling out their meaning in terms of their location within a systematic theoretical framework.[1] In the "orthodox" view (Feigl 1970), the culmination of logical empiricism, there were two kinds of scientific concepts: observational and theoretical. The meaning of the former was fully specified by their direct association with observable entities, properties and processes. The meaning of the latter, on the other hand, derived partly from the system of "postulates" in which they were embedded and partly from "correspondence rules" which linked those postulates with a domain of phenomena. Thus, the meaning of theoretical concepts was determined, indirectly, by their links via scientific laws with other theoretical concepts, and by their connections, via correspondence rules, to observational concepts.

Through the contribution of correspondence rules, the meaning of theoretical concepts is shaped, in part, by experimental procedures and operations.[2] Surprisingly, though, despite its empiricist orientation, the orthodox view downplayed the connection between theoretical concepts and observation and experiment. For instance, as admitted by Carnap in "The Methodological Character of Theoretical

[1] For a historical survey of the philosophical literature on the meaning of scientific concepts see Arabatzis and Kindi (2013).

[2] See, e.g., Carnap (1936), Hempel (1952).

Concepts," "in agreement with most empiricists ... the connection between the observation terms and the terms of theoretical science is much more indirect and weak than it was conceived ... in my earlier formulations" (Carnap 1956, 53; cf. Feigl 1970, 7). Furthermore, as some of its critics pointed out, the orthodox view neglected the use of theoretical concepts in experimental contexts. Theoretical concepts, such as "positron," are related to descriptions of experiments and outcomes ("observation sentences"),[3] but not to the experimental practices themselves, that is, not to the concept's role in the processes of experimental problem solving.

The tenuous connection between scientific concepts and experiments was loosened further with the rise of historicist philosophy of science. Feyerabend, for instance, claimed that "the fact that a statement belongs to the observational domain has no bearing upon its meaning" (1962/1981, 52). Rather, "*the interpretation of an observation language is determined by the theories which we use to explain what we observe, and it changes as soon as those theories change*" (1958/1981, 31; emphasis in the original). As regards the meaning of scientific concepts, Feyerabend opted for "regarding theoretical principles as fundamental and giving secondary place ... to those peculiarities of the usage of our terms which come forth in their application in concrete and, possibly, observable situations" (Feyerabend 1965/1981, 99). Thus, the meanings of scientific concepts (observational and theoretical alike) are "dependent upon the way in which ... [they have] been incorporated into a theory" (Feyerabend 1962/1981, 74).

The theory dependence of concepts implies that theory change leads to conceptual change. Moreover, since meaning determines reference on this view, the older concepts and their descendants refer to completely different entities. The stock example is that of the concept of "mass" in Newtonian mechanics and in relativity theory. Since in Newtonian mechanics "mass" is invariant, but in relativistic mechanics "mass" is dependent upon velocity, the terms cannot have the same referents. Thus, the very subject matter of scientific investigation shifts along with conceptual change. We would like to stress that the fluidity of scientific ontology over time follows from an explicit decision to ignore any evidence in the historical record of the stability of concept use at the observational (and, we would add, experimental) level and to focus exclusively on the theoretical frameworks in which concepts are embedded. This exclusive preoccupation with 'high' level theory was bound to overemphasize the unstable characteristics of scientific concepts at the expense of their stable features, associated to a significant extent with 'low' level methods of measurement and identification of the referents of scientific concepts in experimental contexts.

Furthermore, the exclusive preoccupation with the role of fundamental theories in concept formation led to a neglect of the interplay between concepts and experimentation. On the one hand, experimental interventions are often crucial

[3] Cf. Feyerabend (1960/1999, 18–19, 20–21), Putnam (1962/1975, 217), and Hempel (1973/2001, 212).

for the formation, articulation, and sometimes the failure of scientific concepts.[4] On the other hand, concepts frame and guide experimental research. We will say more about this below but, as a brief example, Faraday's concept of 'lines of force' (later 'field') as processes acting in the space surrounding magnetic sources and charges framed his research program and directly guided his design of experiments to detect possible *line-like* motions, such as vibrating or turning corners, in the transmission of electric and magnetic actions.

15.3 The Ways of Paradox

The innovative character of *Concepts out of Contexts* can be seen most clearly if we contrast it with this theory-dominated account of scientific concepts. G&G did not deny that concepts are partly determined from the theoretical framework in which they are embedded. They pointed out, however, that, when they are used to describe and/or explain novel (unexpected) phenomena, concepts acquire "excess" meaning (G&G, xii). As a result of their enrichment, concepts may come to be at odds with the theoretical framework "out of" which they originated. The ensuing tension gives rise to "paradoxical situations", which are "created when … [a] new phenomenon, as … translated into … [the] descriptive language [of a theory], is irreconcilable with the concepts and mechanisms of … [that] theory" (G&G, xi). To put it another way, a new phenomenon, when described with familiar concepts, turns out to be at odds with their theoretical presuppositions. The elimination of such a paradox goes hand in hand with the revision of the concepts that gave rise to it.

An example from the history of low temperature physics will illustrate this process of recontextualizing concepts. Helium was liquefied by Heike Kamerling Onnes in 1908. By the late 1930s the behavior of liquid helium had wreaked havoc with the classical concept of viscosity. That concept was associated with the internal friction of fluids and also had an operational dimension. There were two distinct methods for measuring viscosity: rotating a disc immersed in a liquid and observing the rate of its deceleration; and letting a liquid pass through tiny capillaries. Those methods had led to identical results for all liquids. However, in the case of liquid helium below a certain temperature (2 K) it turned out that those two methods led to fantastically different results. The "first gives a value that is a million times larger than the second" (Gavroglu 2001, 165). This discrepancy undermined the coherence of the theoretical and the operational dimensions of the concept of viscosity. The internal friction of liquid helium manifested itself only under the specific circumstances associated with the first method of measuring it. Under different circumstances, such as those associated with the second method, it vanished without a trace. This paradoxical situation indicated that liquid helium

[4] Cf. Steinle (2009).

was not a normal fluid; rather it had to be reconceptualized as a 'superfluid' (cf. G&G).

The failure of the viscosity concept in providing a coherent description of low-temperature phenomena can be understood by taking into account the two-dimensional character of scientific concepts. Scientific concepts have a theoretical dimension—originating from the theoretical framework in which they are embedded, and an experimental dimension—specific ways of operationalizing them in experimental contexts. Of course, these dimensions are not independent; rather, the latter is the "material realization" (Radder 1995, 69) of the former. Furthermore, if different material realizations are associated with the same concept, they should lead to the same results. For instance, there shouldn't be a discrepancy between two different ways of measuring temperature, using mercury and resistance thermometers. If that happened the coherence of the concept of temperature would be undermined. The emergence of incoherence in a concept is a sign of its failure to be applicable to a novel experimental situation.

Paradoxical situations also arise in explaining new phenomena via modeling: "When a concept is chosen for an explanation of an unexpected phenomenon there is always a change in its meaning since it is now in a context different from the one out of which it was initially derived." (G&G, xi) To put it another way, when we use a concept to account for a novel phenomenon, we have to endow the referent of that concept with new properties, some of which may be at odds with its antecedently established properties. Again, a historical example will illustrate this scenario. As is well known, in 1911 Rutherford's nuclear model of the atom was put forward to account for the results of the scattering experiment by Geiger and Marsden. The model was developed using familiar concepts from classical mechanics and electromagnetic theory (orbital motion, positive and negative charge, etc.). It turned out, however, that the kind of physical structure represented by the model was forbidden by those very theories. Such a structure would be mechanically and electromagnetically unstable (Heilbron and Kuhn 1969). Classical concepts enabled a model that was excluded by classical laws![5] The model, thus, highlighted the need for a revision of its conditions of possibility. The paradox was resolved with the development of a new mechanics of the atom, and the rejection of the concept of orbital motion for the subatomic realm.

The above examples show in a striking way that scientific concepts have a life of their own, independently of the theoretical framework in which they are embedded. Their autonomy derives from their use in describing and modeling experimental phenomena. Their meaning is partly shaped by information obtained from experiment.

[5] Cf. Nersessian (1984, 93).

15.4 The Return of Experiment

For some time now, experimentation has become the object of sustained philosophical scrutiny. Philosophers of experiment have focused on the authentication of experimentally produced knowledge by means of a variety of epistemological strategies. Perhaps the main insight of the 'experimentalist' literature has been the relative autonomy of experimentation and its complex relationship to theoretical knowledge, from specific models of phenomena to phenomenological laws to deep, unifying principles.

Concept formation and experimentation have a reciprocal relationship. The development of new concepts, such as 'electromagnetic field', can guide the very detection and stabilization of novel experimental phenomena (such as, the rotation of the plane of polarized light by a magnetic source or the detection of electromagnetic waves) (Gooding 1990; Steinle 2005). Furthermore, the explanation of experimentally produced novel phenomena often requires new concepts of the entities and processes that underlie those phenomena (such as the explanation of the rotation of a magnetic needle by a current-carrying wire ultimately requiring the novel concept of field and the development of electromagnetic theory). The refinement and articulation of those 'theoretical' concepts play, in turn, an important role in experimental research.

One of the manifestations of the autonomy of experiment is the relative independence of the concepts employed in experimental settings from the wider theoretical environment in which they 'live'. Concepts can be taken out of their theoretical context to frame experimental research and, in turn, can be shaped by it, independently from the theory they had been living in, as G&G showed with the concept of viscosity. Sometimes, as in that case, they even fail, by becoming incoherent as a result of experimentally obtained information.

The failure of concepts can be particularly instructive as to the surplus content they obtain when they are used in experimental contexts and out of the theoretical context in which they originally obtained their meaning. When new experimental phenomena are discovered their very description and explanation is attempted in terms of antecedently available concepts. As we have seen, however, this process can sometimes lead to tensions and paradoxes that indicate the limitations of those concepts and the need for their revision.

Concepts can also fail in the process of explaining new experimental results. An instance of this type of failure is provided by the 'discovery' of spin in 1925. Spin emerged in the process of coming to terms with the 'anomalous' Zeeman effect, the patterns of magnetic splitting of spectral lines that could not be accommodated by the classical theory of electrons. Those patterns could be explained by the 'old' quantum theory of the atom on the assumption that the electron was a charged spinning sphere, whose rotation about its own axis turned it into a tiny magnet. In order for the electron to have the experimentally indicated magnetic moment, the tangential speed of its surface should be about ten times the velocity of light! Thus, the new property attributed to the electron in the process of accommodating recalcitrant experimental results was at odds with relativity theory. Thus, the concept of spin seemed to violate generally accepted theoretical constraints. That problem was met with a recasting of spin as a non-visualizable property peculiar to quantum mechanics.

All the examples we have considered show that experimentation is crucially involved in the formation and articulation of concepts. Even theoretical concepts, such as ‘electron’ and ‘field’, are shaped by information obtained through observation and experiment. A focus on the experimental content of concepts enables understanding their trans-theoretical character, the extent to which their meaning is independent from theory.

Before we proceed we need to distinguish two varieties of scientific concepts. Concepts of the first variety are formed in the early, exploratory stages of the development of a field with a primarily descriptive and classificatory aim, namely to bring order into an area of natural or experimentally created phenomena. We have in mind concepts such as Faraday’s ‘lines of force’. The formation and articulation of these concepts and the establishment of observable facts and regularities are two aspects of a single process. For lack of a better term, we may call them ‘phenomenal’ concepts. Several scholars, most notably David Gooding and Friedrich Steinle, have examined the emergence of such concepts in exploratory experimental settings. So our focus here will be on a second variety of concepts that develop in later, and more advanced, phases of the research process. Their function is to explain already established facts and regularities. They usually refer to hidden entities (H-E), e.g. ‘electron’, and processes (H-P), e.g. ‘field’, that lie deeper than (and give rise to) observable effects.

The development of such concepts is part and parcel of theorizing about the laws or mechanisms in the hidden domain under investigation. In that process of concept formation and theory construction, experimentation plays a substantial role. New H-E concepts are introduced to explain laboratory phenomena and are, in turn, shaped by information drawn from them. For instance, after J. J. Thomson had put forward the concept of “corpuscle” in order to account for various phenomena observed in the discharge of electricity in gases at low pressures, he inferred from those phenomena that corpuscles had a minute mass-to charge-ratio (three orders of magnitude smaller than the atom’s). Similar inferences for related concepts, such as H. A. Lorentz’s concept of ‘ion’, were drawn from other experiments (e.g., by Zeeman on the magnetic splitting of spectral lines). The convergence of such experiment-driven results led to an amalgamation of ‘corpuscles’ and ‘ions’, under the umbrella term ‘electrons’, and a unified understanding of different phenomena as manifestations of electrons (Arabatzis 2006).

Furthermore, when H-E concepts are created for theoretical (explanatory, predictive) purposes, they are most often not fully articulated, either qualitatively or quantitatively at their introduction. The qualitative features that an H-E must have in order to bring about its purported effects are specified only to the extent that is required in order for them to play their explanatory role in the given context. Furthermore, the magnitudes of those features need to be determined by inferring them from the magnitude of the effects under investigation. Thus, H-E concepts are incomplete and provisional in at least three ways. First, they do not exhaust all the properties of their referents; they may have to be either refined or enriched in response to new experimental knowledge.[6] As an example of refinement, consider Lorentz's 'ions'. Originally the term referred to both positively and negatively charged particles, but as a result of Zeeman's magneto-optic experiments 'ions' were delimited to negative particles. As to enrichment, we may consider the development of the concept of the electron in the period of the old quantum theory. New properties, such as spin, were incorporated into that concept in response to the intricacies of experimental spectroscopy.

Second, H-E concepts are articulated quantitatively on the basis of experimentally obtained information. For instance, as we already noted, the results of magneto-optic and cathode ray experiments led to an estimate of the charge to mass ratio of corpuscles/ions and to their identification with electrons. Third, experiment may indicate that familiar properties of an H-E have to be reconsidered and, therefore, that the corresponding concept has to be revised. For instance, the experimental discovery of electron diffraction in the late 1920s challenged the conception of the electron as a particle.

Thus, experimentally produced phenomena guide the construction of H-E concepts by suggesting, in a certain theoretical context, the properties and behavior of the referents of those concepts. In that process, features of experimental phenomena are directly correlated with the properties and behavior of the H-E 'behind' them. For instance, when Niels Bohr put forward his atomic theory in 1913, he made several assumptions about the behavior of electrons inside the hydrogen atom (e.g., electrons were allowed to move only in certain orbits, in conformity with the discrete structure of the hydrogen spectrum). As Robert Millikan noted, "if circular electronic orbits exist at all, no one of these assumptions is arbitrary. Each of them is merely the statement of the existing experimental situation" (Millikan 1917, 209). To put it differently, scientists construct H-E concepts with an eye to the particularities of experimentally obtained information. Thus, H-E concepts acquire additional meaning by being taken out of their original theoretical context.

Motivated by Hacking's insight that "experimentation has a life of its own" (Hacking 1983, 150), we would now like to suggest that the meaning that H-E concepts acquire in experimental contexts is, to a substantial extent, independent from theory. As a matter of fact, the meaning in question is often quite stable. The reason for this is that it derives from experimental knowledge that is not affected by theory change. For example, from the late nineteenth century onwards the experimentally determined properties of the electron, such as its charge and mass, refused to go away, whereas other theoretical features of the electron concept were not as resilient.

To understand how H-E concepts can have an experimental life of their own, we need to appreciate the various levels of 'theory' and their relation to experiment. As philosophers of experiment have emphasized, the term 'theory' is often used loosely to mean just about any kind of knowledge, from general principles or

[6] Cf. Radder (2006, 121): "the meaning of concepts needs to be articulated when they are being extended or communicated to a novel situation." Cf. also Rouse (2011).

laws with a wide scope to particular models of a phenomenon or an instrument. As regards H-E concepts, the following three levels of 'theory' have to be differentiated: First, the high-level theoretical framework in which these concepts are situated. For instance, the concept of the electron was originally found within the framework of classical electromagnetic theory (Maxwell's laws plus Lorentz's force). The second level of theory determines how the nature of H-E is understood and represented. To stay with electrons, they were originally understood and represented as sub-atomic structures in the ether. The third level of 'theory' involves the low-level knowledge of the properties and behavior of H-E that enables their identification in diverse experimental contexts and their (purported) manipulations in the laboratory.

The experimental life of H-E concepts is related to this final level of 'theory', whose robustness has been stressed by, among others, Ian Hacking and Nancy Cartwright (Hacking 1983; Cartwright 1983). Its significance for experimentation on (and with) H-E is quite readily seen by simply glancing at the experimental reports of scientists. For instance, in C.T.R. Wilson's reports of his experiments on β-rays and X-rays there is a notable absence of high-level theory. Wilson attributed different cloud-chamber tracks to different particles, by relying on the effect that the velocity of a particle and its scattering by atoms would have on its trajectory (Wilson 1923). In other cloud chamber experiments, positrons were distinguished from protons on the grounds of the length of their tracks after they had passed through lead: The "length [of a positron track] above the lead was at least ten times greater than the possible length of a proton path of this curvature" (Millikan 1947, 330). The inference from the length of a track to the identity of the entity 'beneath' it was made possible by the low-level knowledge that particles with different sizes are slowed down differently by dense matter.

In all, we see that the history and philosophy of experiment can flesh out and extend G&G's insights about how concepts function out of theoretical contexts and how they are shaped by experimental contexts.

15.5 The Rise of Modeling

Concurrent with the development of a history and philosophy of experimentation, the primacy of theory was challenged from another direction. Beginning in the late 1960s, philosophers have argued that the structures through which scientists engage in problem solving are not theories, but rather models. The first challenge was to the notion that theories are syntactic structures expressed in a formal language with concepts or terms related to the world through correspondence rules or operational definitions.[7] Deemed the "semantic view" of theories, its proponents argued that a theory should be identified with a set of structures of entities or objects that

[7] See, e.g., Carnap (1936), Hempel (1952).

constitute its models, rather than with a specific formal language.[8] This move places models at "center stage" (van Fraassen 1980, 44) because the theoretical "language" describing a class of models is not unique. There is now a vast literature that examines models and modeling practices across the sciences from the perspective of how models "mediate" between theory and the world in problem solving.[9] Much of the modeling literature focuses on the representational relations between models and their target phenomena. More relevant to the "concepts out of contexts" arguments of G&G, though, is the second direction of challenge to the primacy of theory, including the post-positivist positions of Kuhn and Feyerabend, namely the development of a "cognitive" history and philosophy of science. Much of this modeling literature has been concerned with the dynamics of concept formation and change.

The cognitive perspective examines the actual representational and reasoning practices of scientists (historical and contemporary) and draws from and contributes to the scientific literature on human cognitive capabilities and limitations to advance the claim that models are the structures through which scientists both use and create theories.[10] The argument from this perspective is that such scientific practices are reflective outgrowths of mundane human cognitive practices, and that examining the former through the lens of the latter will provide insight into, for instance, the structure of conceptual representations in science and the processes of concept formation and articulation, both of which are model-based. Giere (1994), for example, has drawn from the cognitive science research on categorization to elaborate the structure of Newtonian mechanics in terms of levels of models, specifically, as comprising basic-level models such as that of a specific pendulum instrument to increasingly abstract and more general models, such as that of harmonic oscillation (comprising more phenomena such as a bouncing spring). Nersessian, along with Gooding, Thagard, and Tweney, has examined how analogy, visualization and thought experimentation/simulation—what Nersessian has called "model-based reasoning"—are generative of concept formation and change. In such model-based reasoning, analogical, visual and simulative processes are used to construct (often incrementally) models that embody various constraints drawn from the domain under investigation (target), analogical source domain(s) and, importantly, those that arise in the constructed model itself. These constructed models, in turn, serve as analogical sources from which inferences are analyzed and evaluated towards solving the target problem, which in the case of concept formation is a representational problem. Concept formation and articulation processes, thus, can be understood as the interaction between model-based representations and reasoning in the context of experimental data and theoretical presuppositions within a specific problem situation.

[8] See, e.g., Suppe (1974), Suppes (1967), van Fraassen (1970).

[9] See, e.g., Cartwright (1983), Giere (1988), Morgan and Morrison (1999).

[10] See, e.g., Andersen et al. (2006), Darden (1991), Giere (1988), Gooding (1990), Nersessian (1984, 1992, 2008), Thagard (1992), Tweney (1985, 1992).

For much of the initial research in this area, the practices of Faraday and Maxwell in articulating the H-P concept of field has provided a prominent exemplar. As with the instance of H-E concepts discussed in the previous section, the H-P 'field' concept entered the repertory of physics provisionally, without full specification of its features, and over several generations of investigation the concept interacted with experimentation and theorizing to reach its current representation in relativity theory and quantum mechanics. When nineteenth-century scientists attempted to incorporate electric and magnetic forces into Newtonian theory, they encountered several paradoxical experimental results with respect to the Newtonian concept of force, including (1) a violation of the principle that only like forces act on one another (electric and magnetic forces appeared mutually interacting) and (2) puzzling phenomena in which these forces appeared to be acting in the space surrounding bodies and charges (a possible violation of 'action-at-a-distance'). The striking visual arrangement of the curved lines formed by iron filings in the space surrounding a magnetic source provided a conceptual model of the hidden processes underlying the transmission of magnetic and electric forces that Faraday used in articulating the concept of the lines of force (later 'field'). It guided his development of experiments to detect the possible motions of the lines in transmitting electric and magnetic forces and, ultimately, all the forces of nature, in interaction with his development of a theoretical account of continuous transmission of force in space devoid of matter by means of lines of force and, indeed, of matter itself as point centers of converging lines of force.

Starting from Faraday's experimental data and his general theoretical perspective, Maxwell used (1) the concept of a space-filling mechanical aether (on analogy with the concept of a lumeniferous aether, but with rotational motion as needed to explain Faraday's experimental findings) and (2) conceptual and analytical resources of continuum mechanics to construct models consistent with experimental, mathematical and conceptual constraints to derive a mathematical representation of the H-P concept of electromagnetic field. Nersessian (2008) provides an extended account of how model-based reasoning was used in articulating the field concept, in particular, and how it generates novel concepts more generally.[11] Here we note only how that account can be seen to address G&G's notion of "paradoxical situations" created by using concepts taken out of contexts in attempting to explain novel phenomena. Framed from a cognitive perspective, G&G's paradox is a fundamental paradox of creativity in conceptual change: how is it that starting from existing representations (e.g., concepts taken out of specific contexts) it is possible to construct fundamentally novel representations (concepts irreconcilable

[11] That account has subsequently been further developed and extended to modeling with physical and computational simulations by Nersessian and colleagues in studies of the contemporary model-based reasoning practices in conceptual innovation in the bioengineering sciences (see, e.g., Nersessian 2012). The bioengineering sciences provide fertile ground from which to study conceptual innovation since concepts are routinely taken out of engineering contexts and transferred to explanations and descriptions of biological phenomena, producing abundant paradoxical situations requiring concept formation and change for their resolution.

with them). In the case of Maxwell's construction of the formulation of the electromagnetic field equations, drawing from Newtonian representational systems (continuum mechanics, machine mechanics) to build, incrementally, a series of models representing various electromagnetic phenomena, Maxwell formulated the laws of a non-Newtonian dynamical system. Maxwell began with the hypothesis that electromagnetic actions are continuum phenomena and with constraints stemming from the experimental findings of Faraday and others, such as that the electric and magnetic force are perpendicular to one another and the rotation of the plane of polarized light by a magnetic source, to select an initial source domain from continuum mechanics, viz., elastic fluids. However, no existing system fit the electromagnetic constraints, so he built a series of hybrid imaginary models that embodied various experimental and continuum mechanical constraints, represented the processes in these models mathematically, and transferred the model solutions, with appropriate modifications, to the electromagnetic problems.

To integrate constraints from such disparate domains, Maxwell used various abstractive processes (idealization, approximation, generic abstraction), the nature and effects of which Nersessian (2008) discusses more generally. Nersessian argues that a solution to the paradox of creativity in conceptual change is that processes of re-representation and abstraction afforded by various modes of model-based reasoning (analogy, visualization, simulation) enable the problem solver to bypass some of the constraints of existing representations (concepts taken out of their contexts) through abstracting and integrating novel combinations of constraints stemming from all the sources contributing to model building. The combinations are such that the models exhibit here-to-fore unrepresented structures and processes that, provisionally, can be transferred to the target phenomena and undergo further scrutiny (such as the experimental search by other scientists for electromagnetic wave motion in the Maxwell exemplar).

15.6 Concluding Remarks

We hope to have shown that G&G's insight that concepts obtain part of their meaning in the process of being re-contextualized is borne out by studying concepts in action. By attending to the uses of concepts in experimentation and in theorizing, we have pointed out various ways in which their meaning is shaped by descriptive, explanatory and modeling practices. A quarter century after its publication, *Concepts out of Contexts* still provides a fruitful starting point for understanding the life of scientific concepts.

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Part IV
Historiographical Musings

Chapter 16
The History of Science and the Globalization of Knowledge

Jürgen Renn

Abstract The paper discusses the relation between the history of science and the history of knowledge, including their normative dimensions. It conceives of science as involving cultural abstractions that result from reflections on concrete practices and experiences accumulated along historical trajectories which can only be understood from a global perspective. The approach is illustrated by a sketch of those aspects of a global history of knowledge that shaped the emergence of modern science.

Keywords Globalization • History of knowledge • Abstraction • Normative thinking

16.1 Beyond the Paradigm of Western Science

The history of science has been dominated by the history of Western and in particular European science. Its paradigmatic topic has been the Scientific Revolution of the sixteenth and seventeenth centuries. This Scientific Revolution supposedly gave rise to modern science, not only with specific discoveries but also by establishing a general scientific method comprising the formulation of hypotheses that are then tested by experimentation or observation. Modern science and the scientific method were supposedly developed in Western Europe, first in astronomy and then in physics, and from there conquered the geographical world and the world of knowledge. Even in the traditional account, however, it has been admitted that some of this expansion was achieved by force, by trying to enforce the laws of physics on biology, for instance, or by the colonial expansion of Western science, often accompanied by the violent suppression of other forms of thinking.

Today, this picture is criticized and rejected on the basis of much more fundamental arguments. Philosophers of science have tried in vain to identify the scientific method allegedly at the core of scientific rationality. And historians of

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science no longer see the Scientific Revolution as the historical breakthrough that fundamentally changed the practice of science at large. Science no longer seems distinguishable from other forms of cultural practices. It has ceased to be a paradigm of universal rationality and presents itself as just one more object of study for cultural history or social anthropology. Even the most fundamental aspects of the classical image of science—proof, experimentation, data, objectivity or rationality—have turned out to be deeply historical in their nature.

Kostas Gavroglu and his colleagues have made fundamental contributions to challenge and revise the traditional views of European science and its spread (Gavroglu 2007, 2012; Patiniotis and Gavroglu 2012). They have shown, in particular, that even within Europe science did not simply "spread" from center to periphery but that globalization processes of science are premised on an active appropriation of new knowledge leading to a transformation of its cognitive and institutional structures.

These insights have opened up many new perspectives on the study of the history of science, which is actually turning more and more into a history of knowledge. It thus includes not only academic practices, but in addition also the production and reproduction of knowledge far removed from traditional academic settings, for instance, in artisanal and artistic practices or even in family and household practices. More importantly, non-Western epistemic practices are also considered without being immediately gauged against the standards of established Western science. "On their own terms" is the slogan under which Chinese science is currently being analyzed, without a constant evaluation of what it lacks in comparison to Western science (Elman 2005). Similarly, the worldwide circulation of knowledge is now considered not just as a one-sided colonial or post-colonial diffusion process, but rather, to put it in the language of Kostas Gavroglu, as an exchange of knowledge in which each side is active and in which knowledge is shaped as much by dissemination as by appropriation.

In recent years, the migration of knowledge has become an active field of research. With few exceptions, the emphasis has been placed mostly on local histories that focus on detailed studies of political and cultural contexts and emphasize the social construction of science. While this emphasis has been extremely useful in overcoming the traditional grand narratives, and also in highlighting the complexity of these processes and their dependence on specific cultural, social or epistemic contexts, it has led to a somewhat distorted and highly fragmented picture of science.

This picture does little justice to the overwhelming societal, economic and cultural significance of science in a globalized world. Rather than representing one of the major and still unexplained economic and societal forces in the modern world, science dissolves into a plethora of highly localized and contextualized activities, which are scarcely connected to each other. It has become a mark of political correctness to provincialize European science as representing just one among many, equally justified points of view within a global culture.

Such well-meaning political correctness does not enable historians and philosophers to compensate for the destruction of indigenous cultures, for the genocides, for the lack of gender equality, in short, for the immense damage and crimes committed in world history in the name of Western rationality and science. The golem of science cannot be tamed by underestimating it, let alone by overestimating our own influence as its witnesses.

But what can we do to avoid ascribing the powerful role of science in the modern world, for better or worse, to its intrinsic rationality, to the superiority of a universal scientific method, or to some kind of capitalist, technocratic conspiracy responsible for its triumphal procession as a driving force of modernization? Neither piling up ever more local studies, nor offering softened versions of the original universalist point of view will do. What is needed is a truly global perspective accounting for the universalizing role of science in today's world as well as for its ever-shaky claims to rationality on historical grounds. Such a global perspective must begin with the insight that the place of local knowledge in the global community is not just a residual niche but rather a matrix. Local knowledge constitutes the substratum of all other forms of knowledge, generating the global diversity also of scientific knowledge.

16.1.1 The History of Knowledge and Its Dimensions

The history of science can only be understood against the background of a global history of knowledge (Renn 2012). The fragmented picture suggested by current cultural studies has induced us to underestimate the extent to which the world has been connected—for a very long time—by knowledge. One might even go so far as to claim that, just as there is only one history of life on this planet, there is also only one history of knowledge.

Is there a theoretical perspective from which such a claim may be substantiated? This question leads to the second part of this essay, which deals with fundamental concepts such as knowledge and institutions and their normative dimensions. In the history of science it is not common to explicitly define such notions but I believe it is important in connecting historical studies to current discussions in the social and behavioral sciences. I will first define knowledge and then institutions, in both cases making reference in an essential way to the fundamental human capacity for symbolic thinking. I will also emphasize the crucial role of external representations, that is, of the material culture serving as the external medium for human thinking and social behavior, such as language, artifacts, art, writing or other symbolic systems (Damerow 1996).

Knowledge is conceived of here as the capacity of an individual or a group to solve problems and to mentally anticipate the corresponding actions. Knowledge arises from the reflection on material, socially constrained actions. Given the fundamental human capacity for symbolic thinking, the dissemination and transmission of knowledge relies crucially on external representations such as, for instance, symbols for counting objects. The reflection on actions involving such external representations may in turn create higher-order forms of knowledge, such as an abstract concept of number. These higher-order forms of knowledge are removed from the primary actions but in ways that are dependent on the contingent material and social nature of the external representations, for instance, on the specifics of the symbol system employed. The dissemination and transmission of knowledge takes place in the context of knowledge systems that rely on societal institutions.

Institutions, such as the family, the state, a school or an enterprise, are a means of reproducing the social relations existing within a given society and in particular the societal distribution of labor. The coordination of individual actions mediated by institutions presupposes behavioral norms and belief systems such as habits, religion, law, morality or ideology. A behavioral norm is the capability of an individual or a group to act in accordance with institutionalized cooperation. The interactions of an individual with others mediated by an institution and their representation by a collective belief system are constitutive of both an individual's identity and of his or her relation to a communal identity. Belief systems result from the reflection of institutionalized actions and implement the regulative framework of institutions in the minds of individuals. They allow individuals to interpret and control their own behavior and that of others in the framework of the societal group to which they belong, forming the basis of normative judgements and their legitimization.

What is the relation between knowledge and institutions? There are some striking similarities and differences. Institutions represent the potential of a society or a group to coordinate the actions of individuals and to thus interact with their environment. As an "action potential" they bear close relations to knowledge but there are important differences. There is no knowledge without the mental anticipation of actions, while institutions must regulate collective behavior without such direct mental anticipation of the collective actions and their consequences.

Institutions involve knowledge on various levels. They must embody and transmit knowledge in the sense of the capacity of individuals to anticipate actions that are compatible with the coordination regulated by institutions, as well as knowledge on social control and knowledge on how to resolve conflicts. Just as institutions have to rely on knowledge, knowledge has to rely on institutions. Institutions form the basis for knowledge systems, which in turn become the condition for the stability and further development of institutions. Institutions, however, do not think. Since institutions mediate collective actions, they have to rely on shared knowledge and engender distributive thinking processes.

As in the case of knowledge systems, external representations also play a key role in the functioning and development of institutions. All kinds of material aspects—persons, animals, places, artifacts, symbols or rituals—may become part of the external, material representations of an institution. They now represent a normative social order, defining a field of actions compatible with the regulations of an institution.

Institutions regulate human interactions to cope with certain regularly occurring problems such as those related to cooperation, the distribution of labor, the redistribution of resources or the resolution of societal conflicts. Such regulations externalize problem-solving capacities; they contribute to solving societal problems because the coordination of individual interactions can be partly discharged to the handling of external representations of an institution, such as following a command chain, dealing with paperwork in an administration, exchanging goods for money on the market or applying written law to a violation of norms. The external representations thus reduce the knowledge required to solve problems of collective interaction.

As in the case of knowledge, external representations also engender processes of abstraction enabling higher-order forms of societal organization in which coordinative functions of institutions are partly taken over by new forms of external representation. For example, in modern society, certain aspects of the coordination of societal interactions are governed by an abstract time represented by clocks. This process of cultural abstraction contributes to the opacity of institutions from the perspective of individuals because it decouples actions with the representations from the concrete interactions at lower levels of societal reflexivity. Regulating one's actions with the help of a clock thus becomes an efficient substitute for the direct coordination of actions among the members of a complex society.

Both in the case of knowledge and in that of social order, external representations may themselves become the objects and means of actions, giving rise to rich symbolic worlds of social and epistemic meaning with feedback on the underlying social and material practices.

16.2 Abstraction, Reflection and Normative Thinking

Let me again explain the crucial process of generating abstractions: Reflective abstractions in science, such as those giving rise to the abstract mathematical concept of number, ultimately depend on the material actions from which they originate, such as the concrete actions of counting material objects with the help of number words or number signs. This will be illustrated later with a historical example. Reflective abstraction is a constructive process in which novel cognitive structures are built up by reflecting on operations with specific external representations such as language, tallies or mathematical symbols. These external representations may in turn embody previously constructed mental structures so that a potentially infinite chain of abstractions is created.

Here I must warn against a common misunderstanding: It may appear as if this chain of abstractions gives rise to a teleologically predetermined hierarchy of steps leading from actions with concrete objects to ever higher-order mental operations. This is simply not the case. The historical development of reflective abstractions is in fact highly path-dependent, contingent as it is on a series of concrete historical experiences. The same holds more generally for cultural abstractions, including legal principles and moral norms. But societal reflexivity is somewhat different from epistemic reflexivity in that it is even more difficult to debunk its abstractions and identify the actual historical experiences that shaped them.

Normative thinking is actually often considered to be fundamentally different from scientific thinking, just as norms and facts are taken to belong to different categories. Science is assumed, at least at its core, to be value-free, while ethical norms supposedly cannot be grounded on facts. Yet, we encounter normativity in scientific thinking, even in basic principles such as in the moral value of truth or in demands for good scientific practice. And we encounter fact-dependence in ethical norms, as when new insights into the nature of human reproduction or new medical practices make it necessary to rethink ethical principles about the protection of life. The theoretical framework presented here suggests that ultimately moral and epistemic norms have the same origin, that they both result from a reflection on collective and individual human actions and experiences.

The possibilities for reflection on human actions and experiences evidently depend on the knowledge economy of a society. This knowledge economy comprises societal institutions in which knowledge is transmitted and generated. Similar to the knowledge economy, there is also a moral economy of a society. The functions of the epistemic and the moral economies are different. The knowledge economy serves to maintain, transmit and develop the cooperative action potential of a society by means of epistemic practices. The moral economy, on the other hand, serves to maintain, transmit and develop social cohesion and the possibilities for cooperation within a given set of institutions and by means of normative practices. Clearly, these functions are closely intertwined: maintaining social cohesion requires problem solving and hence knowledge, while collective problem solving presupposes cooperation and hence moral norms and practices. The knowledge-dependence of norms and the normative dimensions of knowledge are both mediated by the historical evolution of cultural abstractions. These cultural abstractions are neither universal nor merely conventions, but are ultimately based on human experience and its concrete historical representations.

At least in the history of science it has turned out to be extremely useful to analyze the precise way in which experience enters fundamental abstractions such as space and time. It has also turned out useful to analyze contradictions in systems of knowledge as a driving force of this development. For example, in 1905 Albert Einstein confronted seemingly insurmountable contradictions within classical physics. But then he realized that the classical concepts of space and time were neither given a priori, that is, prior to experience, as had been claimed by Kant, nor merely conventions, as had been claimed by Poincaré. Einstein recognized instead that these abstract concepts were actually conceptual constructs based on a limited domain of experience, as suggested by Hume. The realization that the much larger experimental horizon of the new physics of his time transcended this domain eventually helped him to formulate the relativity theory with its fundamentally new concepts of space and time (Renn 2006).

From such instances, an epistemic history of science has inspired a reconstruction of the experiences underlying the fundamental concepts and practices of science. Similarly, one might conceive of an epistemic history of normativity by studying the experiences that have shaped the fundamental precepts of normative thinking and practices.

16.3 A Brief History of Knowledge

This leads to the third part of this essay, which deals with the globalization of knowledge in history and some of its consequences. It may be possible to recognize some of the basic mechanisms of the global exchange of knowledge and its interdependence with other processes of transfer and transformation even in the earliest phases of human development. All of these processes are layered in the sense that the introduction of a new process does not lead to the eclipse of earlier processes. This historical superposition of experiences in itself necessitates a global perspective.

Typically, the outcome of a knowledge production process becomes the precondition for the stability of the level of development attained. This may be illustrated with a historical example. In the fourth millennium BCE, we see the beginning of large-scale settlements in Mesopotamia. At this time we also see, not coincidentally, the development of writing (Nissen et al. 1993; Damerow 2012; Renn 2014). The invention of writing was originally a consequence of state administration. Not only did it change the conditions of the geographical transfer and historical transmission of knowledge, but also extended the human cognitive facilities by stimulating reflection processes and the creation and articulation of previously unknown cultural abstractions. Eventually, writing was converted from a consequence into a precondition, not only for a particular model of state organization but also for a level of socioeconomic development, from literature and law to science, that depended on these novel cultural abstractions. The example of the invention of writing thus nicely illustrates how more or less contingent consequences of historical processes may turn into the necessary precondition for the stability of the current situation as well as for its further development.

It has often been claimed that since its inception writing has been used as a means of representing language. But in fact it emerged, independently of spoken language, as a technology for the administration of centralized politico-economic systems of the ancient Mesopotamian city-states where its communicative function was restricted to the administrative context. Thus, the first writing did not represent the meaning of words or sentences, nor did it reflect grammatical structures of language, but rather meanings related to specific mental models of societal practices such as accounting. Since it was not used as a universal means of communication, it could only transport a very precise meaning in a very precise context. It was on this basis that a long-term and stable Babylonian administrative economy developed, which in turn served as a precondition for further development, in particular, for the second invention of writing, this time as a universal means of codifying language. This second invention of writing would have been impossible without the spread and manifold use of the earlier proto-writing.

As the historian of science Peter Damerow pointed out, a similar development precedes the emergence of mathematics, which also emerged from context-dependent Babylonian administrative proto-writing, originally invented to solve specific local administrative problems (Damerow 2012). This example illustrates the process of reflective abstraction introduced earlier. For many years, not even historians of mathematics imagined that there were numbers whose meaning depended entirely on the context of what they were supposed to count. In other words, the meaning of the respective symbols depended on whether they were counting people, length, field measurements or pints of beer, the latter being an important application of Babylonian mathematics. And yet our present day mathematics, which claims universal validity, emerged from a system of symbols that were originally invented exclusively to solve specific administrative problems and characterized by this very context dependency.

Contrary to what philosophers have long believed, the universality of mathematical knowledge is thus not the characteristic feature of a specific type of knowledge. It was rather the outcome of a specific historical trajectory of globalization. Since the third millennium BCE, the idea of writing probably spread from Mesopotamia throughout the world, although it cannot be excluded that there may have been independent inventions of writing as well. But it does appear that writing spread almost immediately to Iran and Syria, then a thousand years later to the Indus civilization, and another thousand years later to China. This spread led to an enormous increase in the possibilities for transmitting knowledge and also for the emergence of science.

The initial emergence of science in a form familiar to us took place in different parts of the ancient world: Greek and Chinese science developed independently of each other around the middle of the first millennium BCE. The onset of Greek science is to be found in the Middle East, not far from the cultural centers of Mesopotamia. The point that I want to emphasize here is the emergence of cultural abstractions by cultural transfer. As a consequence of the transfer of Babylonian knowledge on medicine, astronomy and mathematics to a different cultural area, that knowledge itself took on another form. In particular, the justification for the validity of a claim was made explicit in the Greek context, while in the Babylonian context it remained part of implicit knowledge. Babylonian science does not comprise explicit scientific proofs so that its knowledge appears to us as an unfounded collection of instructions (Schiefsky 2012).

This knowledge was in fact not unfounded. It was just that the normative control of knowledge operated in a different way. Since knowledge was embedded in the age-old institutional and practical contexts of Babylonian culture, there was simply no motivation to make the reasoning behind certain claims explicit. This changed as soon as another culture appropriated such knowledge, especially when that culture, as is the case for Greek culture, was geared to a public discussion of political decisions and their justification. While the justification of Babylonian or Egyptian scientific knowledge was largely inherent in the institutional and representational structures in which it was generated, it became the subject of explicit normative reasoning in the Greek context.

The process just described was a process of cultural interaction in which knowledge accumulated over thousands of years in the cultures of the Middle East eventually changed its form as a consequence of being transferred to a new context. This is a striking example of the important role of cultural breaks and intercultural appropriation for innovations due to the recontextualization they engender. In contrast to the transition from Babylonian to Greek science, in China there was, at that time, no comparable transmission across a cultural break connected with a complete recontextualization of knowledge. In Chinese as well as in Babylonian traditions, the structures of scientific reasoning therefore remained, at least from our perspective, largely implicit. Thus ancient Chinese mathematics has also seemed to some of its Western interpreters to represent a mere collection of instructions, devoid of explicit scientific reasoning.

Processes of cultural abstraction by recontextualization are not just characteristic of science but have also shaped the traditions of normative thinking, as can be inferred from the history of religion. For instance, the Babylonian exile of the Jews in the sixth century BCE and their later encounters with Persian and Hellenistic traditions not only led to an integration of new cultural resources into the Jewish tradition, but also to a transformation of this tradition towards greater inclusiveness and universality (Geller 2014). This can be illustrated by the biblical account of the prophet Jonah charged by God to preach in the Assyrian city Niniveh, announcing its imminent destruction. Jonah tries to escape the divine mission but is ultimately confronted with the fact that the God of Israel embraces ignorant enemies in His grace. Jonah ends the Book abruptly with God's rhetorical question:

> And should not I spare Nineveh, that great city, wherein are more than six-score thousand persons that cannot discern between their right hand and their left hand; and also much cattle? (Jonah 4:11)

Similarly, the emergence of Buddhism at about the same time in India occurred in the context of a reaction to the contemporary Brahmanical religion and led to a highly reflective textual tradition (Braarvig 2012). Buddhism carried with it packages of knowledge comprising texts, artisanal and artistic practices, but also forms of social organization such as monastic communities that travelled across Eurasia.

Religions such as Judaism, Buddhism and later Christianity and Islam provided to be efficient networks for spreading both knowledge and normative thinking. These world religions embodied much of the structures of authority and the mechanisms for knowledge production and dissemination of the state. But whereas knowledge in the state was limited by its geographic boundaries, the packages of knowledge associated with world religions traveled more or less freely across state boundaries. Religion offered a new social order greater than that of the state, but modeled on the state; thus, for instance, the concept of the Umma in Islam and the City of God in Christianity (Damerow and Renn 2010).

While authority was merely asserted by the state (and grounded in physical force), the world religions needed to justify their authority. Thus they developed sophisticated schemes of justification and produced extensive bodies of knowledge through complex processes of dialectics. Some of these schemes and processes had their origins in earlier systems of thought that had arisen under specific local conditions, such as Hellenistic philosophy. But whereas such schemes and processes had been local, the world religions embedded them in institutions of potentially global extent. It is against the background of these complex schemes of argument, processes of justification and elaborate bodies of knowledge—and in dialogue with them—that modern science was born, as will now be discussed.

The capacity of religion to challenge the authority of the state in terms of its own internal logic ultimately increased the potential of science to challenge religious authority. This is especially true for a religious tradition like medieval and early modern Christianity that systematically committed itself to the augmentation of knowledge, positioning itself within a comprehensive worldview.

In the context of the late medieval and early modern development of extensive commercial networks, of new military technologies, of large-scale engineering endeavors such as the Arsenal of Venice, and of large building projects like the cathedral of Florence, a new class of scientist-engineers such as Brunelleschi, Leonardo and Galileo faced important technological challenges. Addressing these challenges, they relied on theoretical knowledge from antiquity, the Islamicate world and from medieval scholastics, which they combined with contemporary practical knowledge, thus creating a new form of science in which theoretical knowledge was systematically related to experience.

In response to the encompassing religious worldview, the new knowledge accumulated by these scientist-engineers began to assume the character of an equally all-embracing interpretation of the world, as can be found in the great philosophical concepts of the early-modern period, for instance, in the works of Giordano Bruno or René Descartes. Science eventually became a kind of counter ideology by which the emerging bourgeoisie could defend its claims to power, not according to a transcendent, religious order, but according to immanent laws of nature and society. The new knowledge thus also assumed a normative dimension.

This situation helps to explain why, in the sixteenth century, the reform of astronomy by Copernicus, placing the Sun rather than the Earth at the center of the universe, could have had such far-reaching ideological consequences: it occurred within a context of a socially dominant system of knowledge that claimed to be universal and exclusive (Omodeo 2014). The geocentric worldview, placing the Earth at the center of the universe, was deeply anchored within this system of knowledge. Questioning this claim, even with good scientific reasons and without any intent of heretic provocation, still amounted to unhinging the whole system and thus causing an ideological revolution by means of an astronomical, and at the outset purely scientific innovation. In contrast, there was no comparable revolution in seventeenth-century China when Jesuit missionaries introduced Copernican theory, or even Galileo's telescope, which made the new view of the heavens so intuitively plausible. In Ming China, there was simply no combined religious and philosophical worldview that this new discovery could potentially provoke (Schemmel 2012).

In the early modern period, all the patterns of the globalization of science had essentially already formed within the European network of scientific knowledge. It was crucially shaped by Europe's dense but culturally diverse urban landscape. The successful expansion of science within Europe could therefore create a model essentially followed by all later globalization processes of science, including the replication of institutional settings and canons of knowledge. The thus emerging network of scientific knowledge exhibited self-organizing behavior, as is evident in the fact that there was no central control of scientific practice, and yet scientific knowledge accumulated at an astonishing rate and traveled quickly across the emerging scientific community. Positive network externalities fostered the inherent dynamics of spreading science so that the more people engaged in it, the more useful it became. Science developed into a self-organizing network that inherently scales globally (Renn 2012, Chap. 24).

The globalization of knowledge today is a consequence of two processes: the intrinsic globalization of science just described and the fundamental role that knowledge, particularly scientific knowledge, has assumed in other, economic, political and cultural globalization processes. One important result of the interaction between intrinsic and extrinsic processes of the globalization of knowledge is the emergence of global objects of science, in particular global human challenges such as climate change, scarcity of water, global food provision, reliable energy supply, sustainable demographic development and nuclear proliferation.

The production of scientific knowledge in large-scale technological ventures, in global infrastructures and regulations, or in worldwide operating enterprises has given rise to socio-epistemic complexes involving new epistemic communities. These socio-epistemic complexes such as the global energy or traffic systems cause changes on a global scale that cannot be easily undone. Governance of such socio-epistemic complexes requires the production of more and more scientific knowledge which becomes ever more inseparable from the development of policies relying on social and economic knowledge and its normative reflection. Such socio-epistemic complexes may even endanger their ecological and social substrata—unless new scientific knowledge continually becomes available. In consequence, they sharpen the dilemma of human freedom, enhancing humanity's potential to act but making the world increasingly dependent on the appropriate use of this potential.

It thus becomes clear that the much-discussed globalization processes of the present involve knowledge not just as a mere presupposition or consequence of economic or political processes. It is in fact the globalization of knowledge as a historical process with its own dynamics that orchestrates the interaction of all the underlying layers of globalization. The globalization of knowledge and its normative reflection profoundly influence all other globalization processes—including the formation of markets—by shaping the identity of its actors as well as of its critics.

It is important, however, not only to investigate the globalization of knowledge and its normative dimensions, but also to pay due attention to its counterpart, the localization of knowledge and norms in local processes of appropriation, as Kostas Gavroglu and his colleagues have emphasized, in particular in their research initiative on "Science and Technology at the European Periphery (STEP)". Referring such an analysis to the present we may perhaps regain autonomy with regard to the economic dimension dominating our current perception of these processes. An investigation of this kind may explain the sense in which the globalization of knowledge and its encounters with local knowledge has become a critical dimension of today's globalization processes on which their future development depends.

From this perspective, they may turn either in the direction of further subjecting the economy of knowledge to the control of other globalization processes, or in the direction of strengthening the autonomy of knowledge and its normative reflection, and thus also our potential for steering such processes.

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Chapter 17
The Global and the Local in the Study of the Humanities

Rivka Feldhay

Abstract This chapter focuses on some tensions—inherent to the humanities as a field of studies—between an epistemic commitment to truth, an ethical and political commitment to reflexivity and critique, and the quest of the arts and sciences for institutional autonomy. In the first part I delineate a quick genealogy of the problem of the humanities in three stations: the *Studia Humanitatis* of the fourteenth to fifteenth centuries; Kant's ideas of the freedom of philosophy; and Humboldt's conceptualization of the position of the university vis-à-vis the state and the nation. In the second part I present the migration of the tradition of *Geisteswissenschaften* to Palestine and its transformation into *Madaei Haruah* at the Hebrew University. I conclude with a few words about the present and future of the humanities in Israel.

Keywords The humanities [Madaei Haruach] • Humanism • Autonomy • Spheres of culture • State and education

In 1997, Yzhar Smolensky (known by his pen name S. Yzhar, 1916–2006), a prominent and prolific Hebrew author, and a Knesset member and Professor of Education, published a short treatise on the nature of "humanistic knowledge" and its evolution in modernity. Jealous of the prestige conferred on "Science" in the modern world, the human spirit has entered the gates of the University. There it transformed humanistic knowledge into quasi-scientific disciplines: *Madaei Haruach*, a literal translation of the German term *Geisteswissenschaften* into Hebrew. Yzhar thought that through this act humanistic knowledge gave up its essence, its freedom, and its wisdom, producing instead a stillborn, something dead, devoid of spirit.

Yzhar identified the origins of the disease with a particular historical moment in which the "spirit" came to the university. In his poetic, blunt way he threw a big challenge at us, the heirs and carriers of the "idea" and "mission" of *Madaei Haruach* in today's universities. In my paper I intend to meet his challenge by following and perhaps even radicalizing his thoughts about *Madaei Haruach*.

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Looking backwards to the pre-history and forward to the possible post-history of the humanities—those fields of knowledge concerned with human beings and society—I call your attention to the problematic core at their center. This problematic core, "the wound of the humanities," revolves around questions of epistemic status and critical thinking, as well as around the space of freedom necessary to produce such knowledge. Our task is to reflect upon this "wound," the origin of humanistic fields of knowledge and their predicament. While recognizing it (with Yzhar), I hope we shall be better equipped to guard both our "freedom" and our "wisdom" against his pessimistic prophecy.

My essay consists of two parts:

1. First, I shall follow a few historical attempts to claim epistemic status to knowledge about the contingencies of human history and to find for it a secure place in society. In particular, I dwell on the *studia humanitatis* of the fourteenth and fifteenth centuries; on Kant's ideas of freedom and autonomy for the faculty of arts at the University of Koenigsberg in the eighteenth century; and very briefly on Humboldt's idea of the university in between the state and the nation.
2. Second, I speak briefly on the migration of the tradition of *Geisteswissenschaften* to Palestine and its transformation into *Madaei Haruach* at the Hebrew University.

In conclusion, I say a few words about the problematics of the humanities in the present and their possible future.

17.1 The Birth of Studia Humanitatis

The term *studia humanitatis*—not its content, nor its methods, but rather the concept—was born in the fourteenth century, giving form to the criticism of contemporary philosophy upheld by a group of scholars, the best known among them Petrarch, Boccaio, Leonardo Bruni, Collucio Salutati, and Lorenzo Valla. Their common linguistic sensibilities formed a basis for their criticism of the grammatical culture of the scholastics. They claimed that the philosophical jargon developed by the scholastics was an inner kind of discourse, understood only within a closed community. In fact, it was a symptom of their ultimately irrelevant involvement with abstract, incomprehensible concepts such as "the potential and the actual," or "essential forms." Here is a short quotation:

> Who does not laugh at the insignificant little conclusions in which these highly educated people fatigue themselves and others? They waste their whole lives in such conclusions since they are not good for anything else…[1]

The professionals called in Italian *humanista*—teachers, secretaries of states, lawyers—believed that there was no connection between the concerns of scholastic philosophers and the real interest of their fellow citizens at the Italian city states.

[1] Petrarch (1948).

Ordinary people and nonintellectual believers were preoccupied by concrete problems such as richness in Christian society, faction strife, or religious sentiments that were crucial for their lives in the Commune, in the courts of princes, or in those of bishops and popes.[2] In contradistinction to traditional philosophy institutionalized in medieval universities, whose main focus was natural philosophy, metaphysics, and theology, the Humanists elevated the disciplines of the *trivium*—grammar, dialectic logic, and rhetoric—to their main fields of study.[3] These traditional disciplines had blossomed in the Greek and Latin worlds, but were considered only prepaedeutic, serving as preliminary instruction in preparation for studying philosophy at the medieval university later on. The humanists developed the fields of the trivium into professional disciplines in their own right. They delved into philology and grammatical criticism; they were interested in the restoration and retranslation of historical and literary texts, and in reviving alternative philosophical traditions of the ancient world such as stoicism and neo-Platonism. The accumulation of ancient, forgotten linguistic, literary, and historical knowledge provided the basis for producing models for writing and discourse on the problems of the day in the fields of ethics, religion, and politics.[4] Moreover, the concern with philology, literature, and history in itself gave rise to alternative philosophical insights—a kind of "rhetoric turn" that brought into question the atemporal, universal categories upheld by the Aristotelian philosophical tradition.[5] The gist of this turn can be sensed in a short statement found in Lorenzo Valla's "Dialectics." Valla claimed that the usage of man is the creator of words. This short statement embodies Valla's attempt to invent new tools for justifying and legitimizing the insight that the communication through language is in fact a social act; a tool for the formation of man and society. At the same time it exposed the fragility and tension in the attempt to gain true knowledge of God and human society by means of language that is itself a fruit of particular and changing circumstances.[6]

To avoid misunderstandings, I would like to stress one point: I do not claim that the humanists, those who transformed the *studia humanitatis* into the core of a new educational agenda, attempted to provide an alternative to philosophy. Humanist studies developed outside the universities, beyond the boundaries of the accepted academic hierarchies, in academies[7] which sometimes functioned as noninstitutionalized and informal social circles as well as in the courts of princes and bishops. One could say they existed as a form of alternative culture, at least in their primary stages of development. Moreover, it would be wrong to ignore the fact that many of these humanists studied in the universities and were most familiar with scholastic philosophy and with traditional law. Parallel to the humanistic studies, then, the

[2] Garin (1961a, b, 1965).

[3] Kristeller (1961).

[4] Rabil (1988).

[5] Camporele (1972), Struever (1970).

[6] Waswo (1987), Kahn (1985).

[7] Maylender (1926–193), Field (1988), Hankins (1991).

scholarly tradition of the universities thrived, and upheld a critical dialogue with the humanists, a dialogue that led the scholastics themselves to emphasize the need to return to the Greek and Latin origins of philosophy.[8] At the end, philosophy went into a golden era in the sixteenth century, albeit under political conditions much different from those of medieval times. However, in contrast to the scholastics, the humanists did not succeed in institutionalizing themselves, and the *studia humanitatis* remained in essence a profession of learned men, oftentimes traveling from place to place, who were hired to work as teachers, educators of the nobility, secretaries of independent cities (chancellors), or at the courts of princes, bishops, and popes. Some of them lived in republics, and played key roles in their political life. Others ardently defended the princes and clergy in whose courts they found refuge. For all these reasons, it is impossible to find a common theme underlying their distinct philosophies or their political positions. Nevertheless, one can distinguish one innovative insight that prevails in all the works of the humanists that has to do with the connection between knowledge and the social world. For them, the only relevant knowledge had to be anchored in the praxis of concrete human life.

The aspiration of humanists for true, practical, and relevant knowledge of contingent things undermined traditional epistemic canons. Traditionally, it was exclusively to the universal, atemporal objects of mathematics or the natural world that the epistemic status of true knowledge was granted. In contradistinction, the humanists, challenging the medieval dichotomy between the *vita activa* and the *vita contemplativa*, aspired for their discourse to be both theoretical and practical, both epistemically valid and socially relevant, thus a discourse permeated with intellectual tension. In addition, reflection on the relationship between the humanists' intellectual insights and their social existence further exposes the "wound of the humanities" that has surfaced again and again in their later history. On the one hand, the new vision of history as humanly determined, dependent more on human action and choices and less on nature and God's providence, ensured critique of social hierarchies and intellectual authorities. It was this critique that established the principle of freedom of self-formation that the humanists demanded for themselves. Their involvement in politics, a consequence of their basic intellectual assumptions on the contingency and historicity of social life, embodied their message. On the other hand, the social circumstances under which they acted—their failure to anchor themselves in some sort of social institution—imposed severe limitations on their ability to realize this freedom. Thus, they invented the "republic of letters"[9], an imagined, utopian space supposedly independent from social constraints and traditional authorities, a republic in which, as it were, the humanistic principles could be fulfilled. However, the idea of the republic of letters undermined the very principles of humanistic discourse in that it detached its participants from the praxis of social and political life—precisely the context that gave meaning to their work as humanists.

[8] Schmitt (1984), Kraye and Stone (eds.) (2000), Edelheit (2014).

[9] Yoran (2010).

17.2 Immanuel Kant and His Notions of Autonomy and Freedom

In some sense, Kant's conceptualization of freedom and autonomy can be seen (by us) as an attempt to solve the paradoxes that emerged in the life and thinking of the earlier humanists, even though his point of departure as an established philosopher was completely different. Implicitly, the "republic of letters" was the way the humanists chose to impose upon themselves some boundaries to safeguard their liberty to criticize philosophy of the schools or the politics of pope, princes, and bishops. As we shall see in a moment, Kant perfected the art of projecting boundaries into an explicit strategy that grew out of his philosophical tenets but had deep institutional and political implications as well.

Kant's enterprise was rooted in the common creed of the Enlightenment "Philosophes" who sought to raise human reason to the level of a principle from which Kant derived his notions of objective science and universal ethics. For Kant, the essence of reason was its ability to recognize its own limits and discard pretensions unfounded by reason.[10] Thus, reasonable man commits himself to the law he legislates for himself (literally, the auto-nomos). This is the meaning of the notion of autonomy, and Kant identified the acceptance of a self-imposed law with being moral.[11] The ability of reason to set limits is then translated by Kant to the projection of boundaries between spheres of life as well as between disciplines. Kant claimed that although being part of nature, by putting limits to himself man is able to liberate a space in which the laws of nature are "suspended" and where man is dominated by the law of his own making. The sphere of nature is thus emptied of moral significance while the sphere of morality is emptied of the dictates of the laws of nature. In this sense, the human who follows the "categorical imperative" exists in a sphere of freedom.

The social and institutional meanings of Kant's philosophy of freedom and autonomy transpire in two especially popular texts: *What is Enlightenment?* and *The Conflict of the Faculties*. While binding himself to his own self-imposed law, the subject is required to respect the principle underlying the modern idea of sovereignty—the self-imposed limit to his own freedom: this applies to university teachers, as it does to all other citizens. Sovereign rulers do not tolerate the division of their own power and authority, and hence, the Sovereign ruler transformed the medieval corporations of students and teachers—universities—into state

[10] I. Kant, *Basic Writings of Kant*, ed. By A. W. Wood, New York 2001; See for example, Preface to the Critique of Pure Reason, where Kant speaks about an [inner] "court of appeal which should protect the just rights of reason, but dismiss all groundless claims", p. 5; *and* Kant, *Groundwork for the Metaphysics of Morals*, ed. and trans. By A. W. Wood, New Haven: Yale University Press 2002, Preface, p. 4: "All trades, handicrafts, and arts have gained through the division of labor, since, namely, one person does not do everything, but rather each **limits himself** to a certain labor... (My emphasis, R.F.).

[11] Ibid. see, for example pp. 49–50.

institutions, endangering their traditional privileges and curtailing the specific power of the intellectuals in the Middle Ages. As grounds for the regulation of the rights and duties of scholars in state universities, Kant applied the "art of separating and dividing" and suggested a division of labor also between the faculties:

> Whoever it was that first hit on the notion of a university and proposed that a public institution of this kind be established, it was not a bad idea to handle the entire content of learning . . . by *mass production*, so to speak – by a division of labor, so that for every branch of the sciences there would be a public teacher or *professor* appointed as its trustee, and all these together would form a kind of learned community called a *university* (or higher school). **The university would have a certain autonomy (since only scholars can pass judgment on scholars as such) [. . .].**[12] (My emphasis. R.F.)

In what follows, Kant further explains the conditions of possibility needed for this form of autonomy, that is, its boundaries:

> The faculties are traditionally divided into two ranks: *three higher* faculties [by these he means the faculty of theology; medicine and law. R.F.]; and *one lower* faculty [namely the traditional faculty of the arts and sciences called philosophy: R.F.]. It is clear that this division [namely the ranking between high and low] is made and this nomenclature adopted with reference to the government rather than the learned professions; for a faculty is considered higher only if its teachings – both as to their content and the way they are expounded to the public – interest the government itself, while the faculty whose function is only to look after the interests of science is called lower because it may use its own judgment about what it teaches. Now the government is interested primarily in means for securing the strongest and most lasting influence on the people, and the subjects which the higher faculties teach are just such means. Accordingly, the government reserves the right itself to *sanction* the teachings of the higher faculties, but those of the lower faculty it leaves up to the scholars' reason.[13]

According to Kant, the academics of the higher, professional faculties, that is, theology, law, and medicine, must agree to comply with the dictation of their subject matter by the state, as well as with the principle that prevents them from openly criticizing the authorities. In other words, the limits of the autonomy are upheld by the high faculties according to the state authorities' definition. This condition results from the fact that the knowledge with which they are concerned is relevant to matters of state. But alongside these, claims Kant, there must be one place that is free to determine its own content, albeit while using reason, namely, putting limits to itself. This place must be safeguarded for the sake of the pursuit of truth and to publicly and openly criticize the authorities:

> It is absolutely essential that the learned community at the university also contain a faculty that is independent of the government's command with regard to its teachings; one that, having no commands to give, is free to evaluate everything, and concerns itself with the interests of the sciences, that is, with truth: one in which reason is authorized to speak out publicly. For without a faculty of this kind, the truth would not come to light (and this would be to the government's own detriment) [. . .].[14]

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[12] Kant (1979).

[13] Ibid. p. 25, "General Division of the Faculties".

[14] Ibid. pp. 27;29.

In this way, the fundamental Kantian idea of autonomy as freedom within boundaries that the subject determines for himself or herself was applied to the institutional sphere, in which the "idea of the university" crystallized. On the conceptual level, autonomy is freedom at the price of an "internal dissection"—the duty to obey, imposed by the subject onto himself. This idea is articulated in Kant's answer to the accusation of Friedrich the Great that he had been disobedient in writing about the conflict of the faculties. In response, Kant answered: "I shall not fail to put before Your Majesty proof of my most submissive obedience."[15] On the institutional level, "academic freedom" depended on the acceptance of the boundaries dictated by the state to the *lower* faculty. Hence, for Kant academic freedom was, in a sense, a privilege of the weak.

Kant held a chair at the Prussian state university in Koenigsberg of Friedrich the Great, an Enlightenment man, of whom Kant wrote that he "allowed every man to use his own personal reason in matters of conscience." Nevertheless, Kant's work and his status exhibited similar tensions that we have detected in the discourse of the Renaissance *humanista*: First, the inner, philosophical tension between reason/universal and the experience/particular in his theoretical, practical, and aesthetic discourses (which I have not discussed here), but second, the inner conflict between the need to maintain a critical voice in matters of ethics, politics, religion, and society on the one hand and the need for a secure place from which to exercise such critique on the other. As we have seen, Kant's solution to such conflicts always stemmed from his art of separation and the setting of boundaries.

17.3 Humboldt's Idea of a University

Many historians tend to identify the idea of the modern university with the University in Berlin, founded in 1810. Such identification constitutes, in a nutshell, the "myth of the modern university." From the aspect of the relationship between academics, particularly those of the humanities—the modern state and the needs of society, or the nation—Humboldt's ideas represent one more attempt to resolve the paradoxes traditionally involved in structuring a relationship of academics claiming freedom and autonomy to educate the people and criticize rulers, while at the same time being dependent on the establishment to provide them with resources and protection.

In 1809–1810 Wilhelm von Humboldt (1767–1835) served for 16 months as the head of the section of religion and education in the Prussian Ministry of the Interior. After having resigned, he remained the chairman of the founding committee of Berlin University. Thus, he definitely had his impact on the idea of the university in modernity. Humboldt's romantic-humanistic beliefs about — Bildung the cultivation of individuality through culture and education, his Kantian presuppositions

[15] Ibid. Preface, p. 13.

about the necessity of division of labor between different cultural spheres, and his experiences with the state administration crystallized into an idealized view of the dynamics of academics–state–society relationships in modernity. His text on *The Sphere and Duties of Government*, written in 1791–1792 but published only posthumously, indicates a conception of the normative relationship worthy of implementation between a sovereign state and a sovereign, intellectually developed individual in a progressive stage of civilization: "Whence I conclude," he writes, "that the freest development of human nature, directed as little as possible to ulterior civil relations, should always be regarded as paramount in importance with respect to the culture of man in society. He who has been thus freely developed should then attach himself to the State; and the State should test and compare itself, as it were, in him."[16]

Humboldt thought about the individual–state relationship in terms of an analogy, a mirror image, between their respective sovereign positions. Both state and individuals are conceived in Kantian terms as sovereign, reasonable, and autonomous subjects: this meant that each is believed to hold a set of rights and duties vis-à-vis the other. Much as "freely developed" individuals are expected to limit their egotistic desires vis-à-vis the state, voluntarily attaching themselves to it, so is the state expected to restrain itself, holding some rights and duties vis-à-vis such individuals. Humboldt believed that the high level of civilization achieved in modernity justifies such arrangements:

> . . .men have now arrived at a far higher pitch of civilization, beyond which it seems they cannot aspire to still loftier heights save through the development of individuals.[17]

According to this conception, the "nation" is a harmonious community of civilized individuals that recognize their limits without the state explicitly imposing it on them either by force or by a state-organized system of education. The state, on its own part, refrains from imposing anything on the cultural sphere, assuming that its goals would be achieved in a harmonious way if it sets limits upon itself and creates the space for individuals to shape themselves by the means they see fit for themselves:

> In fine, if education is only to develop man's faculties, without regard to any definite civil forms to be collaterally imparted to his nature, there is no need of the State's interference. Among men who are really free, every form of industry becomes more rapidly improved, – all the arts flourish more gracefully, – all sciences become more largely enriched and expanded.[18]

At this stage, Humboldt thought that it was the nation, rather than the state, that should take the financial responsibility for the system of education, hoping to

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[16] The Sphere and Duties of Government. Translated from the German of Baron Wilhelm von Humboldt, by Joseph Coulthard, Jun. (London: John Chapman, 1854). In Online Library of Liberty : http://oll.libertyfund.org/title/589, p. 41.

[17] Ibid. p. 40.

[18] Ibid. p. 42.

loosen the grip of pre-modern absolute monarchies on it. Thus, in a letter to his colleague and partner to the educational reforms in Prussia he wrote: “Education is a matter for the nation and we are preparing (admittedly with great caution) to diminish the powers of the State and win the nation over to our own interests,”[19] an idea repeated in a letter to his wife from March 1809. His plan was to arrange “for schools to be paid for by the nation alone.” Nevertheless, later on, he changed his ideas about the need of the state to finance universities, without modifying, however, the basic structure of autonomy and independence to scholars and researchers which he had presupposed in his early writings on education. In fact, around 1810 he demanded that the University should be endowed with land property, hoping to ensure its independence. This was also one of the reasons for his resignation from the Ministry.

The logic underlying the Humboldtian idea of the university, however, was later adopted by many modern democratic states. The rudiments of the norm that has guided most modern Western states in shaping their relationship to academia is anchored in the insight that there are three forces that constitute the dynamic cultural-political field: the State, which is the main source that provides for the needs of universities and to some extent also supervises them; professors and students, who are the guardians of cultural traditions and producers of new knowledge; and “society” at large. The well-being of society depends on the delicate balance between the power of the State, the carriers of knowledge and the bearers of traditions and legacies, but also of critical thought, and the public, whose needs both state and academia should cater to, but also restrain. Historian Fritz K. Ringer lucidly sums up the situation of the modern state in the Prussian monarchy of Friedrich Wilhelm II: “The State was to support this great objective [pure learning. . .to be cultivated for its own sake] without trying to exercise direct control over the materials learned and taught [. . .]. In the long run, the state and society would surely benefit from the spiritual and moral influence of the new learning.”[20]

One example for the translation of Enlightenment norms into legal arrangements between academia and society in the twentieth century is the law of higher education, legislated in the Israeli Parliament in 1958. The law is an attempt to regulate the relations, in democratic regimes, between states, academics, and societies. No doubt it still echoes humanistic, Kantian, and Humboldtian notions. The law stipulates that “a recognized institution is free to manage its own academic and administrative affairs as it sees fit, within the framework of its budget.” This law was preceded by years of heated disputes in the Knesset (the Israeli parliament) and repeated rejections of bills. The main bone of contention focused on the relative weight of two central and contradicting interests perceived by contemporaries: the “reign of academic freedom,” in the words of the Minister of Education of the time, Ben-Zion Dinburg (who later changed his name to Dinur); and the desire to recruit the academia and its research programs for the needs of the young, recently

[19] von Freese, (comp.) (1953).

[20] Ringer (1969).

established state. “Academic freedom,” wrote Dinburg, “[. . .] has taught [our] generation to stand opposite reality, and to make the effort, independently and courageously, to observe it, to research it and to lift its veils of mystery—disregarding prevalent opinions and various prejudices.”[21] Dinburg thus presupposed the idea that the autonomous faculties of reason and personal conscience are the undisputed foundations of academic activity. On the other hand, Dinburg strove to recruit science for the needs of the state. Statements in this vein were made by Knesset members, who referred to “the unity between science and work in the colonizing-pioneering existence of the state during its first decades.”[22]

The law passed is institutionally oriented. It refers to the freedom of academic institutions to manage their affairs (“as they see fit”) within the constraints of their budget. In Israel the state is a major source of finance for the universities. The budget, however, is allocated by a committee of the Council of Higher Education, which consists mainly of prestigious academics from recognized institutions, both universities and colleges. In the past, members of the council were chosen according to the recommendation of their home institution. This is no longer the case; members are now directly chosen by the head of the council, the Minister of Education. Thus, although the Council was originally intended to create a buffer between the government and academia, the measure of its independence now depends directly on the aspirations of the administration in power, and on the minister of education who nominates the members of the council. The language of the law seems to allow for an exceptionally broad interpretation of the freedom of academic institutions to do “as they see fit,” and yet, the freedom of individual professors and students to study and teach without interference (*Lehrfreiheit* and *Lernfreihit*) is not mentioned. Such freedoms could have been defended by a constitution, if Israel had one; but it does not. Thus, the concern of a woman Knesset member in the fifties—Shoshana Persitz—to defend the right of students and professors to choose their ideology and express it without any kind of pressure from politicians and state organs has been left to the public to discuss, and to academicians and politicians to practice.

My genealogical excursion into the “deep history” of the humanities has traced two foci of tensions inherent in their practice. The first concerns the claim for real, universal knowledge of contingent, context-bounded, and language-dependent objects. The epistemic status, validity, or even relevance of such knowledge has been repeatedly questioned. Such critique was expressed by scholastic philosophers against humanists in the fifteenth century as well as by natural scientists against historians and literary scholars in the twentieth century. A second focus of tension around the field of the humanities is related to the space of freedom necessary for research, articulation, and teaching of knowledge about worldly and otherworldly human and societal topics whose immediate utility is unclear or controversial. The cases of the fifteenth-century humanists, of eighteenth-century philosophers in

[21] Vollanksy (2005).

[22] Ibid., p. 41.

Absolutist state universities or in the "Humboldtian" university, show that a space of freedom may be either gained and/or lost by institutionalization or the lack thereof. I have chosen to name such loci of tension, which are repeatedly emerging in the Western tradition of education, "the wound of the humanities," to distinguish my argument from current "lamenti" over the fate of the humanities. Still, the need is also there to avoid an illusion of universality in the way the problem is manifested. In the last part of my chapter, then, I focus my gaze on the way the problematics of the humanities transpires in the historical work of the "founder" of the faculty of the humanities at the Hebrew University, Professor Richard Koebner (1885–1958). My exploration of Koebner's historical oevre is just one example, by no means a general argument, about how what I see as rather universal problems of the humanities have been localized in the particular case of the humanities in Israel.

17.4 Madaei Haruach at the Hebrew University

Koebner arrived to Palestine in 1934 after 25 years of teaching at the University of Breslau, Germany. Perusing his work one may get some sense of the new transformations that the old tensions assumed in the field of historical research before and after the establishment of the State of Israel. Two aspects of those tensions will be particularly pointed out: the first has to do with Koebner's concern with the problem of facts (particulars) and concepts (universal) in the work of the historian, and the second concerns the involvement of historians and their knowledge in the public and political life of their time.

"Getting the facts right" was obviously the basic task of the historian in Koebner's eyes, "facts" meaning historical constructs that provide precise, empirically based evidence for the representation of the historical past. Some vignettes of his work point out how he thought of the historian gaining a specialized, professional "historical consciousness" through combining investigation of facts and knowledge of "historical reality" [his words] through concepts. We shall see, later on, how he privileged this kind of knowledge through applying his own art of separation and boundary making.

The first vignette comes from Koebner's review[23] of Joseph Schumpeter's (by then a well-known Harvard economist) *Imperialism and Social Class*. The book purported to analyze imperialism, testing its theory against historical experience. Koebner wrote that "abstract arguing dominates the scene and presentation of facts is very thin." Instead of facts, Koebner claimed, Schumpeter used a "selective arrangement of historical illustrations."[24] Thus, Koebner created a kind of dichotomy between "mere illustrations" and a substantial basis in facts. However, Koebner's clear perception that facts are not simply "given" for the historian

[23] Koebner (1952).

[24] Ibid., p. 405.

becomes evident upon reading the sentences opening his studies of concepts, among them: “Despot and Despotism: Vicissitudes of a Political Term”[25]: “The semantics of politics” he writes “offer many instances of a momentous connection between the vicissitudes of vocabulary and the fates of states and societies.”[26] Following the short introduction the author dives into a long and detailed pursuit of the transformations of the Greek word *despot* from the discourse on the economia in the Greek city states and up to Voltaire in the eighteenth century, passing through Greek, Roman, and medieval commentaries and the emergence of the concept of sovereignty from Bodin through Hobbes, to Grotius and Puffendorf. In each of these cases Koebner showed that a word, a term, a concept acquires a major role in shaping political realities when some specific historical conditions become relevant for its use. Thus, he pointed out the interdependence, or coproduction, of political realities and words, terms and concepts used in their contexts. The historian becomes aware of historical realities through his investigations into the words used and transformed by historical agents, of which he makes sense by using his own conceptual apparatus.

Another vignette concerns Koebner’s subtle critique, dating from the 1930s, of the big “stars” of cultural history: Johan Huizinga (1872–1945) and Jacob Burckhardt (1818–1897).[27] His long essays on *Begriffbildung der Kulturgeschichte* testify to his interest not only in historical methodology but also in historical epistemology. Huizinga’s method, he claimed, derived from his reflection on the “spirit” of the Netherlandian nation, preferring thinking through the “concrete” and the “visible” rather than thinking through the “abstract” and the “conceptual.” His style of thinking and methodological choices ultimately positioned him more in the field of journalism than in that of historians, in the eyes of Koebner. At the same time we find Koebner no less critical of Burckhardt and his epoch-making concepts of the Renaissance, the “state as a work of art,” “the individual,” but from the opposite direction. Here he argued that Burckhardt’s conceptual work was too vague and not really philosophical.

How did Koebner think of stabilizing and reconciling the representation of facts and their conceptualization? How was the historian capable of attaining true, objective knowledge that differentiated history from all other kinds of historical writing, such as that of the Kulturwissenschaften of Huizinga and Burckhardts? And how was it possible to articulate and legitimize the superiority of historians’ “historical consciousness” over the popular one which he aspired to criticize, and perhaps correct?

One short but explicit articulation of his views that touches on the problems of history as a discipline or science can be found in the essay published in 1945 “On the object of scientific historiography.”[28]

Into this short piece published in 1945, through which he attempted to convey an intense, urgent message, Koebner condensed two major issues: first, a clear

[25] Ibid., p. 275.

[26] Koebner (1951).

[27] Idem (1934).

[28] Koebner (1944).

differentiation of the field of "history" from other kinds of historical investigations such as those of historians of art or philosophy; and second, the special status history deserved, in his eyes, in the public life of the nation. Thus, he tried to create a nontrivial bridge between historical contents and historical epistemology on the one hand and the role of historians in "the political"—not in politics—on the other. History, he claimed, is the science of "past facts," attained through "continuous and methodical observation," but done from the present and incorporating the point of view of the present. Koebner was very conscious of the tensions he was trying to overcome while putting into action his acute dialectical mind: "In the affinity of history and the public life of the present some essential facets of history that do not comply with the nature of science are outstanding,"[29] he wrote, a propos scientific historiography, and turned to three of these facets: subjectivism, the interest in the particular, and the embedding of history in passing actualities. While admitting the difficulties of writing universal and yet actual history, he still went on to point out the conditions under which this enterprise seemed possible. A globalist "avant la letter:" he was trying to anchor the historians' consciousness in universal political principles (I guess he meant modern liberal principles) and in international public opinion. Ultimately, he relied on the ability of "writers" to create one continuous, universal space of the present fed by insights stemming from the observation of the past and confronted with a similar space of the past, fed by views of the present.[30] Within this space, he believed, historians were able to produce "objective knowledge" that distinguished them from cultural historians, for example, who could afford to be relativists.

In practice, however, I believe Koebner was still relying on the Kantian and Humboldtian practices of separation of fields of study and boundary formation between the political, the social, and the scientific. Both Ernst Simon and Yehoshua Arieli, his colleagues from the Hebrew University, emphasized in their writings on Koebner that he was an engaged intellectual, committed to the public cause first of the Yishuv and then of the State of Israel. In this mood he published, in 1945, an essay condemning Jewish acts of terror. And yet he entitled it: "Non-political reflections on our political troubles."[31] Thus he reasserted the view that there is a limit to the license of the historian to undermine the "historical consciousness" of laymen in the social and national framework in which he chose to live his life. The Kantian practice of imposing a limit on oneself was certainly not alien to Koebner's modes of thinking and acting.

A few words that come to mind instead of a summary and conclusion:

That particular knowledge about human beings which is so closely related to society and structures of power and yet is differentiated from them is our wisdom. Genealogical research and historical reflection enable us to think through this wisdom as a "wound," an inner split between the particular and the universal, the practical and the theoretical, knowledge and value, power and knowledge. But this

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[29] Ibid., p. 99.

[30] Ibid., pp. 100–101.

[31] Koebner (1945).

wound has maintained the vitality of our wisdom for generations. In my essay I have followed not only the contradictions permeating this wisdom but also a few strategies that allowed its survival. Lately—since the nineteenth century—the humanities, or the Geisteswissenschaften, les sciences de l'homme, or Madaei Haruach—thrived in special spaces, within universities, and more or less protected from economic hardship and state intervention. But the modern arrangements of institutionalization, of "laws of higher education" supposed to protect our freedom, of a nation-state committed to pay our salaries, no strings attached—all these seem not to be working any more. We may have to make new alliances: some are already doing it, writing "deep histories" of homo sapience and beyond, realigning themselves with the sciences of evolution, with scientific linguistics and scientific archeology. We may think of connecting ourselves to additional sites of knowledge: museums, archives, libraries, research institutes. We may, on the other hand, think how to redefine our boundaries without losing our autonomy. Many other options may open up if we do not leave our fate to the market and to the regime, lamenting our fate without engaging ourselves in the effort of earnestly rethinking the problematics of the humanities.

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Chapter 18
On Scientific Biography and Biographies of Scientists

Helge Kragh

Abstract The genre of scientific biography is among the oldest in the history of science literature, but its historiographical value has not always been appreciated. With the professionalization of history of science in the post-1950 period, and especially with the turn to social history in the 1970s, biographies of individual scientists became somewhat unfashionable. Although it is generally agreed that biographies that integrate social and institutional dimensions are preferable, the approach is not without problems. One problem concerns the division between science and non-science, and another the involvement of the biographer in the history of his or her chosen subject. In the discussion of the merits of scientific biographies, it is important to recognize how broad and varied the genre is, not least when it comes to audiences. Although the standard biography deals with the life and work of an individual scientist of the past, there are also interesting experiments with more non-standard kinds of biography.

Keywords Biographies • Historiography • History of science • Contextualism • Fritz London • Tycho Brahe

18.1 Introduction

According to a traditional but far from uncontested view, scientific knowledge of nature is essentially due to creative scientists; they do not work in isolation, of course, but the roots of science are nonetheless to be found in the individual scientist. If this is the case, biographies seem to be fundamental to the history of science. Most biographers agree that the life and work of a scientist should be narrated by integrating the subject's scientific contributions into the relevant social contexts. However, this consensus view of an integrated or contextualist approach does not solve all the problems that face the biographer. Some of these problems, if far from all them, are pointed out in the essay. While it should no longer be necessary to defend the art of biography as an essential part of history of science,

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it is always worth contemplating its strengths and limitations. The works discussed in the essay are limited to full-scale scientific biographies in the form of monographs, whereas I disregard the very extensive biographical literature published in the form of articles, entries in dictionaries and the like.

18.2 A Scientific Biography of Fritz London

In 1995 Kostas Gavroglu published a major biography of a scientist who would usually be counted as a relatively minor figure of twentieth-century science. The biography of the German-American physicist Fritz London, who made important contributions to areas such as quantum chemistry, superfluidity and superconductivity, is in some respects typical of the genre of science biographies, while in other respects it is somewhat atypical. I shall use the book as the point of departure for a discussion of themes of a general nature related to science biographies or what are often called scientific biographies. The two terms are sometimes used with different connotations, the term "scientific biography" taken to imply a biographical study with an emphasis on the science of the portrayed scientist, or at least one in which his contributions to science is dealt with in no less detail than his life and career. Gavroglu's book is summarily entitled *Fritz London*, with *A Scientific Biography* appended as a subtitle (Gavroglu 1995; Kragh 1997). Incidentally, a few years earlier I had published a scientific-biographical study of the British quantum physicist Paul Dirac with a similarly unimaginative title (Kragh 1989).

In cases where the source material is abundant the author of a biography needs to face the selection problem, to make decisions of what to include and what to leave out. This is not specifically a problem for the biographer, as it is part and parcel of historical writing in general, but it often turns up with particular force in the biographical genre. It is a problem to which there is no general solution. Realizing that "too much detail is always detrimental to the story as a whole," Gavroglu (1995, p. xvii) says: "To decide when one stops researching; to decide what not to include; to decide when a biography stops is a highly personal decision." London's scientific work was technically demanding, yet to leave out his science would be to leave out an essential part of his life and the whole rationale for writing his biography. But how much should be included and in what detail? This is another dilemma authors of scientific biographies are often faced with, a dilemma which is obviously tied up with the intended readers. "I do not think that a scientific biography should be aimed only towards a readership with scientific background," Gavroglu (1995, p. xix) says, framing his work accordingly. This does not imply eschewing technical details, but presenting them non-mathematically and in such a way that the overall story does not depend on these details.

Reflecting on the essence of biographies of creative people, Mott Greene has argued that the scientific biography is a "hero's quest" that can be brought to fit a folkloric template. Referring to Gregor Mendel, the Austrian monk and precursor of genetics, he notes the absence of a hero's quest and that "almost every major folkloric element which might recommend it [Mendel's life] for biographical treatment is completely missing from the life he lived" (Greene 2007, p. 750).

In this respect the life of Fritz London resembled that of Mendel, for London lived a rather uneventful life and did not have the mythical or heroic status of an Einstein, a Heisenberg or a Feynman. A private person who totally lacked charisma, he disliked publicity and competition and despised the fashions that he found in the pragmatic American culture of science. The fact that Gavroglu nonetheless wrote a highly interesting biography of London suggests that Greene's analysis is unnecessarily restrictive. Indeed, it is very difficult to give general criteria for which scientists are worthy of a biography and for what reasons. Not only would such criteria depend on the era in which the scientist lived, it must also be admitted that the different sciences cannot be treated collectively.

According to Mary Terrall, the biographer of the French Enlightenment polymath Pierre-Louis Moreau de Maupertuis, self-fashioning and the role of the public was all-important for Maupertuis and contemporary natural philosophers; these elements should consequently be given prominence in biographical studies, she suggests (Terrall 2002; Terrall 2006). The significance of self-fashioning, patronage and the public has certainly not diminished over time, although it has changed considerably in form. It is an important element in the lives of some scientists, and should therefore be part of their biographies; such is the case in, for example, Gale Christianson's fine biography of the great astronomer Edwin Hubble (Christianson 1995). On the other hand, there are also important scientists in whose life self-fashioning and the desire for publicity played no role at all. London is one example, and Dirac another.

18.3 Ups and Downs of Science Biographies

Although professional or academic history of science dates back to the first half of the twentieth century and caught on only after World War II, science and scientists have been described in their historical contexts much earlier. The scientific biography has its own interesting history, running parallel with the development of science and its historiography (Shortland and Yeo 1996b; Söderqvist 2007b). I can only deal briefly and fragmentarily with this dimension.

Without stretching the notion of history of science too far, one can find the genre during the scientific revolution, typically written by natural philosophers taking part in or observing the revolution. Moreover, some of the earliest history of science was biographical in nature. Thomas Hankins (1979) drew attention to the famous *éloges* written at the Paris Academy of Sciences by Bernard le Bouvier de Fontenelle, who from 1697 served as secretary of the Academy. Fontenelle and his successors as eulogists were faced with the difficult problem of combining panegyrics with historically accurate biographies. While they did provide such accurate and documented biographies, they also used them to promote the view of science, morals and society they found desirable (Paul 1980). Fontenelle's famous *éloge* of Newton was brilliant, but clearly the work of a conservative philosopher who saw it his duty to defend Cartesianism and consequently distorted parts of Newton's life and work.

But scientific biography did not start with Fontenelle. There are even earlier and no less interesting examples. The first comprehensive biography of a scientist in the form of a monograph, Pierre Gassendi's biography of Tycho Brahe from 1654, is a remarkable work that served as a blueprint for later biographies of the Danish astronomer well into the twentieth century (Kragh 2007). Detailed and carefully researched, *Tychonis Brahei vita* focused on Tycho's astronomy rather than his life and personality, but it also included fairly extensive accounts of his interests in astrology, Latin poetry and chemistry in the Hermetic tradition. Contrary to many later biographies, it was not clearly hagiographic. Scientific biographies may serve many purposes, some of them moral, others political and others again scientific. For example, they may provide more or less propagandistic arguments for a particular view of science. By representing the great discoveries of a highly respected scientist as the result of a methodological choice, the biographer will indirectly advocate one kind of methodology at the expense of others. A leading representative of the new empiricism, Gassendi used Tycho's scientific life as powerful propaganda for the cause of empiricist science. His Tycho was a genius of observational astronomy, a masterful instrument-builder and an untiring collector of accurate astronomical data. On the other hand, Gassendi gave low priority to those aspects of Tycho's work that did not fit his empiricist picture, such as Tycho's theoretical astronomy and arguments for a geocentric cosmology.

While Enlightenment historiography generally conceived science as a rational progress depending on the genius of individual natural philosophers – Newton being the greatest of all – there were only few books depicting their life and science. Moreover, some natural philosophers of the period stressed that science was a cumulative enterprise in which social and political factors were no less important than the individual genius. Joseph Priestley was not opposed to biography, but he resolutely resisted hero worship and the one-sided emphasis of the role of genius in the progress of science. In his semi-historical account of the progress made in electrical studies, he avoided hagiography, yet at the same time deliberately censored the history for elements that did not fit his notion of what scientific progress should be. "Did it depend on me," he wrote, "it should never be known to posterity, that there had ever been any such things as envy, jealousy, or caviling among the admirers of my favourite study" (Priestley 1769, xi).

In spite of the examples that can be found in the Enlightenment and earlier, it was only in the nineteenth century that books on the life and work of scientists emerged as a separate genre. The Victorian era is famous as well as infamous for its obsessive interest in biography, including scientific biography. As the volume of biographical books increased, they also became less critical, many of them deteriorating into hagiographic descriptions of worthy scientists, authors and artists. The Victorian biography of the life-and-letters genre was typically written within the framework of the social and moral conventions of the time. In several cases this led to distortions of the historical record, such as was the case with the *Life, Letters and Journals of Charles Lyell* published in 1881 (Shortland and Yeo 1996b, p. 23). On the other hand, books of this kind often placed the subject's life and work in a broader cultural context and for this reason alone they remain valuable documents for later historians.

While in the early twentieth century a large part of history of science was still written in the form of biographies, with the gradual professionalization of the field after World War II biographical studies came to be seen as much less important, even lacking in scholarly respectability. Authors of biographies typically felt a need to justify their enterprise, to explain to readers and peers why they had invested so much time and effort in studying the life of a single scientist.

There were several reasons for the peripheral status of biography, one of them being the predominant positivist view of science as a cumulative body of knowledge in which discoveries and theories of individual scientists were regarded as irrelevant in principle. The generally accepted separation between the context of discovery and the context of justification did not encourage biography. Another development that tended to sideline the biographical approach was the new sociology of science pioneered by Robert K. Merton in particular. Empirical sociology of science in the tradition of Merton was far from foreign to history, but it had little use of biographical studies. They became marginalized, replaced by the prosopographical studies that promised a better understanding of the social and cultural dimensions of the scientific enterprise (Kragh 1987, pp. 174–181). On the other hand, although prosopography differs from traditional biography, the two genres are not mutually exclusive. In John Christianson's innovative portrait of Tycho Brahe, biography is skillfully blended with a prosopographical study of Tycho's large network of assistants, astronomers, clients and patrons (Christianson 2000).

The trends away from the personal were reinforced by later developments in sociology of science or social studies of knowledge. According to Pearce Williams (1991) social constructivists tend to dislike biography, oriented as it is toward the elite of scientists. Thomas Söderqvist (1996, pp. 49–52) likewise considers the sociological turn in history of science to be a challenge to the art of biography, namely, by changing the traditional individual-centred form to a kind of social biography.

Writing in a different context and not referring to history of science in particular, in his 1962 classic *La pensée sauvage* the French anthropologist Claude Lévi-Strauss expressed his low opinion of biography as a genre of history. He grouped biographical history together with anecdotal history, the two having in common that they were "weak history." As Greene points out, a biography needs to be placed in a context where it is supported by a narrative stronger than the merely biographical (Greene 2007, p. 728).

18.4 The Integrated Biographical Approach

In his 1979 argument for reviving interest in biography as an essential part of history of science, Hankins (1979, p. 2) noted, undoubtedly correctly, that "scientific biography does not enjoy a very good reputation these days." If the trend in recent history of science has been towards an increased focus on social, cultural, institutional and economic factors – and this has clearly been the case – it might seem that there is little need for biography in the traditional sense. After all, by its very nature biography is person-centred. It is concerned with the life, thoughts and actions of an individual, which apparently places it in the "internalist" rather than "externalist" camp, to use the old and no longer fashionable labels. However, there is no necessary disagreement between biography and externalism or what in its modern version is called contextualism.

If a biography describes its subject in close interaction with the social and cultural factors predominating at the time, as many modern biographies do, the true subject of the biography is no longer just the individual as a person. By placing the scientist in the environment relevant to him, and documenting the relevance of the environment to his scientific work, we will not only come to know his science better but also how the social forces helped in shaping it. As Hankins phrases it, biography may serve as a "literary lens" through which we can study the impact of external factors on science. One of the advantages of the biographical method is that it stimulates a more integrated and coherent picture of science, if limited to a unique case only, precisely because of its focus on the individual scientist. Hankins (1979, p. 5) again:

> We can say at least one thing with certainty about biography: the ideas and opinions expressed by our subject came from a single mind and are integrated to the extent that that person was able to integrate them in his own thoughts. We have, in the case of an individual, his scientific, philosophical, social and political ideas wrapped up in a single package. … If biography is honest, we can learn a great deal about the way in which science works, and we can also be protected from too-hasty generalizations.

Most historians will probably welcome such integrated or unified biographical studies, but the programme is not unproblematic and it does not render superfluous the priority issue related to the division of scientific and socio-cultural factors. While Hankins considers the subject's scientific contributions to be essential in any scientific biography – if not, why call it "scientific"? – he has been criticized for putting too much emphasis on "science as a special form of human activity" (Sheets-Pyenson 1990, p. 403).

Addressing the issue of integration from a different perspective, Pearce Williams argued that although there may be no necessary contradiction between sociological studies of science and the classical biography, in the version of social constructivism the contradiction is more than just apparent. A conservative bemoaning the entrance into history of science of "sociologists, anthropologists, ethnomethodologists, feminists, semioticians (sic), psychologists and even ecologists," he maintains that biographies of great scientists are fundamental to the history of science. Moreover, he considers the biographical method advantageous because of its allegedly empirical nature: "The nice thing about writing a biography is that it forces the historian to focus on his subject and does not permit him to wander aimlessly in the social swamp. Thus relevance is rather precisely defined for him; it is everything that can be discovered that impinges upon his subject" (Williams 1991, p. 204).

Today the integrated or unified approach to scientific biography is the rule rather than the exception, at least for biographies of a scholarly nature. Even Williams admits that *Energy & Empire*, the acclaimed biography of Lord Kelvin written by Crosbie Smith and Norton Wise, shows that biography and a soft version of social constructivism can co-exist (Smith and Wise 1989). Another example, if of a somewhat different kind, is David Cassidy's no less acclaimed biography of Werner Heisenberg, in which the German physicist appears as much more than just a quantum genius who transformed our physical world picture (Cassidy 1992). Heisenberg's personal ambitions and political motivations, his relationship to Niels Bohr and his responses to the rise and fall of the Third Reich – all this and more forms part of Cassidy's biography. Or consider Sam Schweber's recent biographical study of Hans Bethe, which to a large extent is about the influences that formed Bethe as a human being and scientist, including the institutions and social networks that made his career possible (Schweber 2012). Many other examples could be mentioned, such as Gavroglu's 1995 book on Fritz London, which succeeds admirably in integrating the personal and professional aspects of London's life. While economic and political factors were of no particular importance for his science, philosophical and cultural factors were, and Gavroglu argues that they significantly shaped his work in theoretical physics.

It is important to note that in none of the books here mentioned does the integrated approach imply that the subject's contributions to science are neglected or given low priority. The books have in common with many other recent scientific biographies that they, directly or indirectly, pose questions related to the subject's moral conduct and public virtue. Ethics has always been an issue in biography, but it is only relatively recently that it has entered significantly in biographies of scientists. John Heilbron's balanced portrait of Max Planck, the celebrated father of quantum theory, is as much or more about the moral dilemmas that Planck faced during his long career as it is about his contributions to physics (Heilbron 1986). Another and more controversial example is Gerald Geison's *Private Science of Louis Pasteur* (1995) which is highly critical to the research practice and ethics of the great scientist and French national hero. The book became part of the so-called science or culture war, with the eminent molecular biologist Max Perutz (1995) angrily accusing Geison of unethical conduct. As demonstrated by this instructive case, it is not only the scientist's morals that may become an issue in biographies. The same may be the case with the moral behaviour of the biographer.

18.5 A Wide Spectrum

Biographies of scientists may still have a dubious reputation in some circles, but they stand out from other academic books in the history of science by having a remarkable public appeal. In a branch of learning always seeking for a wider audience, this is a distinct advantage. Not only do popular biographies of famous scientists sell very well, the same is the case with some of the scholarly books written by historians. Remarkably, Adrian Desmond and James Moore's *Darwin* from 1991 has sold at least a 100,000 copies (Söderqvist 2007a, p. 2). Other biographical best-sellers, if not perhaps on quite the same impressive scale, include Heilbron's work on Planck, Cassidy's on Heisenberg and Abraham Pais' biography of Einstein (Pais 1982). The latter book is exceptional in that it includes large doses of heavy mathematics, something that is normally assumed to limit readership substantially. In any case, generally accessible scientific biographies seem to be a potent answer to what has been called the crisis of readership in the history of science (Shapin 2005).

The genre of scientific biography covers a broad spectrum, both with regard to authors, subjects, audience and style. Many of the books are written by professional historians, but there are also excellent biographies written by journalists or non-academic authors and science writers. An example is James Gleick, who wrote a very successful book about Feynman (1992) and later a biography of Newton (2003). While most biographies portray famous scientists, a few deal with less known or even obscure figures, sometimes with the aim of lifting them out of obscurity and giving them their "proper place" in the history of science.

Luke Howard, a British Quaker pharmacist and meteorologist born in 1772, has no entry in the *Dictionary of Scientific Biography* (but is included in the *New Dictionary of Scientific Biography* of 2008). Yet he was well known in the early nineteenth century, corresponding with Goethe and serving as an inspiration for Shelley and other poets. Today he is recognized as a "father of meteorology," a label principally due to his classification of clouds and understanding of their importance in meteorology. Howard is portrayed in a fine book by Richard Hamblyn (2001), which may not qualify as a scholarly study but is nonetheless an informative account of value to historians of science. There are several other examples of this biographical subgenre, popular and easily read books mainly addressed to the general public. They vary in quality but all belong to the broad landscape of science biography.

"Are the most popular scientific biographies, as a rule, books about the scientist or books about science?" asks Mary Jo Nye (2006, p. 324). Another version of the question relates to the amount of technical science needed in a biography. There is no general answer to the question, I believe, except that a scientific biography must contain *some* explanation of the science with which the subject is connected. As Gavroglu (1995, p. xix) points out: "The Sisyphean pattern between a popular account of the scientific work and a highly technical presentation of it, is almost an inherent feature of a scientific biography."

Science in the technical sense does not need to be the main focus of the biography, and in many cases it is not. How much science to include and in what technical detail depends on the subject of the biography and, not least, on the intended audience. While many biographies are written for either a general audience or for historians of science and culture, others are primarily written for scientists or scientifically trained historians. They often presuppose expert knowledge to such a degree that they are unreadable for or will appear irrelevant to non-scientists. The most complete biography of the Austrian physicist Wolfgang Pauli, written by one of his former students, is unfortunately of such a technical-mathematical level that most readers without training in theoretical physics will be intimidated (Enz 2002). It is detailed and informative, but not of the contextual or integrated kind that most historians appreciate.

I am not implying that biographers should avoid technical details, for in some cases they are essential to the life of the portrayed scientist. When I wrote my biography of Dirac, I felt it imperative to include and discuss some of the quantum equations that occupied such a central position in Dirac's life. I did not then imagine that one could write a good biography of Dirac without equations, although later I was forced to change my mind (Farmelo 2009). In general it is fairly unproblematic for a biographer to reduce the technical details or separate them from the main narrative, if necessary by placing them in an appendix. This, for example, is the strategy adopted in Schweber's new biography of Bethe.

Science biography does not necessarily mean a biography of a deceased or still living scientist. As pointed out by Shortland and Yeo (1996a, p. 1) "biography" has become a potent selling tag and the possible subjects of biography proliferated. It is not unusual to publish "bibliographies" of, for example, scientific ideas or technological innovations. Gale Christianson (1999, p. x) presented his history of the greenhouse effect from Fourier to the present as "the biography of a scientific idea." If ideas can be subjects of biographies, why not entities such as electrons and neutron stars? According to Theodore Arabatzis (2006, p. 43) they can indeed, for there are "interesting parallels between the history of a theoretical entity and the life of a person." Although admitting that this is biography only in a metaphorical sense, he maintains that it is a legitimate and useful notion. The question is whether it is biography at all.

Yet another unorthodox version of scientific biography is found in the cases where an author has *invented* a scientist and then written his life story, thereby crossing the boundary between fiction and authentic biography. (Such crossing may also take place in the form of novels about historical figures, such as Kepler or Marie Curie.) The best known and most successful example is Russell McCormmach's pioneering study of Victor Jakob, a fictional German professor of physics. By inventing or constructing a scientist, McCormmach (1982) provides a captivating analysis of how an average physicist in the early twentieth century responded to the revolutionary trends that threatened to undermine the classical foundation in which he was trained and so much appreciated. For all the insight Jakob's life story gives to this chapter in the history of physics, it is not a scientific biography in the ordinary sense. It is an experiment in historiography.

Finally, in this brief survey I have only included monographic biographies of the usual kind, that is, books recounting the life and work of an individual scientist. In addition to this standard genre, there are also books on the "parallel lives" of two scientists whose careers intersected in significant ways. By telling the life story of two related individuals, in some cases the author can say more about their lives than by treating them separately. This is what Michael Hoskin (2011) has done in the case of the astronomer William Herschel and his sister Caroline. Likewise, Schweber (2000) has compared the different yet parallel lives of Bethe and J. Robert Oppenheimer, in particular with respect to the shaping of their moral outlooks.

18.6 The Role of the Biographer

The author of a biography is part and parcel of the work he or she creates. This is in part due to the rather obvious reason that it is up to the author to shape the story of the subject's life, to decide what sources to use, what the story shall include and what shall be left out. We sometimes hail a biography as the "definitive" account of a scientist, but there is no such thing as a definitive biography. Every generation has its own Galileo and its own Darwin. This is not only because of the discovery of new sources; it is also because the perspective of one generation of historians typically differs from that of the previous generation.

We should be aware of the dangers of presentism but not close our eyes to the fact that the present cannot avoid influencing our understanding of the past. What we find interesting and relevant in the life of a scientist legitimately changes as time progresses. To mention but one example, the Swedish chemist and physicist Svante Arrhenius is primarily known for his ionic theory of dissociation, a cornerstone of physical chemistry, but today also for his early discussion of what came to be known as the greenhouse effect. The first biography of Arrhenius, written in 1931 by a chemist, does not even mention the 1896 paper on the greenhouse effect; and in the detailed entry on Arrhenius in *Dictionary of Scientific Biography* of 1970 it is only dealt with cursorily. By contrast, it occupies a central position in Elisabeth Crawford's later and much fuller biography, even appearing in its title (Riesenfeld 1931; Crawford 1996). The reason is of course that the problem of global warming has attracted massive scientific and political interest only in recent time.

Authors of a biography cannot avoid being influenced by the subject they write about. They may come to regard their subject with either sympathy or antipathy and generally see themselves as involved in the values and actions of the subject's life. The transference of part of the biographer's self to the life of the subject of a biography is particularly hard to resist if the biographer has spent a long time with the subject and conducted many interviews with him (e.g. Söderqvist 2003; Schweber 2012). But even for subjects separated in time from the biographer by centuries, the author may become deeply involved both personally and emotionally. Only after Richard Westphall had completed *Never at Rest*, his magisterial biography of Newton, did he realize that his own views and Christian belief had influenced his picture of the great natural philosopher. His biography had become, in part and in a sense, an ideal autobiography (Westphall 1980; Shortland and Yeo 1996b, pp. 31–34).

In some cases, if not in all, the writing of a biography is a kind of collaboration between the author and the subject, whether alive or long deceased. Now, readers of a biography want primarily to know about the subject, not about the biographer who is supposed to be irrelevant. The problem is that he is not and cannot fully be so. The degree to which the author's person and views enter the biography, thereby adding a certain subjectivity to it, should be done in such a way that readers are aware of it. If not, they will not know whom they are reading about.

If there is a higher aim of science biography as a genre, one that goes beyond mere information about the lives and deeds of scientists, what is it? Most biographers would probably deny that there is such a higher aim, but Söderqvist argues that there is and that it should be sought in our identification with the life stories of individual scientists. In what he calls the existential approach, the primary aim of science biography is "to help scientists and non-scientists alike to strengthen their abilities to live fuller and more authentic intellectual lives" (Söderqvist 1996, p. 75). For this reason he also calls his vision of existential biography a project of edification. Whatever may be said about the project, it is likely to remain a vision.

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Chapter 19
Biography and the History of Science

Mary Jo Nye

Abstract As a genre at the intersection of history and literature, biography challenges its writer to decide organizational rules and elements of plot that are faithful to the subject and attractive to the reader. Mary Jo Nye suggests that there are three principal forms of biography in which the subject is a scientist: the life of the scientist, the scientific life, and the life of scientific collaboration. She explains the meaning of these terms by drawing upon a range of recent biographies in modern science, including Kostas Gavroglu's biography of Fritz London.

Keywords Biography and history • Scientific life • Scientists' lives • Collaborative science • Fritz London • Robert Oppenheimer • Paul Dirac

19.1 Introduction

In an essay of 1939 Virginia Woolf asks the question whether biography is an art, and she answers—controversially, I would say—that it is not. The artist has a freedom that is denied to the biographer, she writes, because the biographer is constrained by verifiable facts, however few or however many the facts may be. In her words, "The novelist is free; the biographer is tied." The biographer does not have the license of creativity that is granted to the artist; "he is a craftsman . . . and his work is not a work of art, but something betwixt and between" (Woolf 1942, 188, 196).

By way of making her point, Woolf discusses the recent writings of Lytton Strachey and what she credits as a new biographical genre that departs from the "wax figures" and hagiography of Victorian lives and letters in favor of psychological portraits and intimate narrative. For Woolf, Strachey's experimentation in biography fails, however, in his portrayal of Queen Elizabeth in *Elizabeth and Essex: A Tragic History* (1928). Elizabeth "moves in an ambiguous world, between fact and fiction, neither embodied nor disembodied," writes Woolf, because Strachey inclined himself too much to invention, lacking the rich documentation of

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every aspect of Elizabeth's life that had made his *Queen Victoria* (1921) a triumph (Woolf 1942, 192).[1]

Still, Woolf saw a bright future for the kind of biography that Strachey had pioneered, and she proposes in her essay that "the biographer must go ahead of the rest of us, like the miner's canary, testing the atmosphere, detecting falsity, unreality, and the presence of obsolete conventions." The biographer's "sense of truth must be alive and on tiptoe," "hanging up looking glasses at odd corners" and enlarging the focus beyond the lives of great men to "the failures as well as the successes, the humble as well as the illustrious" (Woolf 1942, 195).

It is notable that Woolf wanted biography to portray the lives not only of emperors and generals of the past and present, but of less heroic individuals than the larger-than-life figures immortalized in Victorian literature. She was keen on the kinds of insights achieved in pioneering work of eighteenth-century English biography such as James Boswell's 1791 *Life of Samuel Johnson*, a biography which was original for its candid presentation of the personality of its subject and for its empirical foundation in documentation that included letters, private papers, conversations, and personal observations (Possing 2012, 3). Separated by the Victorian century, Boswell and Strachey each had an approach to biography that asserted equality between biographer and subject, rather than adherence to canons of hero worship, hagiography, or ethical instruction (Marcus 2002, 196; Possing 2004, 2–3).[2] Woolf liked this kind of biography, and it would be based in fact, not fiction.

19.2 Biography and History

Woolf was correct in thinking that biography would have great appeal in the future. A 1994 poll on reading habits in Great Britain found biography to be the most popular category of non-fiction book and a genre of literature considerably ahead of contemporary fiction by 19 % to 14 % of readers (Shortland and Yeo 1996, 1). A Harris poll in 2011 found that among Americans who read at least one book a year in print or electronically, 76 % said that they read both fiction and non-fiction, with 29 % of non-fiction reading including biography and 27 % history (Harris Poll 2011). Academic groups exist to advance the craft of biography and its theoretical foundations. Among these are the Zentrum für Biographik (ZetBi) headquartered at the Humboldt-University and the editorial boards of the periodicals *Journal of*

[1] Strachey's first great biographical success was *Eminent Victorians* (1918) with biographies of Henry Edward Manning, Florence Nightingale, Thomas Arnold, and Charles George Gordon.

[2] Birgitte Possing gives as an example for hero worship: Thomas Carlyle's *On Heroes, Hero-Worship, and the Heroic in History* (1841); for hagiography: John Foxe's *Book of Martyrs* (1563); and for ethical biography: Plutarch's second-century *Parallel Lives*.

Historical Biography, based in Canada, and *Journal: Life Writing* in Australia (*Journal of Historical Biography* 2012).

Yet as suggested by Robert Schneider, editor of the *American Historical Review* when he convened a roundtable on the subject of historians and biography, "academic historians have been somewhat ambivalent about the genre of biography" (Nasaw 2009, 573). In that 2009 roundtable, the historian David Nasaw described the state of affairs in the following way: "Biography remains the profession's unloved stepchild . . . Graduate students are warned away from writing biographies as their dissertations. Assistant professors are told to get tenure and promotion before taking on biography. College and university libraries, including my own, adhere to collection protocols that discourage the purchase of biographies" (Nasaw 2009, 573).[3]

Yet historians write biographies and they do so, Nasaw suggests, because "that is where the readers are," especially in the audience outside the academy (Nasaw 2009, 575). Nasaw reported in 2009 that five of the last eight presidents of the American Historical Association had written or edited biographical studies and that the Bancroft Prize in United States history had been awarded to a biography three times in the past 8 years (Nasaw 2009, 575). On an occasion marking the fiftieth anniversary of the Dexter and Edelstein Awards, given by the American Chemical Society for outstanding achievement in the history of chemistry from 1956 to 2006, it was noted that at least twenty of the fifty prize writers had tackled the craft of biography (Nye 2007, 21). These include volumes of short biographies, in a somewhat similar format if not the same spirit of Strachey's *Eminent Victorians*, but more often biographies focused on single individuals. Antoine Lavoisier is prominent among chemical subjects. Indeed six Dexter winners wrote one or more books about Lavoisier, with thematic variations that emphasized Lavoisier as the French originator of modern chemistry or Lavoisier as social reformer and revolutionary or Lavoisier as gifted experimentalist or Lavoisier as a consummate insider in the scientific elite (McKie 1936, 1952; Daumas 1955; Bensaude-Vincent 1993).

Historians use biography, Nasaw suggests, in order to illuminate not only the individual but also the points of intersection of the individual with the historical situation. The historian as biographer, he writes, "might well take as her credo" the statement by Karl Marx in his *Eighteenth Brumaire of Louis Bonaparte* that "'Men make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly encountered, given, and transmitted from the past'" (Nasaw 2009, 574, quoting Marx 1963, 15). This point of view focuses the biography on choices that the biographical subject makes, the repercussions of those decisions, and the meaning of that individual's life in the historical circumstances of the time.

Similarly, the historian of science Mott T. Greene writes of biography as judgment of an individual "folded into a narrative stronger than itself"

[3] Nasaw is the Arthur M. Schlesinger, Jr. Professor of American History at the Graduate Center of the City University of New York. He is an author of biographies.

(Greene 2007, 728). In Greene's view, the aim of the biographer's craft is the building of individual character within a narrative. Certain rules must be followed, the first of which is the rule of veracity. A second rule is one of sequential order. A third rule is that of inclusion of all "important" acts and events in entirety. The fourth and final rule—reminding us of Woolf—is the rule of verifiability. While strictly keeping to these rules, the biographer is free to choose the plot (Greene 2007, 729–732).

A good plot is a good story, and a typically good story follows the scheme of *Bildungsroman* found in the novel of self-development. Greene convincingly gives us the characteristics of the story: an element of psychological and intellectual consistency in the subject's life, often first adumbrated in childhood; the autonomy achieved by the individual as "author of his own life;" and, for the artist, intellectual or scientist, the discovery or invention of something important and an account of a turning point or creative act (Greene 2007, 732–734). Other tropes consistent with *Bildungsroman* are the measurement of the biographical subject against an ideal—a Weberian ideal type—who could be imagined behaving perfectly; and the telling of the story in the folk-tale vein of the hero's quest, beset by struggles and aided by grace or magic before victory and reward are recognized in front of the assembled representatives of the realm (Greene 2007, 736–745).

Where a scientist is the biographical subject, the biographer as historian has thematic choices that are determined by the historian's own stake and interest in the matter. For example, the biography may be the **life of a scientist** (just as we might write the life of a statesman or sculptor) whose actions and values seem to the biographer to exemplify the historical period or to have affected the events of that period in a meaningful way. Or, the biography of a scientist may be **a scientific life** in which the observations, experiments or theories of the individual are keys to the understanding or the development of science of the past and the present. Lastly, the biography may be a **life of scientific collaboration** in which individuals in a community play interlocking roles that the biographer sees as key to understanding both individual lives and the organization and processes of a scientific enterprise. As we will see, these three categories are neither exhaustive nor exclusive in their boundaries, but they provide a useful framework for thinking about biography and the history of science.

19.3 The Life of the Scientist

In what I call the life of the scientist, the biography is a study of the central protagonist in a broad sweep of historical events beyond the scientist subject's expertise in a scientific discipline. Among scientists, J. Robert Oppenheimer is one of the best examples of the scientist as subject whose role in the development of scientific theory—in Oppenheimer's case, theoretical physics—is a crucial element in the story of his life, but whose narrative and dramatic interest for most biographers lies not in the technical science, but in a broader history. For many of

Oppenheimer's biographers, this is the history of twentieth-century America and Oppenheimer's rise to leadership and fall from power in the political framework of the Second World War and early Cold War. In the Preface to his book *Oppenheimer: The Tragic Intellect* (2006), Charles Thorpe contrasts some recent biographers' approaches to Oppenheimer:

> For Bird and Sherwin, Oppenheimer was an authentic voice of American scientific, intellectual, and political liberalism. For McMillan, he was a defeated moderating voice in American foreign policy . . . Schweber suggests that Oppenheimer was 'too fractured an individual' to handle the ethical and political dilemmas presented by Hiroshima and the Cold War, and he instead presents physicist Hans Bethe as the more consistent embodiment of an ethic of responsibility.

Thorpe further characterizes David Cassidy's life of Oppenheimer as a demonstration of the ways in which a new alliance of science, industry, and military power undermined the independent cultural authority of science. Echoing almost perfectly Greene's description of the Weberian ideal type for plotting biography, Thorpe writes that his own biography of Oppenheimer is a sociological biography that reflects Max Weber's idealist themes of "vocation, responsibility, cultivation and expertise, charisma, bureaucracy, instrumental reason, fact and value, means and ends" (Thorpe 2006, xiv–xvi; Bird and Sherwin 2005; McMillan 2005; Schweber 2000, 2012; Cassidy 2005). Schweber, too, explicitly invokes Weberian ideals in his contrast of Oppenheimer and Bethe (Schweber 2000, 14, 15, 93).

Many lives of scientists narrate the protagonists' scientific struggles and achievements as part of a life lived daily in a larger public culture. Biographies of Max Planck come to mind (Heilbron 1986; Hoffmann 2008). Einstein biographies are a primary example, too, and there are so many of them that the Einstein scholar Don Howard called for a temporary moratorium in his review of Walter Isaacson's 2007 book *Einstein: His Life and Universe*. On the whole, Howard liked Isaacson's biography, but Howard judged that Isaacson's treatment of Einstein's physics was insufficiently central to the book and lacked technical proficiency. "Move the chronology and the broader context into the foreground, as does Isaacson," wrote Howard, "and one risks that the science will seem incidental to the rest of the subject's life." Yet, "Present relativity as a single package" thematically in the biography" and "one loses both the narrative thread and a vivid sense of the way Einstein's scientific work was situated in a personal, professional, and political context" (Howard 2008, 126).

Some biographies of scientists living in the greater historical world better meet Howard's demands. He mentions Ruth Sime's book *Lise Meitner: A Life in Physics* (1996), which, like David Cassidy's *Uncertainty: The Life and Science of Werner Heisenberg* (1991) acquired a sales audience of over ten thousand readers despite the detail in which the scientist's physics is described (Howard 2008, 125; Sime 1996; Cassidy 1991).[4] Both biographies narrate and interrogate the responsibilities

[4] I am grateful to Ruth Sime and David Cassidy for answering my question about sales.

and choices of their subjects in the political and moral culture of early twentieth-century Germany, showing the strengths and weaknesses of unfolding character over time.

In her 1998 biography of Fritz Haber, Margit Szöllösi-Janze explicitly lays out her problem as biographer in constructing a biography that captures the complexities and meanings of Haber's life, including the science. There was the good Haber, and there was the evil (bösen) Haber. One was the genial scientist and Nobel Prize winner, the benefactor of agriculture through his synthesis of ammonia, and an intimate friend of Einstein and Planck. There also was the bad Haber, the nationalist and militarist, the war criminal responsible for gas warfare, and the coldhearted tyrant who drove his sensitive wife to kill herself. Haber's biographers have to choose what to capture of the many aspects of that life, including the scientific culture of Germany, the organization of the war economy, and financial hyperinflation after the Great War (Szöllösi-Janze 1998, 9, 15). Szöllösi-Janze succeeds admirably in unfolding Haber's life in a chronological sequence of events that are given structure by the themes of Haber's daily life and by nuances of moral judgment. The book is a prime example of the life of a scientist and of biography as history.

19.4 The Scientific Life

The appellation "scientific biography" applies to different formats for biography, but it applies most precisely to the biography in which the principal interest is the science of the scientist and the contribution of the biographical subject to scientific knowledge.

The pitfalls of scientific biography are highlighted in a review of a recent 600-page biography of the French physicist and mathematician Henri Poincaré: "Of its twelve chapters only the second, entitled 'Poincaré's Career,' tells the fascinating story of Poincaré's life in plain English" (Von Bayer 2013, 362). The scientist's life as his scientific experiments and ideas often has been preferred by scientists, as reflected in the comment by biologist and sex researcher Alfred C. Kinsey that it is nonsense to write a scientist's biography because "The progress of science depends upon knowledge. It has nothing to do with personality" (Capshew et al. 2003, 465, quoting Pomeroy 1972, 431–432).

First and foremost in writing a biography of the scientific life, the biographer wants the reader to understand the subject's scientific achievement. The biographer wants to explain how the scientist came to choose a problem, how results were obtained, and how the scientist's contributions were judged both at the time and later in history. The biographer Richard Holmes suggests the need for a full sense of this scientific life. "We want to read about scientific work as part of a life story—to know what makes a scientist tick, and what set them ticking" (R. Holmes 2012, 498). For Holmes, biographies should reveal the crucial shaping power of childhood and youth, the nature of the creative process, the inner and emotional life of the scientist, and the ways in which error and uncertainty are central to discovery in ways that are lost in the official scientific literature (R. Holmes 2012, 499).

Holmes further describes science itself, not just biography, as a story—a detective story, a mystery story, a love story (R. Holmes 2012, 499). As analyzed by Greene, the story has the elements of the hero's quest with adumbrations in childhood, the search for the holy grail, the struggles and moments of magic and insight, wrong turns, and triumphant recognition of the hero. The biography of a scientific life may delve deeply or superficially into the daily peregrinations of the hero while following the protagonist's mathematical calculations, research notebooks, diaries, published papers, and networks of citations and collaborations in the scientific life. Much depends on documentation, interviews, and insights available to the biographer, as well as the biographer's particular interests.

Different approaches can be chosen, as in two biographies of Paul Dirac. Helge Kragh's study is titled straightforwardly *Dirac: A Scientific Biography* (1990). Most of the book's chapters treat Dirac's different interests and achievements in theoretical physics in their historical context, with some chapters devoted to Dirac's personal life, travels, and philosophical thoughts. "Because Dirac was a private person, who identified himself very much with his physics, it is natural to place emphasis on his scientific work, which, after all, has secured his name's immortality," writes Kragh (1990, ix). In contrast to Kragh, however, the biographer Graham Farmelo gauged that Dirac's private life held the key to understanding the scientific work, and Farmelo titled his 2009 biography *The Strangest Man: The Hidden Life of Paul Dirac, Mystic of the Atom*.

Making extensive use of private papers held in the Dirac family and engaging in detailed conversations with members of the family, Farmelo describes Dirac's extraordinary investigations and achievements in quantum mechanics, relativity, and electrodynamics, but he also analyzes the "hidden life" behind Dirac's eccentric peculiarities and extreme shyness in order to try to account for Dirac's reputation as a man second only to Einstein in originality and brilliance. The biography is organized into 31 chapters, the first titled "Until August 1914" and the last two titled "On Dirac's Brain and Persona" and "Legacy." The 28 intervening chapters have titles such as "September 26–January 1927" and "January 1927–Spring 1927." The result is a seamless chronology and a personal biography that has been praised by reviewers as diverse as the physicist Peter Higgs of the Higgs boson and the playwright Tom Stoppard (Farmelo 2009, book jacket).

Another biography of a scientific life that has received acclaim is Kostas Gavroglu's 1995 book *Fritz London: A Scientific Biography*, which focuses on the life and work of a physicist who played a major role in the development of theories in quantum chemistry and low temperature physics. The biography is organized chronologically for the most part, with London's work in the application of quantum mechanics to chemistry, the theory of superconductivity, and the theory of superfluidity organized roughly by the chronology of London's years in Berlin, then in Oxford and Paris, sandwiched between the early years in Germany and the last years at Duke University in North Carolina. London was a major player in theoretical physics, but he was not a superstar like Einstein or Dirac, nor was he an institutional leader or a member of editorial boards or a writer of popular articles. In short, London's life was his scientific work, his family, and a small circle of friends.

In depicting this scientific life, Gavroglu outlines his aim as two-fold: the biographical purpose of explaining why one path rather than another was followed by London and the historiographical purpose of placing London into the perspective of what Thomas S. Kuhn called the working of normal science (Gavroglu 1995, xv). Many of the tropes of the *Bildungsroman* and the hero story are followed in the biography, although verifiable documentation does not allow Gavroglu to speculate, he writes, on the possible effects of London's early years on his later life. There are available, however, London's early philosophical essays and doctoral thesis, before he turned to physics, and these provide a "coherence in the narrative" that reveals London consistently fighting against reductionism and advocating phenomenological theories that account for behavior at the macroscopic rather than the microscopic level. "I argue," Gavroglu writes, "that there is a continuity from his thesis in philosophy to his work in superfluidity" (Gavroglu 1995, xiv–xv, 13, 118, 127, xviii).

There are struggles as well as triumphs in London's scientific life, among them his disputes over matters of interpretation and priority in quantum chemistry with Linus Pauling, John Slater and Robert Mulliken; in superconductivity with Max von Laue; and in superfluidity with Lev Landau and Laszlo Tisza. There is a magical eureka moment in 1927, which occurs in solving the problem of hydrogen bonding, although the story is recounted by London's co-author Walter Heitler rather than by London. They had begun working together in Zürich on the problem of homo-polar bonding from the perspective of Van der Waals forces, although they were aware of Heisenberg's recent paper on quantum mechanical resonance in which Heisenberg defined the exchange integral for two electrons. Talking with John Heilbron in 1963, Heitler recalled (Gavroglu 1995, 45):

> Then one day was a very disagreeable day in Zürich; [there was the] Fohn. It's a very hot south wind, and it takes people different ways . . . I had slept till very late in the morning, found I couldn't do any work at all . . . went to sleep again in the afternoon. When I woke up at five o'clock I had clearly—I still remember it as if it were yesterday—the picture before me of the two wave functions of two hydrogen molecules joined together with a plus and minus and with the exchange in it. So I was very excited, and I got up and thought it out. As soon as I was clear that the exchange did play a role, I called London up; and he came as quickly as possible.

This creative moment plays the dramatic role of an epiphany experience similar to August Kekulé's dream of a snake holding its tail (the benzene ring) or Isaac Newton's falling apple (gravitation theory) or Heisenberg himself suddenly seeing his way to a quantum mechanics without electron orbits while he was recovering from an asthma attack on the barren island of Helgoland. These stories become the myths that establish the special gift to the hero.

Gavroglu shows, too, the everyday activities of the painstaking work that constitutes normal science for scientists as they seek to solve problems both big and small. London's wife Edith told Gavroglu that kind of story. In the summer of 1934 Fritz London's brother Heinz was staying with Fritz and Edith in Oxford, where the brothers were working with the low-temperature physics group at Clarendon Laboratory. Fritz and Heinz were in the habit of working together for hours, usually during the daytime after Heinz had been in the laboratory until late at night. At the end of August they were also having long conversations at night. Edith recalled, “I was in the kitchen cooking and suddenly the upstairs door was opened by Fritz. ‘Edith, Edith come, we have it. Come up, we have it’ . . . Fritz said ‘The equations are established. We have the solution. We can explain it’” (Gavroglu 1995, 118).

Narrating the processes of scientific creativity and the confirmations and disconfirmations of experimental or theoretical results is one of the major preoccupations of biography as the scientific life, as exemplified in Gavroglu’s *London*. Among the masters of this technique was Frederic Lawrence Holmes. His 1985 study of *Lavoisier and the Chemistry of Life*, like his two-volume study of Hans Krebs (1991, 1993) and his first book of 1974 on Claude Bernard, aimed to use records of the scientist’s life in order to chart the torturous, interlocking, and unpredictable avenues by which scientific experimentation and reasoning work. Holmes’s scientific lives were dense and technically detailed narratives of scientists’ laboratory work and of what he called their “investigative pathways.” Laboratory notebooks were the essence of the story, with little mention of political, administrative, philosophical, or family preoccupations of the biographical subject (F. Holmes 1974, 1985, 1991, 1993, 2004). Holmes’s aim, like many authors of scientific biography, was to show the excitement of science, the passion for science, and the very hard work and struggles typical of science—both the Eureka moments and the Sisyphean moments of scientific life.

Michael Polanyi, who was a colleague and collaborator with London in Berlin, caught the dichotomy between epiphany and the kind of struggle that he called the “slough of despond” (Polanyi 1949), as well as the distinction between revolutionary and ordinary scientific work, in his (modestly worded) statement that (Polanyi 1969, 97):

> the example of great scientists [like Einstein] is the light which guides all workers in science, . . . we must guard against being blinded by it. There has been too much talk about the flash of discovery and this had tended to obscure the fact that discoveries, however great, can only give effect to some intrinsic potentiality of the intellectual situation in which scientists find themselves. It is easier to see this for the kind of work that I have done than it is for major discoveries.

19.5 The Life of Scientific Collaboration

Like Thomas Kuhn, Polanyi reached a point in his career when his interests turned from doing science to thinking about the philosophy of science. In Kuhn’s case, the turn came at about the time that he was completing his doctorate in solid-state physics. For Polanyi, it came after a successful career of more than 25 years in physical chemistry that included laboratory directorships in Berlin and Manchester. Both former scientists insisted that more attention should be paid by historians of science and philosophers of science to the structure and behavior of the scientific community in order to understand how scientific knowledge is achieved. A focus on scientific collaboration as biography in science is such an approach. How was a particular scientific collaboration constituted, who were its members, what were their aims, how did they go about working together, and what did they achieve? The goal is biographical, observing tropes and rules of biography, but the purpose is also more broadly to write the history of science.

For purposes of biography, scientific collaboration may be defined by a membership of two or three or of dozens or more. In *QED and the Men Who Made It*, S. S. Schweber portrays a large community in quantum mechanics, with a particular focus on the four physicists Freeman Dyson, Richard Feynman, Julian Schwinger and Sin-Itiro Tomonaga (Schweber 1994). Schweber's study of Oppenheimer and Bethe perhaps fits into the category, too, of collaborative biography since Schweber here deals with interlocking lives more than with parallel lives. Istvan Hargittai has used this format in the way that he plots his study of five Hungarian physical scientists in the book *Martians of Science*, while Lois Banner is explicit about entanglement in her book *Intertwined Lives* on Margaret Mead and Ruth Benedict (Hargittai 2008; Banner 2004).[5]

Some of the most compelling collaborations for biography are biological families in science, such as the Cassinis, the Bernoullis, the Becquerels, the Curies, the Darwins, and the Huxleys, linked together by successive generations or by sibling ties or as husband and wife (Abir-Am and Outram 1987; Pycior et al. 1996). Lauren Redniss's *Radioactive: Marie and Pierre Curie. A Tale of Love and Fallout* (2011) is an unorthodox biography of a scientific husband and wife, lavishly illustrated with the author's own artwork and interspersed with vignettes on the uses and perils of radioactive elements, but thoroughly well documented in traditional historical sources. In writing the book, Redniss says that she self-consciously ignored the advice of the Curies' granddaughter Hélène Langevin-Joliot to avoid telling the Curies' lives as a fairy tale (Redniss 2011, 187, n. 30).

John Jenkin's 2008 study of the physicist father and son William and Lawrence Bragg is an example of a more orthodox recent biography of a scientific family, as are Michael Hoskin's 2007 volume on the astronomical Herschels of Hanover (the overlapping lives of three brothers and a sister) and Deborah R. Coen's *Vienna in the Age of Uncertainty*. Coen's title does not immediately tip the reader to her richly detailed biographical study of three generations of family life and collaborative work in physics, physiology, meteorology, and animal behavior in the Viennese Exner family's scientific dynasty from the 1840s to the 1920s (Jenkin 2008; Hoskin 2007; Coen 2007). Coen's approach is considerably more rich and complex than

[5] Hargittai's five Martians are Theodore von Kármán, Leo Szilard, Eugene Wigner, John von Neumann, and Edward Teller.

simple portraits of individuals. She demonstrates the influences of early experiences, but she also shows the insufficiency of family biology or essentialism to explain the choices, values, and destinies of her major protagonists.

Another kind of collaborative biography is what Laura Otis in her 2007 book *Müller's Lab* christens "labography" (Otis 2007; Nye 2009). The labography is biography on a different scale than the prosopographical approach advocated by Steven Shapin, Arnold Thackray and Lewis Pyenson in the mid-1970s, and it is more psychological and personal than most studies of research schools (Shapin and Thackray 1974; Pyenson 1977). Otis examines the relationships between the distinguished nineteenth-century physiologist Johannes Müller, who taught in Bonn and Berlin, and seven of his students and protégés, who included Emile du Bois-Reymond, Hermann von Helmholtz, and Ernst Häckel. The labography offers detailed descriptions of daily routines of lecture halls, rooming houses, museums and hospitals, as Otis describes experiments conducted in personal apartments and public institutions. Competition between students, desire for their mentor's praise, and individual struggles to win independence from the master all fit into classical tropes of *Bildungsroman* even while the labography traces the sociology and psychology of private and professional scientific life in mid-nineteenth century Germany. Like Richard Holmes, Otis insists upon the passion of intellectual work, using metaphors of infatuation and seduction, while she also reveals the power of social bonds and personal rivalries (Otis 2007, 233–235).

A worry about biographies is that the typical traditional biography too often has concentrated on stories that are easy to tell, or, as suggested by Greene, on scientists whose lives are most easily structured as the hero's successful and victorious quest. Thus biographies of Darwin abound, while fewer biographers have tackled Mendel's life (Greene 2007, 746–750). In somewhat the same vein as Polanyi, Greene suggests that biography creates a distortion in the representation of science, because traditional biography "is not meant to construct or represent normal or average performance.... creative workers whose lives do not fit the template of the hero's quest are denied biographical treatment" (Greene 2007, 751).

What I am calling the biography of scientific collaboration is an approach to scientific life that does not distort the processes of science by overly emphasizing the role of the individual, although inevitably focusing on individuals. The biographical studies by Jenkin, Hoskin, Coen, and Otis all point to this aspect of science that often is downplayed by concentration on the single life. That aspect is made explicit in Jenkin's title for his biography of the Braggs: "the most extraordinary collaboration in science." Gavroglu does not make the claim of extra-ordinary for the collaboration of Fritz London with Walter Heitler or with Fritz's brother Heinz, but Francis Everitt gets at the importance of the fact of collaboration in his review of Gavroglu's book (Everitt 1996, 1273).

> All his life, Gavroglu notes in this biography, London had a habit of retreating into himself and 'writing a long piece to clarify the conceptual issues of a particular problem.' That being so, it is biographically fundamental that his two determinative scientific papers were collaborations, the first (1927) on quantum chemistry with Walther [sic] Heitler, the second (1935) on superconductivity with his younger brother Heinz. This introverted man needed

> other people. Even his third masterwork, the Bose-Einstein theory of superfluidity (1938), grew out of intense discussions with Laslo [sic] Tisza at one of the very few scientific conferences London attended.

Gavroglu's bibliography for London's published writings lists 67 papers and three books, one of the books co-authored with the French physicist Edmond Bauer. Among the papers, 13 were co-authored, including Fritz London's first scientific paper on the intensity of band spectra written with Helmut Hönl. While in Berlin, Fritz co-authored five papers bearing on chemistry, three of them with Harmut Kallmann, one with Robert Karl Eisenschitz, and one with Polanyi. Berlin provided a professional and social environment of weekly colloquia and research laboratories in which collaboration was especially easy or desirable, and chemistry was a discipline in which collaboration was more common than in theoretical physics. Polanyi, for example, headed research laboratories at the Kaiser Wilhelm Gesellschaft in Berlin and at Manchester University. He authored 218 scientific publications from 1910 to 1949, including one book. Of his papers, 132 were co-authored with 76 different co-authors (Wigner and Hodgkin 1977, 437–445). The study of Polanyi's or other scientists' most frequent co-authors, like research into patterns and styles of collaboration, not only can provide important insights into the intersections between the scientist's social and scientific lives but into the normal processes of science at the time (F. Holmes 2001).

Another of Greene's worries about biography lies in this very area of collaboration. As studies of the history of science push into the sciences of the twenty-first century, biographers will find that much of scientific production no longer appears as single-authored papers or jointly authored papers by just two or three authors with clearly delineated responsibilities. This situation is already true in many areas of the physical and biological sciences. "In such an environment, where a lifetime of publications by a given scientist may include only a few book reviews, review articles, and think pieces as single-author efforts, it becomes quite difficult to see how biography will find the materials to get its job done" (Greene 2007, 753–754).

The case for biography of recent scientists likely is not so bleak. Historians already are systematically working with individual scientists and with archivists and museum curators in order to supplement written records with oral histories and with the means to preserve electronic records that currently exist only in digital clouds. Projects at the American Institute of Physics, the Chemical Heritage Foundation, and the Max-Planck-Institut für Wissenschaftsgeschichte are among examples of this kind of effort, as is the Royal Society's October 2012 launch of a public and popular project for writing women into the history of science (Royal Society of London 2012). Scientists write lively and informative accounts of themselves and their work in their autobiographies, and some scientists, as Thomas Söderqvist puts it, lead a "biographical life" by saving absolutely everything (Söderqvist 2003, xviii). Biographers have always depended on scientists themselves for evaluations of what matters in science, just as biographers respond to the judgments of artists or novelists or politicians or economists about the state of things and who did what. Biographers then bring their historical tools, critical perspectives, and literary tropes to the final biographical project.

19.6 Concluding Reflections

My examples have been drawn mainly from the history of twentieth-century physical sciences and from biographies published as books. As a historian, I have written three books that qualify as biography. My books on Patrick Blackett and on Michael Polanyi fit best into the category of biographies of scientists in which the protagonist's scientific work is crucial to the life, but in which the science often takes a temporary or permanent back seat to the scientist's preoccupations and responsibilities as statesman, administrator, government advisor, military officer, political activist, economic writer, or philosopher in a wider arena outside the laboratory. In contrast to the Blackett and Polanyi books, my earlier biographical study of Jean Perrin, despite his huge and influential role in French politics, education, and scientific institutions, focused almost entirely on Perrin's scientific work in his most creative years from the mid-1890s to the outbreak of the First World War (Nye 2004, 2011, 1972). My approach to Perrin's quest for experimental proofs of what he called "molecular reality" is very much in the folk-tale vein of the heroic venture, its struggles, and triumphs, unlike a later biography by Micheline Charpentier-Morize (Charpentier-Morize 1997). In contrast, while they are in no way life biographies on the scale of Sime's *Meitner* or Cassidy's *Oppenheimer* or Isaacson's *Einstein*, my biographies of Blackett and Polanyi fit the model of *Bildungsroman* and Weberian interrogation.

The fact, as we have seen, that there are multiple biographies of a single biographical subject reminds us that Virginia Woolf had to be at least partly wrong in writing that biography is a craft and not an art. There is no truth to be discovered about the man or the woman or the collaborative group at the heart of the biographer's story, and there are as many different stories as there are different times and places and sensibilities in biographers' own lives. A recent biography that explicitly makes this point is Nicolaas Rupke's *Alexander von Humbolt: A Metabiography*, a study of how Humboldt's life was configured and reconfigured during the course of the nineteenth and twentieth centuries by his biographers (Rupke 2005). As Steven Shapin writes in his review of Rupke's book, "shifting biographical traditions make one person have many lives" (Shapin 2006).

I have concentrated on the topic of biography and history—specifically the history of science—rather than on the theme of biography and literature. In his quest to highlight what he calls the "poetics" of scientific biography, Thomas Söderqvist has written that more needs to be written by way of literary criticism of biographies of scientists, and that this would be a good thing (Söderqvist 2007, 6). Graham Farmelo's portrayal of Dirac's "hidden life" is deeply poetic and literary in tone and effect, as is Söderqvist's own approach of existentialist biography in his life of the immunologist Niels Jerne. Writes Södervist, "this is a biography of Niels Jerne, not a contribution to the history of immunology disguised as biography"

(Söderqvist 2003, xxiii). In a similar vein, Ray Monk writes of his biography of Oppenheimer: "what *most* interests me is Oppenheimer *himself*" (Monk 2013, xiv).

Biographers have many different aims and methods, and biographers of scientists often confront the problem of the highly technical aspects of their subjects' life work in scientific research. Again, Monk writes that if he is to understand Oppenheimer *himself*, Monk as Oppenheimer's biographer must "attempt to understand his contributions to science" (Monk 2013, xiv). Whether written as a life of a scientist or a scientific life or a collaborative life or something else, biography is a difficult and challenging genre, and especially so, for the historian of science with a literary sensibility.

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Chapter 20
Different Undertakings, Common Practices: Some Directions for the History of Science

Ana Simões

Abstract Having participated in collaborative projects with Kostas Gavroglu both in the realm of history of quantum chemistry and in the framework of the international group Science and Technology in the European Periphery (STEP), in this chapter I offer a brief assessment of STEP and the history of quantum chemistry undertakings to highlight ways in which the praxis informing both reveals common features. This comparative exercise is used to suggest a few directions the discipline of history of science might take into consideration in the future. These directions point to the importance of organizational and methodological pluralism and multi-perspectivity, and the promises of exploring the "in-between" character of sub-disciplines or areas of studies in both the sciences and the history of science.

Keywords STEP • Quantum chemistry • History of science • Pluralism • Multi-perspectivity

20.1 Introduction

I still remember quite clearly the occasion on which I first met Kostas Gavroglu. It was at an uncharacteristic bar at the hotel in which the 1991 History of Science Society Meeting was taking place in Madison, WI, USA. I was then at the beginning of my Ph.D. research on the history of quantum chemistry. Although I had been aware of Kostas's work on the history of low-temperature physics, it was Sam Schweber who told me about Kostas's detour into the history of quantum chemistry, fostered by his recent interest in the biography of the German physicist Fritz London. The prospect of having a senior colleague working simultaneously on the same topic of my Ph.D. dissertation was frightening, but this first meeting dissipated the worst scenarios I had come up with in the meantime. What could have been an unfriendly academic competition turned into a fruitful and nonantagonistic collaboration.

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Ever since my return to Portugal in the early 1990s, after completing my Ph.D. in the United States in 1993, we have been collaborating, on and off, accompanying the ebbs and flows in the dynamics of our respective local institutional environments, in which history of science was still an emerging field fighting for academic space, institutional support, and professional recognition. Our collaborative ventures could have been restricted to the history of quantum chemistry, a relatively peripheral, if not marginal, topic for historians of science, which has been receiving increasing, but still moderate, attention by the historical community. But that was not the case. Besides being an outstanding historian of science with wide interests mirrored in the broad thematic range covered in this volume, Kostas has been first and foremost a community builder and a charismatic leader who has played a leading role in the emergence and consolidation of the discipline of history of science not only in Greece but also in other settings. First the launching of the European Project Prometheus, then active involvement in the creation, development, and consolidation of the international group Science and Technology in the European Periphery (STEP), became part of an agenda for the development of the discipline of the history of science in contexts other than those in which the discipline originally developed and came to occupy the core of our joint activities and concerns.

Historiographic (and also philosophical) considerations have been present in all topics to which Kostas has dedicated his scholarly attention, from science in the Greek-speaking world during the Enlightenment and science in the European Periphery to history of quantum chemistry, history of low-temperature physics and artificial cold, and so on. Having participated in collaborative projects with Kostas both in the realm of history of quantum chemistry and in the framework of STEP, to such an extent that both grew in parallel but apparently independent installments, I introduce briefly here a summary of STEP undertakings and those produced in the realm of the history of quantum chemistry, to highlight ways in which the praxis informing both reveal common features.[1] This comparative exercise is used to suggest a few directions the discipline of history of science might take into consideration in the future. These directions have pointed to the importance of organizational and methodological pluralism and multi-perspectivity, and the promises of exploring the "in-between" character of sub-disciplines or areas of studies both in the sciences and in the history of science.

[1] This idea, which I have entertained for long, was reinforced following a discussion held during the meeting "Fifty years after T.S. Kuhn *Structure of Scientific Revolutions*" held at the Institute Max Planck for the History of Science in the fall of 2012.

20.2 A Gift from Prometheus: A STEP Agenda for the History of Science

The STEP group has already 15 years of existence preceded by a 5-year gestation period. Opting for careers away from the "centers" came with extra challenges for some of its founding members. The delineation of parallel agendas for the training of new generations of students and future scholars in many countries of the so-called European Periphery became part of a common project that could have better prospects of succeeding in the context of joint activities and strong networking.

This intention was undoubtedly the rationale behind a European Project launched by Kostas in 1994, at a time when the probability of a successful application for funding by the European Union in the area of humanities and social sciences was neither as bleak nor as time consuming as it has become two decades later. Purposefully called Prometheus, the project joined historians from several European countries, including Greece, Portugal, Spain, Italy, and Denmark, and gave the first collaborative steps in the study of "the Spreading of the Scientific Revolution from the countries where it originated to the countries in the Periphery of Europe, during the 17th, 18th, and 19th centuries."[2]

By looking at the circulation and appropriation of the ideas and practices stemming from the Scientific Revolution in various European countries, the project gave special emphasis to peripheral ideas and practices that were often marginalized from the international master historical narrative for a variety of reasons including, but going beyond, the mere linguistic barriers and inaccessibility of sources. The project attempted to draw a preliminary "thematic atlas" (Gavroglu 1999, viii) of the sciences in forgotten European regions, by identifying actors, institutions, and strategies, including papers, book and textbook writing, diverse popularization means, travels of learning, and other instances of circulation of knowledge. Although the thematic atlas remained an unpublished and unfulfilled venture, research gave way to an issue on the sciences in the European Periphery during the Enlightenment published in the journal *Archimedes* (Gavroglu 1999).

The involvement in Prometheus, together with the participation in the European Science Foundation (ESF) Project on "The Evolution of Chemistry in Europe, 1789–1939,"[3] with the accompanying establishment of strong personal connections among some colleagues, constituted the ground experiment for launching, a few years later, in 1999, again mostly because of Kostas's vision and stamina, the group Science and Technology in the European Periphery, which has been active ever since. From a relatively small sized group, STEP grew to include around

[2] European Community Project, Human Capital and Mobility, Scientific and Technical Cooperation Networks *Project Prometheus – The Spreading of the Scientific Revolution from the countries where it originated to the countries in the Periphery of Europe, during the seventeenth, eighteenth, and nineteenth centuries*. CHRX-CT93-0299, 1994–1996.

[3] See Chap. 5 in this volume by Agustí Nieto-Galan.

200 members from 30 different countries and four continents (Europe, North and South America, Asia, and Oceania). A considerable fraction of members comes from the so-called European Periphery, and especially from Greece, Portugal, and Spain, the countries of provenance of a substantial part of the group's founding members.

From its inception STEP has been conceived purposefully as a loosely structured group, sharing an informative website and a discussion list.[4] The group holds conferences every 2 years,[5] which were thematically oriented until 2008, to create a provision of case studies and methodological musings cementing the group's identity, amenable to comparative exercises, and giving rise to several collective volumes (Simões et al. 2003; Garcia et al. 2006; Special Issue *Nuncius* 2008; Papanelopoulou et al. 2009; Papanelopoulou and Kjærgaard 2009).[6] The rationale behind their original concatenation was lost to a more standard organizational structure, but one in which meetings include sessions on the topics at the forefront of research interest groups subsequently created within STEP. By using different methodological approaches to discuss a variety of themes, encompassing travels, textbooks, popularization of science and technology, science and technology in the press, national historiographies of science, science, and religion, universities, transnational histories, and science and gender, considered particularly attuned to detect specificities in the institutional, social, cultural, and political contexts and functions of science and technology in peripheral places, the endeavors of STEP members delineated the contours of a new historiography of science and technology in the European Periphery.

The systematic discussion of historiographic issues exploring the knowledge amassed in various case studies has heralded the attainment of another stage in the theoretical discussions by group members and in the life of STEP (Gavroglu et al. 2008; Simon and Herran 2008; Nieto-Galan 2011; Patiniotis and Gavroglu 2012; Gavroglu 2012; Patiniotis 2013; Raposo et al. 2014).

However, in a substantial way historiographic aims were at the very foundation of the STEP group. The path to the first joint theoretical paper, authored by nine colleagues, three each from Greece, Portugal, and Spain, began in a first informal gathering of a subgroup of the nine colleagues and friends, held at Kostas's house in the island of Aigina, in 2005 (Gavroglu et al. 2008). This gathering gave voice to these worries: first, to overcome the constraints of the respective local contexts often still heavily tinted by positivistic overtones, oscillating between the rhetoric

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[4] Website: http://147.156.155.104/. List: NODUS: Science and Technology in the European Periphery e-mail list NODUS@LISTSERV.UV.ES.

[5] Scientific Travels, Lisbon, Portugal 2000; Scientific and Technological Textbooks, Aigina, Greece, 2002; Traditions and realities of national historiographies of science, Aarhus, Denmark, 2004; Scientific and Technological popularization in the European Periphery, Mao, Minorca, 2006; Looking back, Stepping Forward, Istanbul, Turkey, 2008; Galway, Ireland, 2010; Corfu, Greece, 2012; Lisbon, Portugal, 2014.

[6] For an extended discussion of the STEP project and research output see also Chaps. 5 and 10 in this volume by Agustí Nieto-Galan and José Ramon Bertomeu-Sanchez.

of backwardness and the opposite rhetoric of national grandeur and hero-worshiping; and second, to bypass the dangers of parochial antiquarian approaches, in the process avoiding the fragmentation produced by a myriad of local studies, and exploring ways to frame new research endeavors within mainstream historiography.

This path has been followed by criticizing the value-ladenness associated with the centre–periphery dichotomy and the assumptions behind diffusionist models born in the context of colonial studies (Basalla 1967), which oppose creative centers to passive peripheries, distinguish between the roles of science and scientists in both types of places, and look at circulation as a mere unidirectional transfer of readymade knowledge from centers to peripheries. Critical scrutiny was followed by the suggestion to circumvent the assumptions behind historiographies of transmission by moving into a new historiography built on the concept of appropriation. The concept of appropriation, which has been itself appropriated from cultural history, calls attention to the specificities of the "receiving" culture, with its social, political, religious, and cultural specificities. In this new framework, the local agents are endowed with creative functions, and attention is paid to the ways practices are transformed when they move from one place to the other. Furthermore, appropriation draws attention to the fact that when practices "arrive" at a certain place, they are never integrated into an ideological vacuum. In contrast, they are intertwined with the multiple cultural traditions of a specific society at a particular moment of its history. New scientific discourses are articulated in the local context, legitimizing strategies and spaces are created, and resistance to the new practices usually emerges. The local peripheral context "chooses" to be influenced in certain specific ways, and choices are taken together with the rejection of various forms of influence.

In this new framework, one attempts to unravel the specificities and contours of appropriation processes, which took/take place in different peripheral contexts in different periods and for different thematic situations. By stressing various and multidirectional responses, one contributes to the international historical scene with a variety of new case studies, which enrich current views, and often revise received ones. This shift has been taking place together with criticisms of diffusionist models by postcolonial, global, and subaltern studies. However, despite emphasizing circulation and transit of knowledge as creative processes, and the innovative role of peripheries and colonial spaces and agents (Secord 2004; Chakrabarty 2007; Raj 2007; Simon and Herran 2008; Schaffer et al. 2009; Sivasundaram 2010), these critical approaches have been mainly applied to peripheral and colonial spaces associated with countries of the so-called European Center,[7] which orbit centers located in their mainland territories or as part of their colonial empires. European

[7] Since the sixteenth and seventeenth centuries, the reference to European science has usually encompassed the space enclosed by a "polygon starting in Cracow, going onto Padua and Florence, proceeding to Paris and London to Edinburgh and completed at Kracow including the Low Countries," which has come to define roughly the European Center (Gavroglu 2012, 311–312).

peripheral countries and their colonies have regularly remained in the shadow as a result of language impediments and the difficulty of access to historical sources, among other reasons.

Furthermore, and without eliminating asymmetries between different European peripheral localities, one should be able to highlight similarities, not differences, among the various peripheral contexts to unveil common trends.[8] This novel enterprise is oriented towards the writing of a historical narrative, which will concur to the emergence and structuring of a concept of periphery, beyond the centre–periphery traditional dichotomy with its associated value judgments, and based on the awareness of the dynamics of the historical co-construction of both centres and peripheries. In articulating the characteristics of peripheries, a preliminary assessment, to be complemented by additional substantiations, points to a considerable weight given to the political rather than the social in the criteria for choosing to appropriate a particular theoretical corpus or set of practices; on the predominance of personal networking when contrasted with institutional backing; on the immediacy of applications, often viewed as a kind of quick fix; on the fluidity of institutional structures, nonexistent at times in extreme cases; on the blurring of dichotomies, in the sense that the same individual may perform different tasks usually associated with the scientist, teacher, or popularizer; and on the rhetoric of modernization (Gavroglu et al. 2008, 169).

The redefinition of the concept of periphery should be mainly operational in the sense of enabling historians to move from the perspective of the centre to the perspective of the periphery. In this sense, Science and Technology in the European Periphery is taken to be a historical problem whereas the European Periphery becomes a historical actor. By raising such issues, while using analytical tools and historiographic concepts stemming from the mainstream international historiography of science, STEP has been able to (re)examine old themes, pose new questions, and critically challenge hegemonic historical accounts.

By realizing that the traditional centre–periphery dichotomy raises as many problems in treating the periphery as in treating the centre, one should be driven not just to rethink the periphery (the margins or the fringes) but also to rethink the centre. In fact, the new vantage point provided by the perspective of the periphery may help to reassess extant historical narratives of core regions or countries. I still remember quite clearly that in the talk delivered in the 6th STEP meeting, which took place in Istanbul in 2008, David Edgerton cautioned about how misleading it was to take the Cavendish Laboratory as exemplary of the organization and aims of universities at the end of the nineteenth century, when the idea of a research

[8] Traditionally, a subgroup of comparative reception studies has been concerned with accounting either for the *differences* between centers and peripheries or between peripheries. Although there are not many comparative studies written by "peripheral" authors, impressionistic comments abound, oscillating between a hagiographic type and the rhetoric of backwardness or decadence. In turn, the accounts about peripheries built up by historians of the so-called centers tend to assess peripheries using criteria stemming from the centre, thereby overlooking the creative role of peripheries.

university was still more a promise than a reality, to such an extent that the vantage point provided by university contexts located in the European Periphery offered a picture closer to end-of-the-century academic norms than the singularity of the Cavendish Laboratory.

Overall, STEP has contributed to the ongoing debates on the various difficulties which have hampered a systematic study of the sciences and technology in the European Periphery, and has contributed to historicize the notion of European science, pointing to its multifarious contexts, often disregarded but fundamental to understand its richness, built on diversity and heterogeneity, not on unity and homogeneity. It offers a privileged standpoint to unveil the dynamics of the hidden agendas behind Europeanization, the parallel processes of Europeanization of the world and of provincializing Europe, and the role of both as privileged standpoints to illuminate and deconstruct the notion of European science and technology, in the sense of enlightening the process of emergence of science and technology as a global phenomenon, and as one of the main building blocks of the construction of an imagined, much more than real, European intellectual identity.

Much is still to be gained if STEP studies (if I may call them so), global studies, postcolonial studies, and transnational studies will converse with each other, exchanging points of view and testing common concepts and similar methods. The multi-perspectivity arising from looking at the same (or similar) problems from different angles offers a promise worth pursuing.

Certainly many scholarly ventures, put forward on an individual or on a collective basis, including STEP, are already delivering results, which supplement and enrich the mainstream history of science. In many ways they materialize the optimism voiced in the leaflet presenting the rationale behind the Prometheus project or in the preface to the *Archimedes* issue already mentioned.

> Europe is presently in the throes of its most dramatic transformations since the end of the Second World War. New nations-states come into being, new borders emerge, new institutions appear, and old institutions restructure themselves. Many historians and other scholars will look again at the past in the light of current changes. The work that has already been done, as well as newly available sources, combine with (comparatively) open intellectual environments and increases in funding for trans-national contacts to offer an unprecedented opportunity for a critical re-examination of the historical character of European science (Gavroglu 1999, viii).

Europe is still in the throes of dramatic transformations, but now for the wrong reasons: financial and economic crises, the commodification of knowledge, and the entrepreneurial mode raging over universities and higher education institutions, together with increasing budget cuts for research and teaching, especially in the countries of the southern European Periphery, and the assault on the social sciences and humanities underlying recent European orientations, have turned the optimism of the mid- and late-1990s into very pessimist prospects. Let us hope that all that has been accomplished already in the "critical re-examination of the historical character of European science," able to reassess its purported homogeneity, dependent on a few selected core countries and scientific centers, will continue to enliven scholarly accomplishments and debates.

20.3 Stepping into Quantum Chemistry: An Identity Erected on Diversity

There are many ways in which the praxis informing both STEP undertakings and those produced in the realm of the history of quantum chemistry reveal common features.[9] One is the choice and exploration of relatively marginal topics within the mainstream history of science, contributing to thematic variety and new vantage points to look at new and old problems. Another is the emphasis on collaborative works, based on extensive and open discussions across time and space, as well as passionate but friendly exchange of points of view. This emphasis is a novelty in the research practice typical of the humanities in general, and the history of science in particular, that is spreading gradually but steadily: its success hopefully announces a new culture for doing history of science that is less individualistic and more communal. Still another is the exploration of similar topics in both instances, such as the role of textbooks, and popularization issues, the diversity of scientific cultures and their dependence on specific contexts (institutional, social, political, cultural, and scientific), the role of styles for doing science, the multiple roles often played by actors, including the simultaneous practice of research, teaching, and popularization strategies, and the role of controversies, to give a few examples. More importantly, these topics include the exploration of a similar theoretical apparatus for the history of science and an emphasis on historiographic reflections. Together with historiographic considerations, concepts such as appropriation, legitimization, resistance, networking, and circulation have been the backbones of both historical enterprises.

It was most probably Fritz London who brought Kostas into the history of quantum chemistry. London traveled an unusual path, starting his career in philosophy and specifically in phenomenology with Edmund Husserl, and moving afterwards into quantum mechanics. Neglected by historians of physics who had been attracted by the young and brilliant revolutionaries who created this most intriguing field, Kostas showed how London's immersion in quantum mechanics revealed the promises of "normal science" in Thomas Kuhn's terminology (Gavroglu 1995). For the shy and perceptive London, it acted as an open window to new research fields, which came to be known as quantum chemistry, on the one hand, and superconductivity and superfluidity, on the other, and which London explored often in collaborative works. In one instance, London found the quantum mechanical explanation of the covalent bond and its dependence on spin in a joint paper often heralded as the beginning of quantum chemistry, and proceeded to apply group theory to quantum chemistry, not very successfully for a variety of scientific, methodological, and political reasons, including the devastating effects of the onset

[9] Despite contributions to the history of quantum chemistry by a growing number of scholars, including A. Karachalios, H. Kragh, V. Mosini, M.J. Nye, J. James, S.S. Schweber, and B.S. Park, having in mind the purpose of this paper, in what follows I briefly recall a few main steps in the history of quantum chemistry as developed by Kostas, individually or in joint collaborations.

of Nazism and of WWII with its terribly disruptive forced migrations. In the other, he showed how superfluity was a property arising from what could be named as a macroscopic quantum phenomenon. In both cases, physical research impinged on the philosophical issue of reductionism.

Biography, a genre arousing mixed feelings among historians of science, led Kostas to tackle London's contributions to quantum chemistry in the wider context of the early years of disciplinary development in various national contexts with their differing cultural approaches and scientific practices.

At least during the first 50 years of the development of quantum chemistry, before the implications of computers in subverting the whole disciplinary rationale were explored to the fullest, the constraints imposed on the actions of participants, be they physicists, chemists, applied mathematicians, or a hybrid sort of scientist, can be tied to the varying ways actors handled, often implicitly, the reductionist dictum of P.A.M. Dirac Dirac 1929: this stated that after the theory of quantum mechanics is "almost complete," "the underlying physical laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known, and the difficulty is only that the exact application of these laws leads to equations much too complicated to be soluble" (Dirac 1929, 714). Summoning Greek mythology, Kostas has defined the situation in very expressive terms in what follows:

> Practitioners of many (sub)disciplines in the sciences are, at times, confronted with an apparent bliss that often turns into a nightmare: they are stuck with too good and too fertile a theory. So-called normal science is surely a rewarding practice, but for that very reason it may, at times, also become boring. Theories or theoretical schemata may make successful predictions, may clarify 'mechanisms,' they may show the way to further developments, and they may be amenable to noncontroversial approximations. If one is really lucky, they may even, at least in principle, be able to answer all questions. There have been many such theories, especially in the history of physics. Laplacian physics, ether physics, or superstrings have historically defined the frameworks for such utopias where everything could be answerable, at least in principle. But one is truly at a loss when one is confronted with this *in principle*. In principle, but not in practice? In principle but never? Confronted with the deadlocks that are implicit in such utopias, scientists started to collectively display a Procrustean psychopathology. They would prepare the beds and, yet, the theories would manage to trick the tricksters: almost all theories appeared to be fitting to any Procrustean bed. They were short and tall and normal at the same time (Gavroglu 2000, 429).

In looking for the 'proper' applications of quantum mechanics to chemical problems, the specification of the meaning of 'proper' by each participant in each cultural context became central to building the identity of quantum chemistry. It became also the ground for the historical assessment of the dependence of the philosophical issue of reductionism on the cultural specificities of practitioners' scientific environments, in the sense that different practitioners with different cultural upbringings and preferences manifest very different attitudes towards what physicists, following Dirac's indictment, have often considered as reductionist claims vis-à-vis chemistry.

During the process of delineating a new “in-between” discipline such as quantum chemistry, born at the interface of physics and chemistry, those who contributed to its becoming did not only devise and appropriate ideas, techniques, and practices, but contextualised philosophical problems to make additional differentiations with respect to the “parent disciplines” of physics and chemistry. The whole problem of reductionism seems to be on a totally different footing when discussed within the context of (quantum) chemistry than when discussed within physics. For that reason it may be interesting to have more disciplinary histories and test the extent to which they may be useful probes for revealing different contextualizations of issues in the philosophy of science (Chang et al. 2013).

The contrast between the theoretical assumptions and methodological guidelines underlying the practice of those who participated in the emergence of quantum chemistry, exemplified on the one hand by the contributions of German physicists such as London, Walter Heitler, Erich Hückel, Hans Hellman, and Friedrich Hund, and on the other by the contributions of American scientists with a mixed background in physics and chemistry, including physical chemistry and molecular physics, such as Linus Pauling, Robert Sanderson Mulliken, J.H. Van Vleck, and J.C. Slater, evidences not so much the importance of national styles, but the impact of different educational backgrounds and institutional frameworks in bringing about different cultural specificities and philosophical preferences for doing what came to be known as quantum chemistry. The former gave flesh and blood to the “promises and deadlocks of using first principles” (Gavroglu and Simões 2012, 9), permeated by a reductionist culture dear to physicists, and the latter revealed an insatiable appeal for “rules, and more rules” (Gavroglu and Simões 2012, 39), including the recourse to empirical approximations and parameters, guided by a pragmatic and operational outlook for which results justified means, and which proved immensely successful in building the constitutive character of a new form of theoretical chemistry. At the core of the whole enterprise was the exploration of alternative routes envisioning differently the positioning of the subdiscipline of quantum chemistry vis-à-vis the neighboring disciplines of physics and chemistry, in the process carving its relative autonomy and shaping its “in-between” character.

The ways in which the appropriation of the mathematical apparatus of quantum mechanics took place informed “approximation methods and crunching numbers” (Gavroglu and Simões 2012, 131), before and after computers entered quantum chemistry. The legitimization of the intensive use of mathematics by C.A. Coulson and his group went in parallel with Coulson’s plea for the importance of mathematics as a midwife for conceptual insightfulness, much more than for calculational wizardry and results. In the end, it helped to change, slowly but steadily, the uneasy relationship of chemists with mathematics from a problematic into a promising connection.

Computers entered into quantum chemistry like a tornado, perverting former disciplinary directives, theoretical constraints, and methodological assumptions, and forcing their reassessment. Computers became "challenging objects" (Renn 2012) in various ways. They acted initially as nonhuman mediators between members of distant research groups competing for computer access; they became central elements in the reevaluation of past cultures for doing quantum chemistry and helped mold new ones, exploring the long-awaited miracles of ab initio approaches but keeping also space for semi-empirical angles; they were central to the reevaluation of the role of experiment in quantum chemistry, by giving rise to the notion of virtual experiments in novel spaces christened as mathematical laboratories; they helped to turn theoretical quantum chemistry into a quasi-laboratory science; and they accompanied the consolidation of networking practices and circulatory trends among different groups to such an extent that quantum chemistry became a truly internationalized and globalized discipline.

The diversity of cultures of applied mathematics which appeared in the process, defining themselves variously vis-à-vis the potentialities of increasingly sophisticated computational means, still awaits further research, but it is already clear how some participants, the most enthusiastic and outspoken of which were Coulson and Löwdin, preached a multicultural practice in many forums (Gavroglu and Simões 2012; Simões and Gavroglu 2013). All along the history of quantum chemistry, diversity was cherished by practitioners, be it in the form of parallel developments in valence bond or molecular orbital, ab initio or semi-empirical approaches, probably with the sole exception of Pauling whose view, entirely based on the valence bond approach as the only adequate underpinning of the structural theory of chemistry, was never abandoned nor permitted any intrusions or amalgamations.

For Coulson, the advocacy of diversity of styles as a core characteristic of quantum chemistry, and perhaps, as a fundamental component of "in-between" disciplines, was illustrated by the cable metaphor. In the same manner that "the strength of an artificial fibre depends on the degree of cross-linking between the different chains of individual atoms," he considered that "the validity of the scientist's account depends on the degree of interlocking between its elements" (Coulson 1953, 37). Following Coulson's footsteps, one might argue that the explanatory success of quantum chemistry throughout successive developmental stages rested on the degree of interlocking among constitutive elements—chemical concepts, mathematical notions, numerical and computational methods, pictorial representations, experimental measurements, and virtual experiments—to such an extent that it was not the relative contribution of each component that mattered, but the way in which the whole was reinforced by the cross-linking and cross-fertilization of all elements. Furthermore, its success depended not only on epistemological but also on social aspects of this cross-fertilization. It involved the establishment and permanent negotiation of alliances among members of a progressively more international community of practitioners, intense networking, and adjustments and readjustments within the community, both at the individual and institutional and at the educational level: in short, it involved a gigantic rearrangement in the culture of quantum chemistry.

The success of quantum chemistry arose from an acquired ideology to accommodate a confluence of diverging trends and reasoning styles. No wonder that even while expanding its domain to big molecules and macromolecules, practitioners recalled van Vleck and A. Sherman's old contrast between the optimists and the pessimists (Van Vleck and Sherman 1935) and their plea for a middle-ground attitude. In the end, the changing character of quantum chemistry became its more permanent defining trait, and diversity of styles became one of its identity hallmarks.

In the book *Neither Physics nor Chemistry* we suggested to delve into the in-between-ness of quantum chemistry by looking at what we called six clusters of issues: the epistemic content of quantum chemistry, the social issues involved in disciplinary emergence, the contingent character of its various developments, the dramatic changes brought about by the digital computer, the philosophical issues related to the work of almost all the protagonists, and the importance of styles of reasoning in assessing different approaches to quantum chemistry. These issues were the six interrelated strands of our narrative to discuss the particularities of the evolving articulations and rearticulations of quantum chemistry with chemistry, physics, mathematics, and biology, as well as its institutional positioning. Here, I want just to point out how the "in-between" character of quantum chemistry favored its action as a donor subdiscipline, promoting the appearance of other subdisciplines such as quantum biochemistry, quantum biology (and to a lesser extent quantum pharmacology), as well as molecular engineering, materials science, and engineering, and last but not the least, computational chemistry and computer sciences. In some instances, it opened the way to a new scenario that centered on the amalgamation of former disciplines, convening experts from different areas in looking at the same problems and bringing in their respective points of view and expert opinions, in what has been called the "new disciplinarity" (Marcovich and Shinn 2011). Finally, it also provided the historical materials and reflections for many incursions into the emerging new area of philosophy of chemistry.[10]

This necessarily sketchy overview of some aspects of the history of quantum chemistry has basically covered the first 50 years of disciplinary development. Much is still to be explored regarding the extended and extensive impact of computers in quantum chemistry, especially after the 1970s, and specifically how computational sciences and computational chemistry interacted and reshaped quantum chemistry.[11] For this, of course, historians of science require an in-depth knowledge of late twentieth-century science, if they want to dive in these waters and not stop at the surface of problems. Again, diversity of backgrounds and diversity of perspectives may shake hands and work together.

[10] Scholars who have contributed to this area include D. Baird, D.A. Bantz, P.A. Bogaard, J. van Brakel, Marta Harris, R.F. Hendry, L.C. McIntyre, N. Psarros, J.L. Ramsey, J. Schummer, E.R. Scerri, H. Vermeeren, G.K. Vermulapalli, S.J. Weininger, A.I. Woody, and R.J. Woolley.

[11] See the contribution to this volume by Sam Schweber.

20.4 As a Way of Conclusion: Musings on the Future of History of Science

Choice of topics and methodological apparatuses reflect historians' options and creeds, but they also reflect the ways they envision disciplinary development and, when possible, how they may choose to actively shape it. The foregoing unconventional joint reflective exercise on both STEP and history of quantum chemistry was, thus, put forward to substantiate a few suggestions that history of science as a discipline may consider in articulating its future development informed by the practice of both STEP and the history of quantum chemistry.

The accommodation of diverse communities of practitioners from various cultural backgrounds in the context of increasing globalization and the concomitant exploration of the potentialities of the computer in what we may call the digital humanities (Renn 2012) offer the promise of a more global, but at the same time more democratic, community.

In fact, one of the most striking changes that has been taking place in the discipline of history of science in the past decades, and which will tend hopefully to increase, if budget cuts and decrease in funding do not put a halt to this process, has to do with the parallel processes of professionalization and internationalization of communities located away from well-established communities such as those in the UK, France, Germany, the Low Countries, or in the United States of America. This process has encompassed communities in many countries of the European Periphery as well as countries in Central and South America, Asia, Oceania, and to a lesser extent Africa. The study of how the discipline has been developing in these various places will enable us to put in perspective, both in practical and organizational terms as well as in what relates to methodological and theoretical orientations, developments associated with the so-called European Center and the United States of America, which are often taken to be exemplary. I have in mind questions such as the background and training of future historians of science, the location of these communities within local university systems, their positioning vis-à-vis scientists and social scientists, their struggle for sources of financing, and their relationship to other related communities such as those of historians of technology, philosophers of science, science and technology studies scholars, and so on. Furthermore, it will enable enrichment of scholarly debates with various case studies stemming from other locations, by using different methodological apparatuses, and often contributing to revise former historiographic perspectives, introducing new ones, on the way to a global history of science.

Simultaneously, much in the same way as the computer changed drastically the organization and practice of the community of quantum chemists, computers and computer technology applied to the history of science are impacting in substantive ways on the daily life of historians. In 20 years' time, the scenario has been dramatically altered: communications between colleagues are easier than ever, the Internet offers growing access to printed and archival materials, there is increasing sensitivity to the importance of housing repositories of materials, including books, manuscripts, and collections of instruments, and the exploration of new e-research tools promises to create an interactive framework for a digital integration of sources and their interpretations which may become at the core of a new vision for the digital humanities with the potential to restructure the way that geographically distant communities of historians of science interact with each other. Indeed, this virtual world, often at the distance of a click, may remain in the realm of utopias if open-access environments do not become truly open and free of charges for everybody everywhere. Let us be reminded of the wise words voiced in the early 1970s by the French quantum chemist Alberte Pullman when musing over the impact of computers on the fulfilment of ab initio dreams in quantum chemistry. As a consequence of the development of techniques to study all valence electrons, and by extension all electrons in molecular systems, the split between the two groups of quantum chemists predicted by Coulson at the 1959 Boulder conference—the ab initionists and the a posteriorists—was to give way to the merging of both groups into one single group, which she named as the "ab initio for everybody." She feared, however, that "the only division that will persist between quantum chemists will be … that between wealthy and poor, those who have the means to carry sophisticated calculations and those that do not have them" (Pullman 1971, 14).

The various trends and heated debates which confronted the history of science in the past decades turned around several opposite choices, from an emphasis on content and conceptual development to the contextual situational character of the scientific enterprise, from a focus on place to a concern for circulation, from a history of science geographically situated in selected places and continents to a more global one, from a more narrowly defined history of science to a more encompassing history of knowledge. These trends also involved the establishment of privileged ties, of linking and delinking with neighbouring disciplines, varying in time and encompassing the philosophy of science, sociology of science, and science and technology studies, as well as many other disciplines in the social sciences and humanities, and in the sciences, to such an extent as shaping the "in-between" character of the discipline of the history of science. The discussion of such ongoing issues may profitably take place under the guidance of methodological pluralism and multi-perspectivity, much in the same way they have guided the practice of quantum chemists and have opened new avenues in the framework of STEP. Diversity, variety, and alternative points of view may counterbalance a too-narrow focus on fashionable tendencies, which may stifle creative momentum and hinder original proposals.

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Part V
Beyond History of Science: Mathematics, Technology and Contemporary Issues

Chapter 21
The Meaning of *Hypostasis* in Diophantus' *Arithmetica*

Jean Christianidis

Abstract Historians of ancient philosophy and theological writers often come up against the puzzling issue of understanding the meaning of the term *hypostasis* used by different ancient authors. One could hardly expect that the same issue would be of interest for historians of ancient mathematics. Indeed, altogether absent from the works of Euclid, Archimedes, and Apollonius, and scarcely appearing in a nonmathematical context in the works of Heron and Nicomachus, the term *hypostasis* and its cognates appear 127 times in the six books of Diophantus' *Arithmetica* preserved in Greek. This chapter examines Diophantus' use of the term *hypostasis* and argues in favour of interpreting it as a term for numbers qua specific, individual entities. It is composed of three parts. The first part discusses the different statuses of numbers in a worked-out problem according to Diophantus' general method, and the relevant issue of the Diophantine conception of an arithmetical problem; the second part investigates all instances of the term within Diophantus' text; and the third part surveys briefly the testimonies of the Byzantine commentators of the *Arithmetica*, which provide further evidence supporting the interpretation proposed in this paper.

Keywords Diophantus • Hypostasis • Greek mathematics

One of the classical works of Greek mathematical literature, Diophantus' *Arithmetica*, has never ceased to fascinate scholars, whether as mathematicians, especially in the past, they were seeking the source of their inspiration in its pages, or as historians of mathematics in our time, they have been trying to understand its author's mode of thinking, to recognize the intellectual traditions that influenced his thought, and to evaluate its impact. The increased recent interest in Diophantus demonstrates that there are still issues to be settled with regard to his undertaking. This chapter examines Diophantus' remarkably frequent use of the term *hypostasis* and argues in favour of interpreting it as a term for numbers qua specific, individual entities.

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21.1 The Status of Numbers in Diophantus and the Notion of the Arithmetical Problem

The *Arithmetica* comprises 13 books, only 6 of which survive in the Greek language, with 4 more in their medieval Arabic translation.[1] The treatise opens with a lengthy preface, written as a letter addressed to a certain Dionysius, to whom Diophantus describes the most important stages of his method for solving arithmetical problems. The structure, technical vocabulary, and heuristic prerequisites of this method have been studied in a number of recent publications,[2] so it is not necessary to repeat the conclusions of these publications. For my purpose here, a brief summary of the essence of Diophantus' methodology will suffice.

In a typical Diophantine solution, sought-after numbers are named, given numbers are assigned specific numerical values, and operations are performed on the given and named numbers according to the prescriptions of the problem. Through these processes, the problem is gradually converted into an equation, which, when completed, must be expressed in the artificial language of the named numbers, in contrast to the initial problem, which was formulated in the common Greek language. The equation is then simplified and solved. Finally, the numerical values of all sought-after numbers are calculated, thus solving the problem, followed by verification that the numbers found "do the problem." This method of solving problems has been called algebraic since medieval Islam.

Diophantus begins the presentation of his method by discussing the different statuses of numbers. He organizes his discussion into three levels. He refers first to the realm of "all numbers" (*pantas tous arithmous*) (2.14–15),[3] the boundless field of numbers considered solely from the aspect of their common property of being made up of some amount of units. Next, he remarks that the whole realm of numbers can be divided into domains, not necessarily disjoint from one another. So he separates them into classes such as squares (*tetragônoi*), cubes (*kyboi*), sides (*pleurai*), numbers made by squares multiplied by themselves, numbers made by squares multiplied by the cubes of the same side, and so on (2.17–4.7). In addition to these, one can distinguish other kinds of numbers as well, such as primes, evens, and perfect numbers. For Diophantus' purposes, however, only the kinds just mentioned are of interest. Note that the catalogue of kinds starts with squares, because numbers that are simply numbers do not delimit a specific class within the realm of "all numbers."

It is interesting that, up to this point, Diophantus' text greatly resembles the first part of the mathematical passage in Plato's *Theaetetus* (147d ff.), in which

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[1] For the Greek books, see Tannery (1893–1895) and Allard (1980), and for the Arabic books see Sesiano (1982) and Rashed (1984). My analysis is based on the Greek books. Thus, all references to problems from books IV–VI refer to the corresponding Greek books.

[2] Christianidis (2007); Bernard and Christianidis (2012); Christianidis and Oaks (2013).

[3] Quotations from the *Arithmetica* without mentioning the editor refer to Tannery's edition.

Theaetetus describes his own procedure for studying lines and numbers. For the sake of brevity, I quote below how Jacob Klein summarizes that part:

> Theaetetus begins by showing that some square magnitudes to which certain numbers (of units of measurement) correspond have sides that, by themselves, cannot be measured by numbers (of those units) and are therefore called *dynameis*. Because "such roots [and also the corresponding square numbers] appear to be unlimited in multitude," he tries to "gather them into one," so that he may designate them "all" properly. For this purpose he divides "the whole realm of number" (*ton arithmon panta*) into two domains: to one of these belong all those numbers which may arise from a number when it is multiplied by itself . . ., to the other, all those which may arise from the multiplication of one number with another. The first number domain he calls "square," the second "promecic" or "heteromecic" (oblong), designations which occur in all later arithmetical presentations. Thus two *eide* are indeed given that allow us to articulate and delimit a realm of numbers previously incomprehensible because unlimited. (Klein 1992, 55)

The similarity of the *modus operandi* described by Plato with that of Diophantus is evident. It lies not only in the fact that the two texts speak about the same process of delimiting kinds within the genus of numbers (the kind of squares being identified in both), but they sometimes exhibit even the same wording. Thus, the generic notion of number is rendered in both by the expression "all numbers:" *ton arithmon panta* in Plato and *pantas tous arithmous* in Diophantus.

It is very important here to notice that *immediately after* Diophantus sets out the kinds of numbers, but *before* he moves on to the discussion of numbers as individual entities that enter solutions, he interrupts his exploration of the field of numbers and interposes in his text a short paragraph about how arithmetical problems are formulated. He writes: "It is from the addition, subtraction, or multiplication *of these* <numbers> or from the ratio which they bear one to another or to their own sides that a host of arithmetical problems might be formed" (4.7–10; my emphasis). At first glance, the insertion of a passage describing the enunciations of problems in the midst of a discussion about numbers should not surprise us. After all, the problems Diophantus handles are arithmetical problems. However, placement of the passage immediately after the presentation of the *kinds of numbers* and before the discussion of each number as an *individual entity* is neither accidental nor without importance. On the contrary, the situation of the passage within the economy of the entire discussion about the statuses of numbers indicates what sort of 'numbers' the specifications describe in enunciating the problems and thus provides a key to the correct understanding of the enunciation of the arithmetical problems. I return to this point later.

After delineating the classes of numbers and indicating, in discussing the enunciations of problems, the capacity of numbers to take part in arithmetical operations, Diophantus writes, with reference to *each* number individually: "It is confirmed that *each of these numbers*, after receiving an abbreviated name, constitutes an element of the arithmetical theory" (4.12–14; my emphasis). This phrase opens the discussion of numbers as particulars. Each particular number from the aforementioned classes, says Diophantus, must be designated by name to be susceptible to arithmetical treatment in the context of what he calls "arithmetical theory." And the text continues by describing the designations to be assigned, starting with the designation *dynamis* assigned to each number of the first kind, the squares. The other designations are *kybos*, *dynamodynamis*, *dynamokybos*, *kybokybos*, the corresponding fractional parts, and, finally, the *arithmos*, the designation applied to any number not belonging to the other classes. These terms are introduced as designations that can only be applied to individuals, on condition that the class membership of each individual number (i.e., of being a square, a cube, a square multiplied by itself, and so on) is always taken into account. In this way, the designations reflect the kind to which each individual number belongs. Later Diophantus adds more: that the designations also reflect an operational character. So, the designation *dynamis* means that the number in question is one member of the class of square numbers, but it is also the product of multiplying an *arithmos* by itself. Similarly, the designation *dynamokybos* refers to one entity from the class comprising the numbers resulting from the multiplication of squares by cubes from the same side; but at the same time, the number so designated is produced when a *dynamis* is multiplied by the *kybos* from the same root, or by a *dynamodynamis* multiplied by its root (*arithmos*). It should be understood, however, that the operational character of the named numbers is made possible only because, besides referring to the defining property of the corresponding class, *the names are also applied to individuals*; therefore, they signify the multitude of units constituting each number as an individual entity. It is only because the names are assigned to single numbers that operations can be performed on the names. Thus, although the power to take part in arithmetical operations is in the nature of numbers, in the *actus exercitus* operations can only be performed by individual numbers. Each name, being assigned to an individual number, refers to the amount of units that this particular number contains; hence, it is susceptible to arithmetical operations.

From the foregoing discussion, we can now understand why Diophantus uses the plural in that part of the preface concerned with numbers in terms of their *eidetic* qualification, while using the singular when discussing numbers in terms of the names-to-be-assigned. In the former context, the words square, cube, etc. refer to numbers qua numbers that share the common property defining the corresponding class; in the latter, the words *dynamis*, *kybos*, etc. refer to individual numbers within the corresponding classes, and designate each time the specific multitude of units that *makes up every single number*. This distinction leads us to the conclusion that terms denoting numbers, when used in the enunciation of problems are, grammatically speaking, common nouns, whereas terms assigned as names in the course of the solution function grammatically as proper nouns. Indeed, regarding numbers entered as requested numbers in the enunciation of a Diophantine problem, only the fact that they are of such-and-such a kind and are amenable to such-and-such an operation with one another makes them objects of attention. Therefore, the determinateness of the sought-after numbers that participate in the enunciations is not a result of the fact that each number is composed of a certain amount of units (or is assumed to be), but is the result of some *eidetic* and relational properties of the numbers.

In this respect, we understand that the Diophantine conception of a problem is comparable to that which Proclus ascribes to Zenodotus, an otherwise unknown mathematician of the early (?) Hellenistic period, and which originated from the school of Oenopides. Indeed, in the famous passage from his *Commentary* on

Euclid, in which he discusses views expressed in the past on classifying geometric propositions into theorems and problems, Proclus writes:

> On the other hand, the followers of Zenodotus, who belonged to the succession of Oenopides, and was among the disciples of Andrôn, used to distinguish theorem from problem in the sense that a theorem seeks what is the property (*ti esti to symptôma*) predicated of its matter (*hylê*), whereas the problem <seeks> of what being something is (*tinos ontos ti estin*). Hence, the followers of Posidonius similarly defined the one as a proposition according to which one seeks whether something is (*ei estin*) <such-and-such> or is not (*mê <estin>*) <such-and-such>, the other a proposition in which one seeks what is (*ti estin*) <such-and-such> or what sort of thing is (*poion ti <estin>*) <such-and-such>. (Friedlein 1873, 80.15–22)

Arguably, Diophantus' problems fit this description. In a Diophantine problem, a twofold determination is proposed for the numbers sought, with information given about their kind as well as the operations and relationships to which they are subjected, and another type of determination is required, namely, the concrete multitude of units that makes up each number. The former is a determination bearing on the properties attributed to the numbers. The latter is a determination that specifies the numbers which possess these properties. This being the case, the whole point of a Diophantine problem can be stated as follows: how to get quantitatively determinate numbers when given a combination of qualitative determinations to which they are subjected; in other words, how a combination of qualitative determinations of numbers results in quantitatively determinate numbers that "do the problem."

21.2 *Hypostasis* as a Term Referring to Numbers as Individuals

The foregoing conception of a problem helps us to elucidate the puzzling sentence from the opening paragraph of the preface, in which Diophantus describes his task as aiming "to hypostasize (*hypostêsai*) the nature (*physis*) and power (*dynamis*) of the numbers" (2.6–7). *Physis* and *dynamis* are terms by which Diophantus refers to numbers as a generic category. *Physis* and *dynamis*, however, are hypostasized through specific numbers. Indeed, the *nature* (of being amounts of units) and *power* (of being of one kind or another, and capable of being subjected to one or the other arithmetical operation) that characterize all numbers can be disclosed only through particular numbers. The term in the foregoing sentence that conveys the meaning of transition from generic to particular is *hypostêsai*, the active meaning of which includes bringing some reality to the surface or into existence.[4]

[4] See Smith (1994, p. 33). Ancient Greek literature provides a variety of usages of the verb *hyphistêmi*, both nontechnical and technical (especially philosophical and theological). The range of its meanings, according to the Liddel and Scott *Greek-English lexicon*, include 'give substance to', 'cause to subsist', 'treat as subsisting', 'subsist', and 'exist' (A 4, B IV.2). The foregoing meanings are given also by the *Patristic Greek lexicon* of G.W.H. Lampe, which also

We are thus led to the verb *hyphistêmi* and the corresponding noun *hypostasis* of which Diophantus makes such ample use in the *Arithmetica*. The verb occurs twice, always in the active voice: first in the preface in the sentence just discussed, and second in problem IV.39: "We have posited (*hypestêmen*)[5] the median <number> '1 *arithmos*, 2 units', the lesser '2 units wanting 1 *arithmos*', and the greater '7 *arithmoi*, 2 units'" (304.19–20). As mentioned earlier, the names '$1x+2$', '$2-1x$', and '$7x+2$' in this case signify the amounts of units of which the three named unknown numbers are posited to be composed. They refer to the underlying numerical values of the three numbers, which, although still unknown, are nevertheless considered to have been specified, and can thus be treated arithmetically. Therefore, the context within which the verb is used is that of the numbers considered as specific, individual entities.

The noun *hypostasis* belongs to the same family as the verb *hyphistêmi*.[6] Broadly speaking, *hypostasis* has the meaning of a 'real entity,' that is, something that really *is*, as opposed, for example, to something that is merely apparent, or a pure contrivance of thought. So to be something, a *hypostasis* must have a definite identity and possess singularity and presence of some sort. Thus, according to the Liddell and Scott *Greek-English lexicon*, the range of meanings of *hypostasis* includes 'substance,' 'actual existence,' and 'reality,'[7] and, according to Lampe's *Patristic Lexicon*, 'particular,' 'concrete entity,' and 'individual.'[8] In the same spirit, the *Oxford Dictionary of Byzantium* gives the first definition of *hypostasis* as "an ancient term used by philosophers and scientists primarily to designate individual or real existence."[9]

The word *hypostasis* appears 127 times in the Greek books of Diophantus' *Arithmetica*. This frequency is remarkable for at least two reasons: first, because *hypostasis* is an uncommon term in Greek mathematical literature; and second, because, in sharp contrast with the Greek books, the term is virtually absent from the four books of the *Arithmetica* in Arabic.[10] The latter, a fact noteworthy in itself, must be explained by the background of the transmission of Diophantus' text into Arabic and its subsequent transformations within the new cultural environment. In contrast, the abundance of the term in the original Greek text is a fact that should be studied against the background of Diophantus' conceptualization and practice.

has 'cause to exist', 'make', 'exist as a substance', and 'exist as an entity'; see Lampe (1961, A 6, 7, B 2).

[5] *Hyphistêmi* functions here the same way as all other verbs used by Diophantus to state positions (*tassô*, *plassô*, *estô*).

[6] For an in-depth discussion of the philosophical and other usages of the term, the reader is referred to the book by Romano and Taormina (1994) and the references included therein.

[7] See Liddell and Scott (1996), *s.v. hypostasis* (B III.2).

[8] (Lampe 1961, A 7.a).

[9] See Khazdan (1991), *s.v. Hypostasis* (ii, 966).

[10] Problem 42 in the Arabic book IV is the only place in which we encounter a clause that seems relevant. See Rashed (1984, 3, 83); Sesiano (1982, 120.1361–1362). In regard to the Greek original behind this clause, Sesiano writes that it "must have been something like *anatrechomen epi tas hypostaseis*."

Such a study is worthwhile not only because of the frequent use of the term in Diophantus but also because of its scarcity in other classical works of Greek mathematics. Indeed, the word is altogether absent from the works of major Greek geometers (Euclid, Archimedes, Apollonius); it appears rarely in Heron's works,[11] and occurs in Nicomachus only three times in a purely philosophical context.

In all its occurrences within the *Arithmetica*, *hypostasis* refers to numbers considered as individual entities. It does not mean a property predicated on a class of numbers, such as 'square' or 'even.' Its referent is the being itself, that is, the individual number, and it signifies the definite amount of units of which the number is made or posited to be made. Accordingly, *hypostasis* is a term inextricably linked with the numerical value of each number. This numerical value can be referred to either directly, as 'five units,' or indirectly, through a name (or whatever defining description functions as a name) assigned to a particular number (as the name 'John' is assigned to an individual human being). *Hypostasis* can be applied in both cases. In what follows, I argue that all occurrences of the term within the *Arithmetica* fall under this description; the term always refers to particular, individual numbers.[12]

In 119 of its occurrences within the Greek books, the word is part of the standard phrase "*Epi tas hypostaseis*," which appears in 97 problems (including the *lemmata* that precede some problems). This phrase usually signals the last stage in every problem worked out according to Diophantus' method, that is, the stage in which the numerical values of the requested numbers are calculated, and it is demonstrated that the values thus found hypostasize what was described in the enunciation, thereby "doing the problem." The phrase can occasionally be found elsewhere, in comments and remarks intercalated by Diophantus in the course of the solutions. Finally, in several resolutions in which Diophantus solves an auxiliary problem within the main problem, the phrase is repeated in both. In particular, such is the case with solutions "in the indeterminate," in which the phrase "*Epi tas hypostaseis*" announces the identities of the requested numbers (in the auxiliary problem), not directly as concrete numerical values, but through their names. These names are applied later to the requested numbers of the main problem.

The phrase is elliptical (the verb is implied)[13] and usually serves as a title introducing the last stage of the solution: this can be deduced from the fact that it

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[11] The verb *hyphistêmi* appears twice in *Metrica*, and once in *Dioptra*, with the meaning of assigning a concrete measure or identity to a line segment. The occurrences in *Metrica* are "Similarly, by positing (*hypostêsômetha*) the diameter AB 14 units" (Heron 1903, 74.26–27); "And the side of <the square of> 63 is approximately $7\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}$. Therefore, to find the area I have to posit (*hypostêsamenon*) so great the perpendicular" (Heron 1903, 28.1–3). The term also occurs (as either noun or verb) 15 times in the pseudo-Heronian *Definitiones*.

[12] This was also the point of view of Tannery, who in his "Index Graecitatis apud Diophantum" (*s.v. hypostasis*) defines *hypostasis* as "numeri quaesiti valor vel numericus vel expressus in *x*" (Tannery 1893–1895, ii, 285).

[13] From several instances we encounter within the *Arithmetica* the phrase is combined with verbs such as *erchomai* (come to, arrive at), *dierchomai* (go through, pass through), and *poiein* (to make, to do).

is repeated unchanged, even in problems in which just one number is requested. Even then, the plural *hypostaseis* is preserved, despite the fact that only one numerical value is calculated.[14] The interpretation of the phrase as title is corroborated by the fact that in 15 problems from the last two Greek books, the last stage of the resolution is merely announced by it, without being pursued further.[15]

Apart from the phrase "*Epi tas hypostaseis*," the word *hypostasis* is found eight times in the *Arithmetica*. Its clearest and most unequivocal use is found in problem I.39, at the end of the following passage: "And it is clear that the <number resulting> from '3 *arithmoi*, 15 units' [$3x+15$] can never be the largest; because the <number resulting> from '5 *arithmoi*, 15 units' [$5x+15$] is larger. Therefore the <number resulting from> '3 *arithmoi*, 15 units' [$3x+15$] will be either median or lesser; on the other hand the <number resulting> from '5 *arithmoi*, 15 units' [$5x+15$] will be either larger or median, while the <number resulting> from '8 *arithmoi*' [$8x$] may be either larger or median or lesser; for the *hypostasis* of the *arithmos* is not known." (Allard 1980, 409.4–9).[16] The word appears here as part of the formula '*hypostasis* of A,' where A stands for a named unknown. It refers to the amount of units of which the *arithmos* is composed, and means that the unknown is precisely this amount. Therefore, *hypostasis* refers to the underlying numerical value of the unknown *arithmos*.

A second occurrence is in problem III.12, where the word *hypostasis* is once again part of the same phrase as in problem I.39, that is, '*hypostasis* of the *arithmos*.'

It occurs again in problem II.11. Here, the term *hypostasis* refers not to an *arithmos*, as in the foregoing examples, but to a *dynamis*, the second power of *arithmos*; other than this, there is no difference in the way the term is understood.

A fourth occurrence is found in the second solution to problem III.15. The problem seeks to find three numbers X, Y, Z so that, $XY+(X+Y) \rightrightarrows U^2$, $YZ+(Y+Z) \rightrightarrows V^2$, $ZX+(Z+X) \rightrightarrows W^2$. Diophantus sets $X := 1x$, he adopts value 3 for Y and, based on the first condition of the problem, he derives the value 5½ for x, therefore for X too. Then, by assigning the values 5½ and 3 to X and Y respectively, which satisfy the first condition, he infers, on account of the last two conditions, that the two expressions $4x+3$ and $6\frac{1}{2}x+5\frac{1}{2}$ must both be squares. At this point, he makes the following comment: "But, since the multitude of the *arithmoi* and the units in one <expression> are greater than those in the other, but neither of the ratios of corresponding multitudes is that of a square to a square, the *hypostasis* that was made is ineffective." (174.1–4) Here the term *hypostasis* is part of the expres-

[14] See problems I.7–10, II.11, IV.18, and IV.37.

[15] See problems 15–17, 21–22, 27–28 in the fifth book, and problems 5, 7–11, 15, 17 in the sixth book.

[16] The translation is based on Allard's edition of the text. In Tannery's edition (78.14–19), the last word *arithmos* is replaced by its abbreviation.

sion "the *hypostasis* that was made" (*hê gegenêmenê hypostasis*) which refers to the two numbers, 3 and 5½, that were adopted as numerical values for the first two sought-after numbers. Therefore, even in this case the term is employed with reference to specific numbers.

In all cases discussed here, save the last one, the word *hypostasis* appears as a part of the phrase 'the *hypostasis* of ...', the second term of which was a named unknown, and means the multitude of units, or fractional parts of the unit, that are designated by the term in question. In all cases it refers to a concrete amount, that is, to a number qua particular entity. This interpretation is not inconsistent with the fact that the concrete number to which the term *hypostasis* refers is unknown, and therefore the multitude of units it signifies is unknown. On the contrary, the fundamental presupposition of the Greek concept of number, as a *definite multitude of definite things* is preserved in Diophantus not only with respect to known numbers, such as the numbers 5½ and 3 above, but also to algebraic unknowns such as *arithmos* or *dynamis*: they are all *definite numbers of units* or *of fractional parts of the unit*. This issue has been aptly explained by Jacob Klein: "In its special Diophantine sense of an *unknown* number, the *arithmos* is defined as 'having in itself an *indeterminate* multitude of monads' ... The multitude of monads which the unknown number contains is, however, indeterminate only 'for us' ... The whole point of each problem lies precisely in this – that a *completely determinate number of monads completes the solution in each problem*. When the unknown or its sign is introduced into the process of solution, it is precisely *not* indeterminacy in the sense of 'potential' determinacy which is intended." (Klein 1992, 140; Klein's emphasis) It could not be clearer! A completely determinate number of units 'hypostasizes' (*hypostêsai*) the 'nature' (*physis*) and the 'power' (*dynamis*) of the numbers designated in the enunciation of the problem, thereby completing the solution.

In the foregoing examples, *hypostasis* refers directly to the numbers of units underlying the algebraic unknowns. However, in some problems we find a more sophisticated use of the term. Diophantus occasionally characterises the names assigned to the sought-after numbers as *hypostaseis*. Thus, not only are the concrete numbers of units to which the names refer described as *hypostaseis*, but the names themselves as well. Such a use is made first and foremost in those problems in which Diophantus seeks to establish solutions "in the indeterminate"; that is, solutions which, in the end, leave the numerical values of the found numbers undetermined. As stated earlier, this approach is adopted for problems of an auxiliary nature, in the sense that they serve to solve the problems that come next. Such problems are the *lemmata* preceding problems IV.34–36, the first *lemma* preceding problem V.7, and problem IV.19, which serves as preliminary to IV.20. But there are also problems in which a solution "in the indeterminate" constitutes a preparatory stage before the final assignment of the names on which the equation will be formulated. An example of this last category can be seen in problem IV.17. Its enunciation asks to find three numbers X, Y, Z such that $X + Y + Z \rightrightarrows U^2$, $X^2 - Y \rightrightarrows V^2$, $Y^2 - Z \rightrightarrows S^2$, $Z^2 - X \rightrightarrows T^2$. Around the middle of the solution we read: "To the *hypostaseis*. The first will be '13 *dynameis*, 1 unit' [$13x^2+1$], the second '52 *dynameis*' [$52x^2$], and the third '104 *dynameis* wanting 1 unit' [$104x^2-1$]. And again three of the prescriptions have been solved in the indeterminate." (224.15–17) This phrase means that a major task in advancing the solution, namely, finding the names to be assigned to the sought-after numbers, has been accomplished: the positions to be adopted are $X:= 13x^2+1$, $Y:=52x^2$, and $Z:=104x^2-1$. These positions, however, should not be regarded as solving the problem. They constitute an intermediate result, undoubtedly very important, but intermediary, which paves the way for the rest of the solution. Using these expressions as names for the three numbers, and taking into account the last condition of the problem, Diophantus assigns to the side of the still-unnamed square T^2 the name '$104x+1$,' and then formulates the equation $10{,}816x^2-221=(104x-1)^2$ from which he derives the numerical value of x. With the numerical value of x at his disposal he is now ready to proceed to the final stage of the solution, the calculation of the numerical values of the requested numbers. Again the phrase "*epi tas hypostaseis*" appears in the text, opening this final stage: "To the *hypostaseis*. The first will be $\frac{170,989}{10,816}$, the second $\frac{640,692}{10,816}$, and the third $\frac{1,270,568}{10,816}$." (224.23–24) The term *hypostaseis* is thus used twice in problem IV.17: once referring to the expressions used as names for the requested numbers, or the "solution in the indeterminate," and again with reference to the concrete numerical values of the requested numbers. Thus, the *hypostasis* of a sought-after number, such as the number represented by X in our discussion, participates in the text in two guises: as a concrete numerical value ($\frac{170,989}{10,816}$) at the end of the solution, and as a name ($13x^2+1$) referring to that numerical value when the latter is still unmanifest. Diophantus uses the term *hypostasis* to refer to both.

In addition to the indeterminate solutions, Diophantus uses the term *hypostasis* when referring to names in two other instances: in problem IV.39 already discussed, and in problem IV.25, in the sentence "We have in the *hypostasis* of the side of the cube '2 *arithmoi*' [$2x$]." (244.21)

One more instance of *hypostasis* meaning the assigned name is found in the following sentence from the preface to the *Arithmetica*: "All this should be worked out with subtlety within the *hypostaseis* of the propositions, as far as possible, and until one species is left equal to one species." (14.21–23) The phrase "*hypostaseis of the propositions*" has puzzled scholars in the past. However, its meaning is clarified, if we take the word "proposition" to mean the entirety of a worked-out problem and not solely its enunciation. If "proposition" meant only the enunciation, then "the *hypostaseis* of a proposition" should be interpreted as something like 'the constituents of <the enunciation of> a proposition,' but there is no evidence to confirm this interpretation. The only sustainable interpretation for "proposition" in this case is one that covers both enunciation and solution, and in this sense the expression "*hypostaseis* of the propositions" is fully understandable as referring to the terms assigned in the course of the solution as names to the unnamed unknown numbers. Therefore, the interpretation of *hypostasis* as referring to numbers as individual entities is preserved even in this last instance.

21.3 Byzantine Commentators on the Meaning of *Hypostasis*

The Byzantine commentators on Diophantus generally adopted the interpretation of *hypostasis* as referring to the underlying numerical value of the unknown. Thus, commenting on problem I.12, a late thirteenth-century Byzantine scholiast describes the objective of solving the problem by saying "Further, what we want is to find the *hypostasis* of the numbers" (Allard 1983, 685.139–141). The task described by the scholiast as "finding the *hypostasis*" is not restricted to that particular problem. As already noted, the whole point of each Diophantine problem is precisely to find the numerical values of the numbers sought that "do the problem." For the scholiast, finding the numerical values of the requested numbers and the *hypostaseis* of the unknown numbers undoubtedly constituted two ways of saying the same thing. In the same spirit, this scholiast writes in his comment on problem I.13, "Therefore, the *hypostasis* has been found 36 units" (Allard 1983, 686.165), while another scholiast of the thirteenth–fourteenth century writes, in his comment to problem I.10, "the *hypostasis* of the *arithmos* is 76 units" (Allard 1983, 706.160). All these comments testify to the understanding of *hypostasis* as the underlying numerical value of an unknown number.

Further evidence supporting the same understanding is provided by an anonymous commentator on problem I.8 who explains how the equation $3x + 60 = 1x + 100$ is simplified to yield the two-term equation $2x = 40$ (Tannery 1893–1895, ii, 258.22–259.2).

The same understanding of *hypostasis* is reported by another comment, this one on the passage from problem II.11 mentioned earlier. The remark dates to the thirteenth century and is published by Allard (Allard 1983). Commenting on the phrase from the Diophantine text "I form the square from 1 *arithmos* wanting so many units so that the *hypostasis* of the *dynamis* surpasses the units of the wanting part that was set forth before, that is, in the present case, the 2 units," the commentator writes:

> That is to say, the *subsistent* (*hyphistamenê*) *dynamis*. *Subsistent dynamis* is that which results after the *hypostasis* when the *arithmos* is found to be so much. For, if the square is formed of '1 *arithmos* wanting 3 units' [$1x - 3$], the *dynamis* <to emerge> from the *arithmos* found will not exceed two units; because the *dynamis* of '1 *arithmos* wanting[17] 3 units' [$1x - 3$] becomes '1 *dynamis*, 9 units wanting 6 *arithmoi*' [$1x^2 + 9 - 6x$]. When what is wanting is added together and when like is subtracted from like, <by the> 'To the *hypostaseis*' <stage> the *arithmos* will be found to be $\frac{4}{3}$, and its *subsistent dynamis* will be $\frac{16}{9}$, which does not exceed <two units>; because, the two units are $\frac{18}{9}$, in which case the proof cannot proceed. If, however, <the square> is formed of '1 *arithmos* wanting 4 units' [$1x - 4$], the *subsistent dynamis* of the *arithmos* found after the *hypostasis* to be $\frac{15}{8}$, exceeds the two units." (Allard 1983, 696)

[17] In Allard's edition the sign for *leipsis* is missing.

This comment seems to suggest that, by *hypostasis*, the commentator understands the underlying numerical value of the *arithmos*, and on this basis, he interprets what Diophantus calls “*hypostasis* of the *dynamis*” as “*hyphistamenê dynamis*” (subsistent *dynamis*). ‘Subsistent *dynamis*’ is the particular, concrete (although not manifest to us) multitude of units (or fractional parts of the unit) designated by the unknown number bearing the name ‘1 *dynamis*.’

Finally, the interpretation of *hypostasis* as numerical value is endorsed by Maximus Planudes, the most important Byzantine commentator on Diophantus, who explains the term in his comment on the first problem in the *Arithmetica*: “He calls *hypostaseis* the requested numbers themselves, in fact, their existences (*hyparxeis*)” (Tannery 1893–1895, ii, 147.27–28). Obviously, the *hyparxis* of an unknown number is what exists within it, in other words, the multitude of units (or parts of a unit) that constitute the unknown number.

21.4 Conclusion

In the enunciation of a Diophantine problem, a combination of properties is proposed for the numbers sought, giving information about their kind as well as the operations and relationships to which they are subjected, and the determination of the specific numbers which *hypostasize* these properties is required. Thus, the whole point of each problem lies precisely in the determination of the specific numbers that “do” the properties stated in the enunciation, or, as Diophantus puts it, which “do the problem.” Diophantus refers to the numbers which hypostasize the properties stated in the enunciation as *hypostaseis*. In this paper, we have shown that in most cases a *hypostasis* is identified as a concrete numerical value, and this interpretation was endorsed generally by the Byzantine commentators of Diophantus. However, a more sophisticated use of the term, according to which the term *hypostasis* is applied to the name assigned to an unnamed sought-after number, is allowed by Diophantus as well.

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Chapter 22
On the Hazardousness of the Concept 'Technology': Notes on a Conversation Between the History of Science and the History of Technology

Aristotle Tympas

Abstract Historians of science and historians of technology have recently turned their attention to the conceptual history of 'applied science' and 'technology' respectively. 'Technology' was a concept introduced in the nineteenth century as concerning both 'applied science' and 'industrial arts.' A developed version of this concept caught on after the first decades of the twentieth century, following the establishment of technological networks and the rise of 'Fordism,' 'Taylorism' and 'technocracy.' Based on interpretations of the nineteenth-century circuit of the steam engine and the twentieth-century network of electric power, this chapter brings together observations from the history of science, the history of technology and the critique of classic political economy to elaborate on the suggestion that 'technology' has been a 'hazardous' concept. Central to the argument of the chapter is the retrieval of a correspondence between the conceptual couples 'technology'-'-technics' and 'surplus value'-'value.'

Keywords Technology • Applied science • Fordism • Taylorism • Technocracy

22.1 Introduction

Historians of technology are no longer obliged to prove that technology has been as noble as science, while historians of science do not have to worry if it turns out that science has been involved in non-noble work. The two fields can now advance by jointly researching the historical differences between science and technology without assuming beforehand what these differences are. This was not always the case. Historians of science initially assumed that technology was 'applied science'. Being nothing more than applied science, technology did not have a right to its own

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history. Unsurprisingly, following the break in the late 1950s with the history of science and the efforts to institute and establish their specialty as a distinct field, historians of technology of an earlier generation spent much of their energy arguing that technology is not applied science. Things have certainly changed since then. While historians of science and historians of technology now agree that technology is not applied science, they take issue with the persistent appeal of the rhetoric of presenting technology as applied science. In this chapter I register some notes on this issue by way of contributing to an ongoing conversation between historians of science and historians of technology.[1]

To understand why technology has been presented as applied science, historians of technology have started paying attention to the history of the concept 'technology'. It also helps that historians of science have started to research what 'applied science' actually was. Recently (2012), a special issue of *ISIS*, the journal of the History of Science Society, offered a critical survey of the historiography of both the history of the concept 'technology' and the history of the concept 'applied science'. Those who follow this historiography from the perspective of the history of technology seem to agree that a key contribution is that of Leo Marx, who has argued that the concept 'technology is a "hazardous" one. Here, I elaborate on the argument about the hazardousness of the concept 'technology' by retrieving a correspondence between this concept and the concept of 'surplus value' of another Marx, Karl. I think that it makes it all the more interesting to know that Karl Marx himself experimented with the use of the initial version of the concept 'technology', which he juxtaposed to the concept 'technics', so as to reinforce the difference between the concept 'value' of classic political economy and his own key concept, 'surplus value'.[2]

[1] For those who want to follow the development of the historiography of technology, there are, for example, the accounts by Eugene Ferguson (1974), Reinhard Rürup (1974), John Staudenmaier (1985) and Alex Roland (1997). The assumption that technology is applied science could not be sustained once the attention was shifted from the moments of the invention of technology to its long-term use. The reconfiguration of technology in use involved hardly any science. Influential here has been an article by David Edgerton (1999). For equally insightful articles, see the ones by Carol Purcell (1995) and Ruth Cowan (1996), which show that the shaping of technology in use is inherently a process of construction of gender. A balanced integration of constructivist approaches to the historiography of technology has certainly contributed to opening up the definition of technology beyond the limits set by those who assumed that technology is applied science, Staudenmaier (2002, 2009), Tympas (2005).

[2] On the history of the concept 'technology' and/or the meaning of 'applied science', some of the most valuable contributions are authored by Ronald Kline (1995), Wolfgang König (1996), Leo Marx (1997), Ruth Oldenziel (1999), Eric Schatzberg (2006, 2012), Carl Mitcham and Eric Schatzberg (2009), Jennifer Alexander (2012), Robert Bud (2012), Graeme Gooday (2012) and Paul Lucier (2012). Earlier attempts at a history of the concept 'technology' include the ones by Graham Hollister-Short (1977) and Jean-Jacques Salomon (1984).

22.2 ‘Technology’ as the Science of Classification of Equivalent Arts

The concept ‘technology’ was introduced in the nineteenth century in countries where industrial capitalism was advanced in connection with what was then called ‘industrial arts’. Industrial arts were not like the ‘mechanical arts’ of the past. For one thing, unlike the mechanical arts, the industrial arts did not include painting and sculpture. Following the split between ‘fine arts’ and ‘vulgar arts’, industrial arts would have been vulnerable to being placed on the side of what was devalued as vulgar. This was avoided by connecting industrial arts to ‘applied science’. But, of course, there could be no talk about ‘applied science’ before the establishment of the concept ‘science’, which also took place in the nineteenth century.[3]

Eric Schatzberg summarizes our present knowledge of the history of the relations between these concepts nicely when he writes that in the context of a nineteenth-century rhetorical drive aiming at transferring agency from art to science, the discourse of pure and applied science dispensed with the need to address the mechanical arts at all. ‘Industrial arts’ remained common until World War I (this changed with the emergence of ‘Taylorism’, see below). Flexible as the concept ‘applied science’ was, it could refer either to an independent body of artisanal knowledge or to an application of the principles of science to practical problems. Schatzberg explains that this flexibility allowed it to do boundary work for both engineers and scientists at the time when both were stabilized as professions in the second half of the nineteenth century. As demarcated from art, ‘science’, and therefore ‘applied science’ too, would be disassociated from workers (Schatzberg 2012).

It was not until the 1930s that ‘technology’ was put into wide circulation. Between the 1860s and the 1930s, the meaning of the concept was developed. Schatzberg is correct in arguing that ‘technology’, as first introduced, was an obscure concept. In most cases, this concept referred to the ‘science of the arts’, with science perceived here as the possibility of a uniform classification of the arts. This can be confirmed by the Greek case. The concept ‘technology’ was used in a 1864 Greek educational book to indicate the possibility of placing previously unconnected arts under one classification scheme. The early association between ‘technology’ and the attempt to place the arts under one classification merits special notice. We now know that no scientific classification is neutral. Classifications come with consequences. For one thing, when something is successfully drawn into

[3] For scattered and rather experimental uses of ‘technology’ before the nineteenth century, see the review of Carl Mitcham and Eric Schatzberg (2009). The *Oxford English Dictionary* credits the naturalist-theologian William Whewell with the introduction of the term ‘scientist’ in 1834. Before then, ‘science’ was used to signify any knowledge that was well established.

some classification, it enters into a minimum of equivalence to everything else that has fallen under the same classification.[4]

In this case, the classification of the arts advanced by the concept 'technology' was inseparable from the attempt to establish an equivalence of exchangeable commodities at a market determined by the industrial-capitalist mode of production. Without such classification, it would have been impossible to move on to present as equivalent social experiences that were previously unconnected. We can argue so based on an interpretation of the steam engine, the paradigmatic modern machine. The circuit of the steam engine did indeed institute equivalences between different arts. For example, it made equivalent the art of feeding a boiler and the art of moving a loom while it simultaneously established an equivalence between the coal that fed the boiler and the textile produced at the loom. Moreover, it produced an equivalence that could extend from, for example, the boiler feeder to the loom operator, and from the coal miner to the cloth maker and further. Those standing at the two ends became connected by what the theory of value of the classic political economy defined as 'labor'. As is well known, from Adam Smith to David Ricardo, classic political economy came to argue that the source of 'value' is not land—as with the physiocrats—but labor. And as we saw, without the circuit of the steam engine that connected the arts at the material level, this argument would have been impossible.[5]

'Technology' is a concept that appeared in response to (and in support of) the interconnection of the arts, on the grounds of equivalent labor. At the time, there was no definite concept of science to determine the shaping of the concept 'technology'. The two concepts developed in synergy. To indicate how, let us observe that at roughly the time when 'technology' was introduced to replace the 'industrial arts', 'science' was introduced to replace 'natural philosophy'. The transition from natural philosophy to science overlaps with the transition to thermodynamics. The key concept of thermodynamics, 'energy', was brought forward to establish the equivalence of heat and motion, which was not covered by Newtonian physics. In this sense, the use of the concept 'science' came to signify a critical enlargement of the range of equivalent natural phenomena, while the use of the concept 'technology' came to signify the equivalence of all industrial arts.[6]

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[4] The 1864 Greek book on 'technology' was authored by Dimitrios Apostolidis. On the normative dimensions of classification, see the relevant argument by Geoffrey C. Bowker and Susan Leigh Star (2000).

[5] For the emergence of the classic political economy and its labor theory of value, see the clarifications offered by John Milios (2009).

[6] The kinetic theory of heat had prepared for the equivalence between heat and motion. The development of thermodynamics and the use of the concept 'energy' marked the establishment of this equivalence. For an introduction to the history of thermodynamics as a socially situated science, see, for example, a perspective offered by Faidra Papanelopoulou (2008).

22.3 'Technology' After the Drive for 'Scientific Management'

We can now turn to the meaning of 'technology' as it had developed by the 1930s, which is actually the meaning that allowed for the massive use of the concept. By then, the electric power networks had been firmly established through an expansive reproduction of the technical pattern introduced by the steam engine. Many engineers argued about continuity between such mechanical and electrical engineering artifacts as steam engines and electric dynamos. In the context of this continuity, a dynamo with an electric power transmission line was perceived to be analogous to a steam engine with very long energy transmission rods. The transformation of the circuit of the steam engine overlapped with the expansive use of the concept 'energy' so as to obtain the much broader reach of an electric power network. Between the 1860s and the 1930s, the concept 'energy' was developed to include in the equivalence not only heat and motion, but also electricity and mass. Kelvin's concept of 'energy' is associated with the circuit of the steam engine, while Einstein's is associated with the much larger circuit of an electric power network (which represented the expansive reproduction of the steam engine circuit through a network like the electric power transmission grid). In short, while the 1860 version of 'technology' referred to the material and social equivalence introduced by the relatively local circuit of the steam engine, the 1930 version covered the more global equivalence of an electric power network (and other networks such as transportation, communication, etc.).[7]

'Technology' started as a concept pointing to the potential of material and social equivalence through the 1860s science of classification, but it was put into mass use when this equivalence became broader: when comparatively isolated circuits became interconnected networks by the 1930s. 'Technology' is a concept that was introduced when the First Industrial Revolution (steam) was established, but obtained the meaning allowing for its generalized use after the establishment of the Second Industrial Revolution (electricity). Leo Marx has suggested that 'technology' was a concept that came to cover the "semantic void" that emerged when the available concepts could not capture the change in the material environment brought about by the Second Industrial Revolution. If my above line of reasoning is correct, it may be more appropriate to replace the argument about a semantic void by a more dialectical explanation: the concept 'technology' was developed in

[7] There is much known about the continuity between mechanical and electrical engineering through the work of Stathis Arapostathis (2008). On the broader continuity of mechanical, electrical and electronic engineering, see my argument in (Tympas 2007). For the influence of the emergence of technological networks in Einstein's concept of 'energy', see the history offered by Peter Galison (2003). A very useful history of the history of the transition from steam engines to electric power networks has been written by Louis C. Hunter and Lynwood Bryant (1991).

reaction to the successful development of industrial capitalism from the First to the Second Industrial Revolution.[8]

Between the 1930s, when the use of concept ‘technology’ started to become popular, and the 1860s, when an initial version of it was introduced, the mode of production based on the use of a technical pattern set by the steam engine—the industrial-capitalist mode of production—succeeded in undergoing a double expansion. Inside the factory, it grew toward more specialized machines and correspondingly unskilled workers. This is known as ‘vertical integration’. At the same time, there was an attempt at a ‘horizontal integration’: inputting only raw materials at one side and integrating marketing with production at the output side. This two dimensional integration took place under the combined influence of what became known as ‘Fordism’ and ‘Taylorism’, respectively.[9]

Henry Ford was not the only one who cared about generating a mass demand to match the mass supply of products manufactured in his factories. Samuel Insull, who had started as Thomas Edison’s secretary before moving on to financially control an empire of electric power utilities, had the same concerns. Insull realized that to make the most profitable use of electricity supplying (generating) factories there had to be constant demand for electricity. Ford himself symbolizes the enlargement of the mass-producing factory to its limit. In comparison, Insull symbolizes a version of Fordism that referred to the network that grew together with the Fordist factory. This was the network formed by the lengthening and interconnection of the lines connecting the mass-producing factory to mass consumption. It is only after the establishment of the unit formed by a mass-producing factory like Ford’s and a massive network like Insull’s that the concept ‘technology’ started to be used on a massive scale.[10]

Fordism was complemented by Taylorism. Fordism could open the factory up to the motion of special purpose machines only as long as Taylorism could make space by controlling the movements of the workers. Similarly, Fordism could rely on skills embedded in machines only as long as these skills could be extracted from those who would work them. This is what Taylorism sought to do. Yet, de-skilling at one level went hand in hand with re-skilling at another. The other face of the formation of a pyramid of workers was the formation of a pyramid of engineers. No one was actually unskilled. But relative differences in skill were ideologized as being absolute. In reality, skill has been indispensable to profit. Skill was the source of profit. Skill, however, could be rhetorically neglected by presenting the machine as the source of value, therefore paying less for skill and making more profit. This is

[8] The development of the meaning of ‘technology’ over the course of the Second Industrial Revolution was associated with a shrinking in the meaning of the concept ‘arts’. As the arts were devaluated in comparison to both industry and science, the meaning of ‘technology’ came to cover both the industrial arts and applied science. On this point, see (Schatzberg 2012). For Leo Marx’s argument about a “semantic void”, see (Marx 1997).

[9] A classic history of these changes is given by David Hounsell (1991). On the limits to Fordism, see the work of Phil Scranton (1997).

[10] For an introduction to Insull, see that of Thomas Hughes (1989).

what the rhetoric of Frederick Taylor was all about. Skill points to art. Taylorism was about replacing the arts with science. Self-presented as 'scientific management', Taylorism sought to advance a science of the arts that would be independent of the workers. This is just like the industrial arts as connected with applied science. Taylor's definition of 'scientific management' was an aggressive version of what was meant by 'applied science'. It was then a concept that shared its meaning with the concept 'technology'.[11]

In partnership with Fordism, the drive toward 'scientific management' gave rise to a movement called 'technocracy'. If Taylorism was about managing society at the factory level, technocracy was about managing society as the most general level: the central government. While Taylorites sought to establish an engineering rule at the factory, technocrats were pursuing the management of politics with an engineering rule. Taylorism and technocracy became popular in the first decades of the twentieth century. But, in 1929, technocracy took a hit because the US citizens had elected as president an engineer (Herbert Hoover) who failed to regulate Fordism and thus avoid mass overproduction and the unprecedented crisis that this induced.[12]

Technocracy was explicit about the connection between material and political artifacts. By contrast, the use of the concept 'technology' concealed the fact that politics is embedded in materialities. While technocracy took a strong hit by the big crash, the technocratic ideals could rely on the quickly spreading use of this concept to survive. In other words, the wide use of the concept 'technology' came to the rescue of the technocratic ideals when the politics of technocracy were proven to be questionable. Adjusted versions of these ideals became an organic part of governments throughout the world, from the United States of America of Franklin Roosevelt to the Soviet Union of Joseph Stalin, and, from the European totalitarianism of Germany and Italy to the Asian totalitarianism of Japan. To explain why Leo Marx is right in suggesting that technology is a 'hazardous' concept, I suggest that we start by noting that this concept was catching on amidst a terrible economic crisis and the devastating war that followed it.[13]

22.4 'Technology' Is to 'Technics' What 'Surplus Value' Is to 'Value'

For a further elaboration on the hazardousness of 'technology', I suggest that we elaborate on a conceptual deadlock that emerged parallel to the spread of the paradigmatic circuit of the steam engine. As mentioned above, the availability of this circuit made it possible to argue for a general equivalence of the arts, which

[11] The most influential study of Taylorism is, perhaps, that of Harry Braverman (1974).

[12] For a relevant history of engineering, see the classic by David Noble (1977).

[13] On the international spread of 'Americanism' (the Fordism-Taylorism mix), see (Hughes 1989).

brought forward the labor theory of classic political economy. The spread of steam engines, paralleling the advance of this theory from Smith to Ricardo, established the attribution of value to labor. While this theory demanded that it was labor and not land that produced value, it could not answer the question regarding the role of the machine in the making of value. For classic political economy, it is labor and not the machine that produces value. But classic political economists had to acknowledge that the machine, too, was somewhat involved in the process of production of value. Yet, at the same time, they insisted that labor could be the only source of value. This was then the constitutional ambiguity of classic political economy: the availability of machines clearly affected the production of value, yet the only source of value was labor (Milios 2009).

This ambiguity, I think, is a manifestation of the infamous 'machinery question' of the nineteenth century. Historian Maxine Berg has shown how important this question was for both nineteenth-century British society and for classic political economy (Berg 1980). As early as 1929, in his masterful *A History of Economic Thought*, Isaak Rubin had shown that the concepts of classic political economy could not address this question. This would require a concept that could open up the possibility of a break with classic political economy. This was precisely what Karl Marx's concept 'surplus value,' was all about. It was introduced in a book subtitled *A Critique of Political Economy*. 'Capital', the concept that gave the title to the same book, was about the self-propelling accumulation of surplus value. To better understand the difference between the concepts of classic political economy and the concepts of its Marxian critique, let us turn once more to the circuit of the steam engine.[14]

Through the use of the circuit of the steam engine, heat was made equivalent to motion. But the flow in the circuit of the steam engine was one directional: from heat to motion. It was designed to produce textiles from coal. One could not use the same circuit in the reverse to produce coal from textiles. Coal and textiles could be exchanged outside the circuit of the steam engine, outside the factory. Inside the factory, using the circuit of the steam engine, only textiles could be produced. The machine and its factory represented a one-directional flow toward an irreversible production, not a reversible two-directional exchange. In the language of the science of thermodynamics, the quantity of energy was equal at the two ends of the steam engine, but the quality was not. The two were not then of equal value. The mediation of the steam engine meant production of extra value and not just an exchange of equal values. Industrial production meant a loss of energy quality for a gain in value.

The second law of thermodynamics laments this loss of available energy (and worries about it) as an irreversible loss in nature. It was indeed a loss of labor, of human or other agents of nature (in the context of the science of thermodynamics

[14] On the Marxian concepts, see (Milios 2009).

there was an increase of 'entropy').[15] But as an industrial process, this natural loss came with an increase in social value. The classic political economy could not theorize this increase in value because it assumed an equal exchange of it. It looked at the economy from outside the factory. This view black-boxed the steam engine in a manner that concealed how laboring with it resulted in an increase of a value. Classic political economy lingered at the exchange part of economy: the market value. It did not consider the part of the economy dealing with machine production: the surplus value (the capital). This is why its concept, 'value', was marked by an ambiguity in regards to the machine. This is why the machine was a problem for political economy.

At the climax of the 'machinery question', which could not be addressed by the concept of value, Marx did not only make use of the concept 'surplus value'. Taking note of the introduction of 'technology', he used this concept to further point to what political economy could not see. As Guido Frison has observed, in a revised edition of *Capital*, Marx differentiated between 'technics' and 'technology'. According to Marx, the two were referring to the same process viewed from different angles. When a factory steam engine was viewed from the perspective of the material artifact that made two arts equivalent, he wrote of 'technics'; when the same was viewed from the perspective of the science that made such equivalence possible, he wrote of 'technology'. The new concept, 'technology', seemed very appropriate for such differentiation. Technology was technics inseparable from logos (techno-logy). It was a concept to acknowledge that materialities are inseparable from discourses.[16]

I think that technology was to technics what surplus value was to value. Marx introduced the concept 'surplus value' to point to the ambiguity of the theory of value—to show what was concealed by the concept 'value'. The new concept, 'technology', offered him an opportunity to point to the same ambiguity at the level of the conceptualization of the material practice referred to by this theory. Both the 1860s and the 1930s version of 'technology' were about the inseparability of a discourse about material artifacts and material artifacts themselves. Karl Marx used the 1860s version of the concept to be explicit about the discourse from science that could lead to an understanding of how materialities contain socialities. By the

[15] The opening paragraph of the infamous *On the Age of Sun's Heat* by Sir William Thompson (Lord Kelvin), which was published in Macmillan's Magazine on March 5, 1862 (vol. 5, pp. 388–393), touches on this irreversible loss in nature, which would lead to death if the universe were not finite: "The second great law of thermodynamics involves a certain principle of *irreversible action in Nature*. It is thus shown that, although mechanical energy is *indestructible*, there is a universal tendency to its dissipation, which produces gradual augmentation and diffusion of heat, cessation of motion, and exhaustion of potential energy through the material universe. The result would inevitably be a state of universal rest and death, if the universe were finite and left to obey existing laws."

[16] For Guido Frison's observation, see (Frison 1988). For a further contextualization of this, see other articles by Frison (1993a, b, 1998) and by Fumikazu Yoshida (1983a, b). Little has been written on Karl Marx in history of technology journals. For one of the few exceptions, see the 1984 article in *Technology and Culture* by Donald Mackenzie (1984).

1930s, the discourse of 'science' that was influencing the concept 'technology' was determined by the 'scientific management' rhetoric of Taylorism. This rhetoric, which we find in the 1930s version of the concept 'technology,' was tailored to conceal the fact that materialities contain socialities. The concept Karl Marx had tried because he thought it was promising had been developed into the concept Leo Marx has aptly called "hazardous."

22.5 Addendum: On Central and Peripheral Historiographical Issues

To add support to the call for research on the emergence and development of the concept 'technology' in the modern period, we may also consider the implications of the absence of such a concept before modernity. The realization that 'technology' did not exist as a concept before modernity invites us to question if there can be such a thing as a history of technology in antiquity or in any other pre-modern historical period, western or not. If the answer is yes, how do we produce meaningful analogies between modern technology and its historical counterpart in another period (whatever that may turn out to be)? In pursuit of such analogies, it seems to me that we need to revisit the answers to some of the foundational questions of historiography of technology. Consider, for example, the question posed in *Does Technology Drive History? The Dilemma of Technological Determinism*, which was edited by Leo Marx and Merritt Roe Smith (1994). Would it be meaningful to raise such a question in the context of the historiography of antiquity? And is there a dilemma about technological determinism in antiquity?[17]

In regards to modernity, the dilemma of technological determinism is a different manifestation of the ambiguity of the classic political economy that this chapter has argued about. It follows that the hazardousness of 'technology' has to do with the way the use of the concept allows for the hegemony of the ideology of technological determinism. Considering the correspondences between this concept and that of 'surplus value', technological determinism can be interpreted as the key ingredient of an ideology that the *Capital* introduced as the 'fetishism of commodities'. We saw how important to this ideology is the vulgar presentation by 'scientific management' of materialities as independent of socialities.

Having touched on the similarity between technological determinism and the ambiguity of classic political economy, I will conclude by registering one last note regarding the absence of the concept 'technology' before modernity. The concept 'technology' was not used, for example, in Greek antiquity or during the Byzantine period. It follows that those who profess to practice the history of technology in these periods have to be explicit about the concepts (if any) they consider to be

[17] A suggestive update on the persistence of technological determinism is given by Sally Wyatt (2008).

equivalent to the modern concept 'technology'. A historian who specializes in the study of technology in modernity in a country like the US is not directly challenged to pose this question or respond to it. After all, there is no such thing as the history of the US in antiquity. The same is the case with any other country that has a history fully contained by the modern period. But the issue cannot pass unnoticed in reference to a history about Greece (or, to take the example of the world's most populous country, China).[18]

Distinguished scholars of technological determinism, from Leo Marx and Merritt Roe Smith to Bruce Sinclair and Joseph Corn, have assumed that technological determinism is about rhetorical constructions of a glorious technological future. However, as ongoing research on the history of technology in modern Greece has shown, technological determinism can actually be based on rhetorical constructions of a glorious technological past, that of ancient Greece. For example, as Spyros Tzokas has argued, the founding members of the modern community of Greek engineers were promoting the most technocratic visions regarding the technological infrastructures of modern Athens by arguing that the glory of ancient Athens was actually due to its technological infrastructures. For them, modern Greece would become glorious only as long as it could invest in a future technology that would be as advanced as ancient Greek technology was in the past. Unavoidably then, those who wish to study the history of technology in modern Greece are obliged to be explicit (and convincing) about the macro-historical periodization which subsumes their study of the history of technology in modernity.[19]

The Greek case suggests that it is not only the proper study of the history of technology beyond modernity that is at stake here. It is also the proper study of the history of technology in modernity, with the latter feeding on the former. If this is the case, a seemingly peripheral issue concerning the practice of history in (and about) Greece may have a more central historiographical message to deliver. As Kostas Gavroglu has argued, any historiography of science and technology in the peripheries would manage to address central historiographical questions only to the extent that it could begin to question the very definition of center and periphery. If this chapter manages to somehow help the history of science and technology move to the center of the constellation of historical specialties, it is because of Kostas Gavroglu's masterful and tireless teaching on how to reevaluate the periphery.[20]

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[18] For a history of technology in antiquity that is sensitive to concepts, I recommend that of Serafina Cuomo (2007). The importance of the Chinese case is convincingly argued by G.E.R. Lloyd (2004).

[19] For a first attempt at such periodization, see (Tympas 2002). For a sample of studies on the futurism of technological determinism, see (Sinclair 1986; Corn 1988, 1996; Marvin 1990; Wright 1992; Nye 1994; Corn and Horrigan 1984). For the construction of a history of technology in antiquity by modern Greek engineers and its integration into technological determinism, see (Tympas et al. 2005).

[20] Gavroglu et al. (2008).

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Chapter 23
Wireless at the Bar: Experts, Circuits and Marconi's Inventions in Patent Disputes in Early Twentieth-Century Britain

Stathis Arapostathis

Abstract This chapter brings together the law and the relevant institutions at the center of the analysis, with the aim of shedding light on the culture of invention as it developed and, eventually, prevailed in the field of wireless technology. It supplements the existing historiography of the wireless industry in Britain in the early twentieth century which focuses on the business strategies, the development and economics of manufacture and the role of corporate R&D in technological transitions, as well as that of contracts and agreements in a national and international setting. Inventorship in the industrial setting of wireless is reconstructed as a complex activity that was formed through the performance, agency and, most importantly, interaction of various experts and actors. The management of Marconi's inventions involved circulation of knowledge, expertise, credit and trust in various locations: the laboratory, the public sphere of technical journals and the law courts. The case study in this chapter concerns the making of a patent, and a strong monopoly, through the decision-making process of the British law courts. The story argues that Marconi's success in the Marconi vs British Radio Telegraph and Telephone Company court case was the result of preparation and organization, and the use of experts who combined scientific, practical and legal credibility. Despite the ideologically driven public discourse on the cognitive and social superiority of science over invention and practice, in the law courts a mixture of scientific authority and practical experience provided credible witnessing.

Keywords Wireless telegraphy • Intellectual property • Law courts • Experts • Innovation

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23.1 Introduction

Patent disputes were (and still are) legal events where 'intellectual property' assets acquired ownership and proprietary status through the agency and the performance of several experts: judges, lawyers, patent agents and expert witnesses. Recent scholarship in the Law, Science and Technology field has stressed the importance of experts, and the contribution of their authority and credibility in the making of law and the construction of techno-scientific meanings in the courts, particularly in the Anglo-Saxon world (Arapostathis 2013; Golan 2004; Gaudill 2007). Historians of science and technology whose research focuses on the interrelation of patent law and the construction of techno-science have pointed out the importance of advisors, consultants, lawyers, patent agents and expert witnesses in the construction of the proprietary status of patents and copyrights (Guagnini 2002; Hong 2001; Toscano 2012, 57–85; Arapostathis and Gooday 2013; Miller 2006). More particularly for the developments in the history of wireless, Hong has shown the role of John Ambrose Fleming as an AC expert in the first transatlantic transmission of 1901 (Hong 1996a, 431–465, 1996b, 2001). Anna Guagnini (2002) has stressed the role of patent agents and attorneys – most importantly, of Edward Carpmael and Fletcher Moulton – in the making of Marconi's first wireless patents. The present chapter focuses on the management of expertise by the Marconi Company and its role in the making of patents. The context is Britain in the early twentieth century – though some comparisons with the American case are made. I show that the law courts (the British Bar) were institutions in which the meanings of innovations were configured and experts competed to establish trustworthy testimonies and credible advice. Following Harran et al., the law court is approached as a 'public theatre of contestation' (Haran et al. 2008, 72) where lawyers, witnesses and inventors struggled through the adversarial system to establish a legitimate and credible framing of technical problems and inventing activities (Arapostathis 2013).

The chapter supplements existing historiography of business and technology history that focuses on the business strategies, the development and economics of manufacture, and the role of corporate R&D in technological transitions, as well as that of contracts and agreements in a national and international setting (Aitken 1976, 1985; Hong 2001; Sonneborn 2005; Bruton and Gooday 2010). It brings together the law and the relevant institutions at the center of the analysis, with the aim of shedding light on the culture of invention as it developed and, eventually, prevailed in the field of wireless technology. In the Anglo-Saxon legal culture, the adversarial setting and the principle of precedence shaped the context in which the law was formed and rewritten in a very dynamic way, through the interaction of a variety of actors: lawyers, witnesses and judges. The main methodological direction used here is to approach 'law as technology' (Arapostathis and Gooday 2013; Arapostathis 2013). Inventorship is understood as a nested 'trading zone' (Galison 1997; Collins et al. 2010) that is formed through the performance, agency and, most importantly, interaction of various experts and actors. 'Interactional expertise' (Collins and Evans 2007; Collins et al. 2010) through processes of socialization was an important condition for ‘legal-technical hybrids’ (Swanson 2009, 547–548) such as judges, legal experts and consulting engineers. In patent disputes and legal contestations, networks of trust are informative about the judicial processes, as well as the culture of expertise in public institutions like the courts. As the case on Marconi’s inventions shows, the judgments of judges about whom to trust had to do with the interrelation and management of the expertise of actors: expert witnesses, lawyers, and external advisors (Arapostathis and Gooday 2013; Arapostathis 2013).

23.2 Managing Intangible Assets and Their Histories in Wireless Industry

Mario Biagioli (2007) has shown persuasively that patents are forms of credit: credit as money through the establishment of monopoly regimes, and credit as reputation through the increase of social and cultural capital of the inventors. British inventors of the late nineteenth and early twentieth centuries developed knowledge management strategies and literary tactics acting as bricoleurs who participated in several economies: institutional, legal and cultural (Arapostathis and Gooday 2013). Public sites such as the daily press, the quasi-public technical press of the period, as well as the law courts became the main forums where stories about inventions circulated and where the struggles for the legitimization of the ‘true’ and ‘correct’ story took place. Patent disputes constructed and diffused a public discourse of invention as an individualist process while the inventor was represented as the heroic individual who, through laborious activities, managed to tackle practical problems with the invention of technologies (Arapostathis and Gooday 2013).

Guglielmo Marconi followed a multifaceted strategy that was comprised of secrecy, selectivity in the disclosure of information, seeking exclusive contracts with state institutions and foundations, pursuing patents, and combating patent rights in the law courts with the aim of exercising a monopolist regime in wireless and radio industry. A year after his landing in England (1896), he was awarded the first wireless patent. He aimed to capitalize on the existing patent that was considered to be very broad, and thus an asset that could easily establish a strong monopoly in the emerging industry. Initially, Marconi himself developed this strategy with the support of William Preece who was the Chief Engineer of the Post Office, an authority in telegraphy and a leading figure among Victorian electrical engineers (Baker 1976; Tucker 1981–1982). The strategy was re-affirmed under the agency and the managerial strategies of Cuthbert Hall who was the manager of the Marconi Company the period 1903–1908. Hall insisted that the company should avoid enforcing their monopoly through litigation; instead, it should pursue exclusive contracts and secure state patronage of any kind. In Britain, the company started to pursue legal battles only after 1909 and under the influence of Godfrey Isaacs, its new manager. Isaacs put emphasis on the power of intangible assets and on the strong management of 'intellectual property' as part of the identity and the corporate strategy of the company (Arapostathis and Gooday 2013, 20–21; Bruton and Gooday 2010). Due to Isaacs, the Marconi company initiated its first legal dispute in Britain: the Marconi vs British Radio Telegraph and Telephone Company. It was a patent dispute that attracted the interest of the industrial and engineering worlds of Edwardian Britain and in which legal and engineering experts fought to secure a major triumph for the corporate side they supported.

While this was the strategy in Britain, in the United States the context was very different, because intellectual property law was different. The Patent Office, as an examining institution, was more organized than its British counterpart and could play a more substantial role in shaping of techno-scientific policy on IP issues. In the US setting, Marconi and his company were forced to legally defend their inventions in order to secure their place in a competitive market, with no option for state or military patronage. Marconi himself had to give evidence in the US patent office in order to defend his patent applications (20 November 1902; 2 February 1903; 9 February 1904) from interference by applications from Reginald Fessenden and Harry Shoemaker. Marconi did not only mobilize his close collaborator, George Stephen Kemp, but also tried to support his patent applications by himself. His evidence was a rich reconstruction of how things occurred: when, where and under what conditions. Marconi tried and managed to set out a master narrative of his priority in inventing a hysteresis magnetic receiver, a contested innovation (Marconi 1904). Some years later, in a legal dispute with the National Electric Signalling Co, which managed the patents of De Forest, Marconi gave evidence in the Eastern District of New York court. His testimony began on June 17, 1913 and lasted for several days. His testimony had a strong historical element, trying to establish his priority as a trustworthy storyteller (Marconi 1913).

Marconi's battle to establish priority claims occurred not only in legal or legalistic settings. He and his supporters promoted his cause and his status as the inventor of wireless through publications and lectures in public spaces and in mediating institutions, like the popular press and technical journals (Arapostathis and Gooday 2013, 141–152; Toscano 2012, 77–84). William Preece's admiration and support to the young inventor was expressed publicly in a lecture in the Royal Institution where he attributed to Marconi the invention of 'signalling through space without wires'. The attribution of credit to Marconi triggered the reaction of major electricians and physicists. Both Oliver Lodge and Silvanus Thompson reacted, arguing that Marconi's innovations should be considered either as improvements of their inventions or as misappropriation of un-patentable inventions and principles. Marconi's appropriation of the 'ownership' of wireless was considered to be a violation of the scientific ethos in Victorian Britain (*Electrician*, July and September 1897). In 1899, John Joseph Fahie published a history of wireless telegraphy, from 1838 to 1899. His aim was to uncover the roles and contributions of multiple scientists, telegraphists and engineers in the making of a new industry. Fahie gave credit only to Anglo-Americans and Western Europeans, and structured his narrative in chronological order, avoiding distinguishing between induction, conduction and Hertzian wireless telegraphy. Through an evolutionary understanding of the wireless developments, the only distinction he introduced was between three periods: (a). the period of experimentation, or what he called ‘the period of the possible’; (b). the period when wireless began to be viewed as a practicable possibility due to the emergence of new and innovative research (this period was dubbed ‘the period of the practicable’); and c. the third period, the ‘period of the practical’. In this way, he attributed credit to the natural philosophers and experimentalists who worked on electrical and magnetic phenomena since the first half of the nineteenth century. His narrative ran from 1838, documenting 12 eminent men who “may fitly be called the Arch-builders of Wireless Telegraphy”. Fahie viewed the invention of wireless as a cognitive and technical process, which involved the contribution of different practitioners at the international level. In the last period, ‘the period of the practicable’ – when wireless became a material reality – Fahie identified three main inventors: William Preece, Marconi and Willoughby Smith. He distributed the credit between them with the aim of challenging Marconi’s individualistic account and attempt to represent himself as the solo inventor of wireless (Fahie 1899, xiii). The book was dedicated to his patron, William Preece of the UK Post Office, for many reasons, including his innovative work and as a ‘slight token of esteem and friendship, and in acknowledgement of many kindnesses extending over years’.

Despite the reactions and allegations, Marconi in his public appearances and lectures laid claim to the ownership of wireless telegraphy by stressing his priority in terms of introducing a workable system of long distance telecommunications (Toscano 2012, 77–84). In his Nobel lecture, Marconi (1909) tried to represent himself as the inventor-engineer, the problem-solver of practical problems who understood physicists’ theories and experiments, but whose activities departed from a more scientific experimental practice. He fashioned the role of an ingenious inventor with an inherent inclination towards solving technical problems.

23.3 Marconi Versus British Radio Telegraph and Telephone Company: Managing Expertise, Testimonies and Scientific Advice

23.3.1 Preparing the Case: Advisors and the Shaping of Marconi’s Legal Strategy

The first legal battle in relation to Marconi’s patents in the UK was the case of Marconi Company vs British Radio Telegraph and Telephone Company. It took place in the Chancery Division of the High Court of Justice in front of Lord Justice Parker (Baron Parker of Waddington (1895–1918)) and lasted several weeks – from 12 December 1910 to 21 February 1911 (Agnew 2004). The Marconi Company was accusing a competing corporation of selling a transmitter that used a form of transformer, the so-called autotransformer, which according to the plaintiffs was covered in their well-known patent, No 7777 of 1900.

The Marconi Company asked the advice of James Swinburne, Dugald Clerk, Messrs Carpmael Co and John Ambrose Fleming in order to prepare in the best way for a difficult and important legal battle. The Marconi Company had the resources to secure the advice of important actors both in the Metropolitan circles of advisors, consulting engineers and patent agents, but also in the industrial circles of inventors and the community of electrical engineers. Fleming was someone who had contributed to the development of Marconi's wireless system since the early days (Hong 1996a, 2001). Swinburne was an engineer with an established expertise in power and heavy electrical engineering, and with years of experimentation and practical experience in the study, design and manufacture of transformers (Freeth 1960). The transformer and its function in the wireless system would be under debate during the legal case. The Marconi Company secured the advice of leading patent agents like Dugald Clerk and Messrs Carpmael Co who themselves had made substantial contributions to the making of many important engineering specifications in Britain and the United States. Dugald Clerk (1854–1932) was a mechanical engineer and inventor. In 1888 he established in Birmingham the Marks and Clerk patent agency in partnership with George Croydon Marks (1858–1938). Messrs Carpmael Co was a major Victorian patent agency that had been established by the engineer William Carpmael (1804–1867) (Guagnini 2002; Arapostathis 2013). Fleming's deafness prevented him to appear frequently in the law courts, thus the company secured the services of James Swinburne and Dugald Clerk as expert witnesses in the legal proceedings.

Swinburne argued that Marconi could win the battle, though the fight would be difficult. For the 7777 patent, he said that the case was '. . .clear on all points subject to the harmonics covered by the claims being practical. The infringement is clear; though there will be great deal said to try to prove that they do not use a transformer or jigger at all, probably they will argue that they do not transform up to all but use an arrangement of induction coils which transform down, so that the secondary pressure is less than the primary' (Swinburne 1910). He focused on the need for legitimizing the appropriate readings of Marconi's patent to support his interests.

On 14 October 1910, a report by Dugald Clerk arrived in the headquarters of the Marconi Company. The patent agent and adviser had inspected papers sent to him by Marconi's solicitors, as well as devices supplied by both the plaintiffs and the defendants. Using his expertise on drafting specifications, his contribution was particularly detailed in terms of analyzing the contested specifications. Working as a literary anatomist of patents and published work, he provided an analysis of the subject matter of the contested specifications and he assessed existing common knowledge and its importance in the making of wireless patents. He argued that the 7777 patent was '. . .valid, and the Defendants have infringed. In view, however, of the nature of the infringement, very careful consideration will have to be given to the prior publications of Tesla, Oudin and Braun' (Clerk 1910, 51). The patent agent insisted on advising the company and its legal team to follow a very cautious strategy as the published work of inventors like the three mentioned above could destabilize any priority claim by Marconi and thus result in a legal defeat; Marconi's solicitors should pay particular attention in how they construct the meaning of the term 'transformer'. He wrote, 'It is especially necessary to consider in what manner the word 'transformer' as occurring in Marconi's claim is to be interpreted, in view of the varying arrangements which are possible with auto-transformers' (Clerk 1910, 33). After close reading of the patents, he insisted that Braun's patent of 1899 (No. 1852) was the one that could cause problems to the Marconi side as it described a similar invention to that specified in Marconi's patent. But he directed the solicitors to insist that while Braun had acknowledged the importance of the use of two circuits, the condenser and the one of the antenna, it was not clear in the patent whether he meant to tune the two circuits (Clerk 1910, 25–26). Tuning would have made the difference in giving the system a workable status, while Braun made provision of the use of the transformer '…simply for the purpose of obtaining increased potential in the antenna circuit' (Clerk 1910, 26).

Fleming appears to have had a very crucial role in the management of information, knowledge and expertise as he was called to advise the company not only for the advisability of starting a legal case against supposedly infringers, but also in assessing the advice provided by other consulting engineers. Since 18 April 1910, he argued that the meaning of the term 'transformer' should be broadly interpreted, covering the term 'auto-transformer' too. According to his view, the Directors of Marconi's Wireless Telegraph Company could follow a rather hard patent management policy in relation to Marconi's inventions, particularly at a period that the fundamental Marconi Patent of 1896 (No. 12039) was about to expire (Fleming 1910a, 4). The importance of the meaning of the terms 'transformer' and 'auto-transformer' is something that Fleming had stressed in his reports and notes in regard to the corporate patent policy. Through his influence, the legal team focused on the meanings of the term transformer and the relevant circuits, directing the solicitors' understanding of the wireless systems, their technical details and the way that they needed to be understood in the court so that Marconi could achieve a legal vindication.

Fleming was asked to assess and comment on Swinburne's and Clark's advice. In general, he agreed with their position, argumentation and conclusion. He moved a step further though, arguing that, 'I would suggest that in arguing the case on behalf of the Marconi Company as regards the first patent (Marconi 7777 of 1900), Counsel should first set out clearly the nature of the improvements covered in this patent and the advance made in it over the original method of wireless telegraphy described in Marconi's first British Patent Specification No. 12039 of 1896' (Fleming 1910b). Fleming insisted that Marconi's legal team needed to organize a strategy focused not on the ratio or the scale of the transformation, but on the technical details and characteristics of the circuit that the defendants were using in their system. Supporting and expanding on Swinburne's suggestions, he insisted that the issue at stake for their side was to show that the British Radio Company was using an autotransformer that functioned using transformers similar to those used by Marconi in this wireless system, as well as by other engineers in ordinary electrical engineering works, such as in electric lights and power systems (Fleming

1910b, 6–7). He explicitly urged the lawyers, ‘In arguing this Case I think it is most important that Counsel should present a broad view of the nature of the invention disclosed in this specification. It is not limited to the use of a particular kind of transformer.’ This was a view that Fleming promoted in the corporate setting since a meeting he had with major patent agents and legal advisors Messrs Carpmael in mid-April 1910 (Fleming 1910c). As he revealed to Isaacs, the patent agents believed that the case would not be easy for Marconi. This was a view that they reiterated in their formal report. Messrs Carpmael urged the legal team to organize the witness testimony and the opposition with the aim of creating a narrative about the place of Marconi in the history of wireless. The contribution of other inventors should be acknowledged, but Marconi’s invention should be represented as the one with the workable and efficient system (Carpmael 1910).

23.3.2 Configuring the Meanings of Inventions: Lawyers and Witnesses in the Law Court

The plaintiffs’ legal team comprised J.M. Astbury and J.H. Gray (instructed by Coward, Hawksley, Sons & Chance) while the defendants secured the legal advice and representation of T. Terell and Colefax (instructed by G.F. Hudson, Matthews & Co). Terrell’s strategy of defense against Marconi’s accusations was structured into four main points. First, he insisted that the construction of Marconi’s patent specification was not the correct one. Following a rather common approach, he attempted to show that the specification was incomplete and that it was missing critical information that would facilitate its replication. He insisted that his clients’ specification described an auto-transformer while Marconi’s specification described a transformer: ‘The defendants’ device is an inductive shunt, acting differently from a transformer, and that it was not known as an equivalent of a transformer at the date of the Patent’ (RPDTMC1911, 195). Furthermore, the legal team argued that Marconi’s patent was anticipated by Braun, Lodge and Thompson, and thus what was described in the specification was common knowledge. Terrell rather caricatured Marconi’s practice by arguing that he should not be accredited with any patentable invention as he plagiarized or otherwise used the ideas of other practitioners, engineers and physicists, and added only common knowledge. At best, Marconi’s practice was a synthesis that was far from being innovative and original (RPDTMC 1911, 196). His colleague and team partner, Colefax, concluded their defense by saying: ‘There is great danger of confusing the knowledge of the present time and that of the time of the publication of Marconi’s Specification’ (RPDTMC 1911, 196).

The defendants’ mobilized, among others, W.D. Duddell, E. W. Marchant and W.H. Patchell to support their causes and, most significantly, to establish the distinction between the transformer and the auto-transformer. Duddell, then Vice-President of the Institution of Electrical Engineers, argued that, ‘In 1900 a transformer meant an instrument with two separate coils; and an autotransformer was always distinguished by the use of some prefix' (RPDTMC 1911, 196). E.W. Marchant, who held the chair of Electrical Engineering in the University of Liverpool, argued along the same lines: 10 years earlier, the semantic distinction between a transformer and an auto-transformer was clear in the community of electricians and engineers (RPDTMC 1911, 197). The third major witness, W.H. Patchell, a prominent member of the electrical engineering circles of the period and a well-established consulting engineer, questioned the idea that Marconi's invention covered the auto-transformer principle, while stressing that Marconi's attempt to blur the much-discussed distinction was absurd as it flew in the face of what was established knowledge in the period. He pointed out that, 'In numerous books auto-transformers were treated as distinct from transformers, and they were different from choking coils' (RPDTMC 1911, 197).

The plaintiffs' strategy was orchestrated by Sir John Meir Astbury (1860–1939), an Oxford graduate in law (Trinity College) who, after his studies, worked as a barrister in Manchester before moving in London in 1895, establishing an authoritative legal career with a specialism in patent litigation (Landon and Beloff 2004). Astbury's main line of argument stressed that Marconi was not only the first to patent the principles of wireless telegraphy, but also – and most importantly for the case – built a workable telegraphy system. He said, 'In 1896 the messages could only be sent 1¾ miles; in 1897 the distance was increased to 15 miles; in 1901 to 2,000 miles; and recently a message was sent from Clifden to South America, about 6,000 miles. Marconi alone achieved any real success with wave telegraphy' (RPDTMC 1911, 192). Marconi increased the distance of signal transmission, an important practical step in the development of telecommunications that secured him important contracts with the Admiralty in 1901, the UK Post Office and the Canadian and Russian governments in 1904, and agreements with the Board of Trade and the Trinity House a year later. Those exclusive contracts not only guaranteed Marconi's contribution in patenting ideas and innovative devises, but also established him as someone whom major state actors could trust to work with and whose system of telecommunication it was worth adopting. The other line of support, as executed by Astbury, was to try to set a distinction between discovery and invention. He linked the first with theoretical and experimental advances of wireless, while the second – relevant to Marconi's invention – was related to technological inventions and telecommunications systems.

As mentioned before, James Swinburne and Dugald Clerk did not only advise Marconi's legal team, but they acted as expert witnesses to support the company. Swinburne appeared first and tried to capitalize not just on his authority in the electrical engineering community and in industry, but also on his practical expertise in the design of transformers and electrical machinery in general. He testified that Marconi's invention differed from those of Tesla, Lodge, S.P. Thompson and Braun due to the achievement of tuning and the use of a 'two-independent circuit transformer'. He appeared fair by acknowledging that Lodge's 1897 patent was a 'great advance' because it directed engineers and practitioners to the importance of syntony between the transmitter and the receiver. He insisted that Marconi's innovation supplemented Lodge's by introducing a closed circuit with a large capacity into Lodge's invention (RPDTMC 1911, 193–194). Swinburne's arguments were supported by Clerk's testimony. The renowned patent agent focused on Lodge's (No 11,575 of 1897) and Braun's (1899) patents. Marconi's invention could achieve the necessary train of waves and increase the energy in a more efficient way than Lodge's invention. While perusing Braun's patent he exercised his authoritative status as patent agent and thus as co-author of several inventions – and insisted that the specification was incomplete, lacking substantial information to enable the modification of the apparatus in directions that would secure the increase of the distance of transmission. Through the agency of expert witnesses like Swinburne and Clerk, the Marconi side attempted and managed to set the stage for the judicial legitimization of the 7777 patent as a valid patent covering a workable technology. The combination of practical and techno-legal authorities framed the problem in the patent dispute and determined the verdict.

23.3.3 Parker's Verdict: Assessing Witnesses and Historical Accounts

Parker's legal reasoning was inscribed in his verdict that was structured into three parts: the first was a historical reconstruction of wireless, the second was a critical assessment of the contested patents and specifications, and the final was an attempt to place the case and the dispute in the legal culture and practice of the period. Parker's judgment was based on trusting experts' testimony, giving credit to 'virtual witnessing' (Shapin and Schaffer 1985, 60–65) as it was constructed in the patent specification. Despite the defendants' preparation of a small experimental installation that, according to them, would clarify the technical details of the dispute, the judge focused on the assessment of witnesses' testimony and the documents submitted concerning the disputed parts. While he tried to provide a coherent judgment, he wasn't always successful. His reasoning was shaped by the agency and performativity of expert witnessing and this led to inconsistencies in parts of his verdict.

Parker treated his verdict as a matter of storytelling – of historical narrative – rather than as a verification of experimental findings. Indicative of this is that he started the verdict as one of the historiographers of wireless telegraphy (RPDTMC 1911, 199). He made a distinction between wireless and inductive telegraphy and, based on this, he reconstructed the story of wireless telecommunications. The historiographical emphasis was on the cumulative activities of practitioners and an attempt was made to distinguish between theoretical, experimental and practical contributions. His brief account started with Henry – in 1839 – who introduced the first theoretical principles of wireless, then continued with Kelvin and Helmholtz, who proved Henry's theory. The next stop was Maxwell's theory that etheric disturbances would be distributed through space in every direction and continued with Hertz's experimental demonstration of Maxwell's theoretical understanding of the creation of waves. Parker attributed to Crookes and Lodge the credit, first, for setting the vision of wireless telecommunications and, second, for popularizing the potential and the technical difficulties encountered by practitioners of the period. Marconi had a place in Parker's story, as the inventor whose innovations resolved the technical difficulties. He represented Marconi as a problem-solver and practitioner with an understanding of the issues and with an ability both to synthesize and to solve critical problems (RPDTMC 1911, 199–201).

After presenting Marconi as the inventor of wireless, he moved to a close consideration and analysis of the specifications of the relevant inventions (RPDTMC 1911, 210). The issue at stake was the interpretation of the term 'transformer'. Lord Parker based his judgment largely on the testimony of the expert witnesses, and Swinburne's practical authority and credibility was given particular weight:

> The name 'transformer' was originally given to instruments used for stepping up or stepping down the voltage of an electric current, and had direct reference to the effect produced ... The instrument originally, and most generally, used to step up or step down voltage had two distinct and separate coils....An instrument so arranged might, however, serve other useful purposes, as, for example, where it was desired to avoid in a working circuit any metallic connection with the source of supply, though it was not desired to step up or step down the voltage, and an instrument so arranged became known as a '1 to 1' transformer, that is a transformer which strictly speaking, had no transforming effect, but had other uses. As soon as the word 'transformer' came to be used as including an instrument which had no transforming effect on the current, the word not unnaturally began to connote, not any change in the voltage, but a transformation of electrical energy in another circuit; in other words, that the current in the secondary was an induced current. (RPDTMC 1911, 215)

Parker insisted that it was inconceivable that '... an electrical engineer, in say, 1899, would have any doubt that what could be done by an air-core two-coil transformer could also be done by an air-core auto-transformer' (RPDTMC 1911, 216). With reference to the practices of the 'competent engineer' of the period, he stressed that a broader reading and understanding of the specification would have resulted in the resolution of the problem. Understanding the 'literary technology' (Shapin and Schaffer 1985, 25–26) of the specification in a narrow way would not be necessary to provide a workable telecommunication system. The necessary step was to understand the general idea in Marconi's patent (RPDTMC 1911, 217). In a very dramatic tone, he concluded his verdict with a most clear and strong statement: 'Being of opinion that every claiming clause of Marconi's Patent of 1900 is a claim for an entirely novel combination, producing an entirely new and useful result, and that the use of a two-coil transformer is no essential part of his invention, I hold that the Defendants, who, in my opinion, have taken all the essential parts of the invention, are infringers, notwithstanding that they have substituted an auto-transformer with an air-core for any such purpose as that for which Marconi has used the transformer may have been new' (RPDTMC 1911, 219).

23.4 Conclusion

The case study in this paper concerned the making of a patent, and a strong monopoly, through the decision-making process of the British courts. In a period when his master patent (British Patent, 12,039 of 1897) was about to expire, Marconi won an important legal battle, further securing his dominant status in the British market. The story shows that Marconi's success in the court case was the result of preparation and organization, and the use of experts that combined scientific, practical and legal credibility. Despite the ideologically driven public discourse on the cognitive and social superiority of science over invention and practice, in the law courts a mixture of scientific authority and practical experience provided credible witnessing. While in US patent disputes Marconi appeared as witness in the patent office and the courts, in the British case he preferred to use the regimes of trust and credibility that were common in the British judicial system. Marconi knew that the British case was not a case of priority claims but the issue at stake was to establish the legitimate reading of the specification. Instead of appearing in the courts as the authoritative Nobel prizewinner for his contribution in the development of wireless, he preferred to use experts that could really promote his causes and service the case in the most appropriate way.

The chapter has explored the role of Ambrose Fleming in advising and directing the legal team, through directing their attention to specific aspects of the contested patent and the most appropriate interpretations of the specification for Marconi's interests. Fleming did not mention these contributions in his autobiography. He referred only to his scientific research and inventions, but not the techno-legal dimensions of his consulting duties. As a Cambridge graduate in mathematical physics, he fashioned the role of scientist-engineer (Hong 1996a). Thus, he directed attention away from any hybrid techno-legal practices necessary for his consulting duties. This despite the fact that from his reports and advice it is clear that he understood invention as an activity with cognitive, technological, economic and legal aspects.

The case study typifies the dynamic context of patent disputes in early twentieth century Britain. The detailed analysis of the judicial process has shown that both science and practical experience counted as substantial qualities in the established regime of trust in the British courts. Swinburne was trustworthy because he was established as credible witness due to his vast practical experience. Dugald Clerk was an effective expert witness because he combined engineering expertise with legal authority and experience in the literary construction of specification. As the case study has shown, trust in expert witnesses was a process influenced both by existing traditions of authority and by the performance of the experts. The management of expertise and the rhetorical and literary technologies were an integral part of the adversarial system. The law courts were 'trading zones' where actors, experts and stakeholders interacted. In this adversarial setting, the judges acquired the 'interactional' expertise that combined both an understanding of the patent law and an understanding of engineering problems and practices. Parker was immersed in the language and culture of invention through the agency of lawyers and witnesses like Astbury and Swinburne. Expert witnesses, lawyers and judges played important mediating roles in shaping the culture of invention and in translating innovations into legally robust inventions. During the preparation of Marconi's legal case and along the law court proceedings there was a process of construction of both the subject and the object of the innovation and the relevant technology and technological practice inscribed in the 7777 patent. While the invention acquired meaning, identity and legal status, the inventor's practice was constructed too (Barry 2001, 104–123). In this context, we should talk of the co-production of law and technology (Jasanoff 2004).

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Chapter 24
Curating the European University

Hans-Jörg Rheinberger

Abstract The paper takes a critical look at the present situation of the European university and traces its history over the second half of the twentieth century. The assessment leads to a statement about the mission of academic education, both past and future. The paper concludes with reflections about the task and function of research for the institution of the university.

Keywords European university • Disciplinary change • Research • Critical institution

There appears to prevail and endure a widespread concern over the European university. The general undertone, if I see it correctly, is that there is not only something to curate in this respect, but first and foremost something to cure. But if there is something to cure, something must have gone wrong in the first place. To begin with: is there, or should there be, such a thing as THE European university? Of course, there are already institutions in different European countries that have been in place for shorter or longer periods of time and carry the name "Europe" in their designation. We have, to name a few, the European University Institute in Florence, the Central European University in Budapest, The European University Viadrina in Frankfurt/Oder, and more. However, most of the institutions carrying the label "European" in their name are - business schools.

This, however, is not the point to be made here. The question concerns higher education in Europe in general, including the traditional, classical universities as well as those established more recently over the course of the past century.

In their little book with the allusive title *Beyond Excellence*, the Belgian education scientists Maarten Simons and Jan Masschelein-addressing not only the European university in general but also a future world university (*Welt-Universität/ université mondiale*)-proceed from a clear-cut distinction between what they call

For Kostas Gavroglu on the occasion of his retirement. This essay takes up an argument that was presented in 2011 at the Catholic University of Leuven in a public debate on "Curating the European University."

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the "historical university" of the past and the contemporary "entrepreneurial university." Here is what they claim:

> The historical university works as a machinery that modernizes on the basis of the generation of facts and through a knowledge-based cultivation of society. The entrepreneurial university cares less about facts or questions of values, but rather about resources and products. It is a machinery that transforms, in a creative manner, human and other resources into products (competences, technology, etc.) in order to subject society to permanent innovation. (Masschelein and Simons 2010, 38)

Although it appears suggestive, if not as a *fait accompli*, then at least as a tendency, I would like to delve behind this apparently clear-cut dichotomy. I am inclined to claim that it is based on a diagnosis that essentially remains at the surface of the changes we see happening in our universities today. In fact, I will argue that changes of this kind are not restricted to the turn of the twenty-first century, but have been going on throughout the past century along with its scientific revolutions.

Let me state right from the beginning that my considerations concern the graduate level of our universities, not the undergraduate education that has obviously caused so much trouble in many places over the past 15 years in the name of what we have come to call the "Bologna Process." Driven by virtue of a questionable claim to comparability, this process must remain on the outside of things, based as it is on only numbers and quantifications. It was my conviction, right from the beginning, that it was absolutely unnecessary to erase so many of the local European study traditions and to organize European university education in the name of equalization. In my opinion, two "rules of thumb" would have sufficed. First, no European student should leave university after their masters degree without being able to speak at least three European languages fluently. Second, no European student should leave the university after their masters degree without having spent at least 1 year at a university of a European or other country of their choice, other than their home country. Paradoxically, if I see it correctly, the "Bologna Process," with its tight study schedules, has on the contrary had a tendency to discourage mobility instead of enhancing it.

In order to convey my main message let me, in a first step, recall briefly my own student days in the late 1960s and 1970s at the Free University of Berlin. I happened to arrive there in the spring of 1968 and studied on both sides of the big divide between the two cultures of the humanities and the sciences: philosophy and linguistics to start with, biology and chemistry as a follow-up. The verdict against the traditional university in those times was surmised under the motto that was chanted again and again, as some of you might recall: "Unter den Talaren, Muff von tausend Jahren" (under the gowns, the niff of a 1,000 years). The general impression was that the university had closed itself in upon its traditional disciplines. Its main occupation was to tread its own worn paths, having lost lively contact with the real world around it, both intellectually as well as in terms of the future work perspectives of the students. A few remarks must suffice here to make the point. The students of philosophy were under the impression that the really interesting and challenging questions were not dealt with in the philosophical canon officially in place. It came from vibrant intellectual centers such as California or Paris. Looking back, I must confess that we spent the greater part of our time as students of philosophy in self-organized reading groups, not in traditional seminars. Among the texts that were read and enthusiastically discussed were those of Michel Foucault, Jacques Derrida and Paul Feyerabend, to mention only three of many. They were all authors who found themselves on the fringes of their respective disciplines, on the verge of opening them, of moving them, and above all marginalized by the grail-holders of the disciplines. These authors claimed to practice a new grasp of history in general and history of science in particular (Foucault), occidental philosophy and literature (Derrida), and philosophy of science (Feyerabend). They were perceived to be in touch with the intellectual grass-root movements the students themselves felt they were a part of: not strictly academic movements, but intellectual ones in the sense of being part of and commenting, in all rigor, upon aspects of one's own time, the time now and the time ahead. Besides the cross-disciplinary perspective, there was also a practical aspect: it was expressed not least in the vigorous pursuit of dissemination by translation. We were learning by translation and materially, at least in part, living from it.

The picture is not completely different when it comes to the life sciences I studied in the 1970s. Throughout those years, while learning textbook knowledge in lectures and courses, a heated curricular discussion was going on, pushed by the students and backed by a few sympathetic professors. *Mutatis mutandis*, it revolved around the same issues that had moved things on in philosophy. Contemporary biology was felt to be undergoing deep changes, without these changes being represented at the level of the organization of academic learning. For instance, in light of molecular biology the distinction between zoology and botany no longer appeared to make sense – an introductory course into general biology would thus be needed, one that would replace the traditional introductory courses to botany and zoology respectively. The impression was that frontiers of research were no longer adequately reflected by the established structures of knowledge transmission. Project learning and learning by practicing research was put on the agenda. The connection to society – professional practice, "Berufspraxis" as it was called – was on the agenda as well.

Boundaries were called into question, "boundaries of the impossible, of the 'maybe,' the 'as if,' the 'when,'" as Jacques Derrida addressed them in *The University without Condition*. "This boundary," he there concludes, "is the place at which the University is exposed to reality, the forces of the out there (be they cultural, ideological, political, economic or other forces)" (Derrida 2001, 76). These are at the same time the boundaries, as Derrida incessantly reminded us, of deconstruction. And these boundaries were just as much at stake then, at the university 50 years ago, as they are today.

There are in fact two boundaries: one is negotiated from within-the frontier boundary of research-and one is negotiated from without-the boundary of science and society, of science in society. But both are engaged in an incessant negotiation: both boundaries are by necessity continually redrawn and reoriented. This boundary work marks the essential strength of the position of the university in our modern world. I cannot see that the situation of today is a radically new one. The accents vary, to be sure, but the university was-and remains-this peculiar boundary object as long as its *autonomy in questioning* these two boundaries remains intact. The drive, in the last instance, must come from research, research that forces the redrawing and renegotiation of, first and foremost, the academic disciplines as they have come to be established. It must remain the privilege of the students, in each generation anew, to *call for* the necessary renegotiations in both directions.

In order not to leave these claims in the abstract, let me give an example of shifting research boundaries from the realm of the sciences: biology. The life sciences appear themselves to be a living example of displacements which, in retrospect, must be valued as nothing less than dramatic, if not revolutionary. Biology had emerged from the nineteenth century as a formation *sui generis*, relatively well consolidated in comparison with physics and chemistry, and at whose center, for a good part of the century, the disciplines of botany and zoology continued to be located. Yet, in the course of the second half of the nineteenth century, another branch of biology, namely physiology as the science "of the phenomena of life common to animals and plants" as the nineteenth-century French physiologist Claude Bernard (1974) put it, had emerged and was becoming prominent. To physiology was added, toward the end of that century, experimental developmental biology, and the beginning of the twentieth century was marked by the meteoric rise of a latecomer in the association of biological disciplines: genetics. Physiology, developmental biology and genetics formed the core of what, in the course of the first decades of the twentieth century, came to be called "general biology" (see, e.g., Hartmann 1927).

As the twentieth century progressed, this landscape again was forced to profoundly change. Two hybrid sciences first sounded the terrain anew and then put up for renegotiation the boundaries between biology, chemistry and physics. One of these was the hybrid science of biochemistry, whose rise in the 1920s and 1930s was closely linked to a new form of investigating biological processes occurring within cells: the characterization of enzymes and other biologicals, such as vitamins and hormones, in the test tube. Biochemistry presented itself as biology "in vitro." The other hybrid was biophysics, which emerged in the 1930s and 1940s. This development coincided with the coming into being of a new generation of research technologies with whose help the structure of biological macromolecules could be investigated. Among the technologies characteristic for this period were ultracentrifugation, electron microscopy and X-ray crystallography.

Molecular biology emerged thus around the mid-twentieth century. It established itself as an amalgamation of biophysical and biochemical techniques with questions and problems arising from genetics. In molecular biology and at its core-molecular genetics-, physics, chemistry and biology came to be articulated in a completely new form. Precisely from this constellation arose an equally new, unprecedented vision of what makes life different, of biological specificity. The nucleic acids, DNA in particular, were located at its center and it resulted in the creation of a new conceptual frame. It accreted around concepts of genetic information and genetic programming. With the so-called "dogma" of molecular biology-"DNA makes RNA, RNA makes protein"-the biological sciences as a whole were set on a new foundation. This led, in the late 1950s in the United States and in the 1960s in Europe, to the deep-running reorganization of the life sciences at the universities mentioned earlier, with the molecular structure-function relationships of the cell at its center.

In the 1970s, it was this new, molecular biology that gave rise to gene technology. With the prospect of a technological manipulation of the molecular foundations of life, new interfaces were again created and boundaries reconfigured. Molecular biology ceased to be an esoteric enterprise of a community of pure basic researchers; it was rather transformed into a field on which economic and social interests began to be combined with the prospect of a technological development of these sciences in relation to medicine and agriculture. The human genome project was the epistemic expression of this new constellation, its economic expression being the development of a biotechnology industry, that in parallel formed a previously unknown close relationship to academic research. This situation led to new social, cultural and ethical questions that concerned the application of gene technology and reproduction biology in human medicine, human procreation and agriculture, as well as in the production of foodstuffs and renewable primary products. It led again to a renegotiation of the double university boundary, and it is on developments such as this that Simons and Maschelein's diagnosis concerning today's universities is based.

This picture, however, would be essentially incomplete without mentioning two further areas connected with the multifaceted development of a molecularized biology. There is, first of all, the field of molecular developmental biology, which today—in making use of the methodological arsenal of gene technology and bioinformatics in parallel—is on the verge of transforming itself into a new systems biology. Second, there is the molecular take on the so-called "higher" functions of the organic, in particular, the performance of the human brain. The debates of past years, not only about the nightmare of the reproductive cloning of humans but also about man's free will and its limits, are only the most visible expressions of a reconfiguration that has seized the totality of the life sciences and positioned them, as far as the future image of humanity is concerned, as the leading sciences of the twenty-first century, both technologically as well as culturally.

It is therefore not by chance that a university like the Free University of Berlin has merged biology, chemistry and pharmacy into one and the same faculty. Another large university in Berlin, the Humboldt University, is in the process of creating- as one of its big steps into the future 200 years after its inception -a broadly conceived "Institute for Integrative Life Sciences." At this institute, the molecular life sciences, the theoretically oriented biological sciences such as systems biology and evolutionary biology, human biology, but also the social and human sciences where they intersect with the life sciences, are supposed to enter into a productive exchange. Today, there is hardly any of the relevant problems in the life sciences that would not require, in order to be tackled, a number of competences that half a century ago either did not exist, or were distributed over a number of neatly separated disciplines. In the context of the molecular biosciences, this has been the case for half a century in physics, chemistry and biology. Today, this also includes more and more the social sciences, if for instance one considers the problems that a geneticized medicine will present in the future. It is also true for the humanities in the narrower sense, in particular for the philosophical and historical disciplines concerned with the shaping of a responsible and responsive image of what it means to be human. All of these specialties, for a long time encapsulated in tightly constricted academic corners, are called up, not to fuel already existing competences into a clearly defined project, but rather to interact with each other in a highly dynamic, scientific field of ongoing research and in such a way that their specific productive capacities can be fruitfully deployed.

This much appears to be clear: the sciences of the twenty-first century will no longer be bound by disciplinary frontiers that started to come into existence toward the end of the eighteenth century, that became so characteristic for the sciences of the nineteenth century and that were binding for the university-based sciences well into the twentieth century. Just as in the nano-sciences, the information sciences, or the life sciences, these disciplinary boundaries are losing their importance. The relationship between basic science and applied science is also in the process of being reconfigured. In this latter context, there is more and more talk about a new hybrid entity called "applied basic research" (see e.g. Carrier and Nordmann 2010). As always, we might say, when boundaries have become fluid and new, at least meta-stable patterns that could replace the older ones are not yet in place, such patterns will emerge as a result of the ongoing process.

This brief historical assessment shows that since the inception of its modern form in the eighteenth century, research in this particular area of the life sciences has given rise to permanent -sometimes in-depth- reconfigurations of the research process, meaning that the universities have had to permanently respond to this challenge. As a consequence, the disciplinary divisions have been constantly shifting, as far as the boundaries from within are concerned, as well as those from "without," a without at least from the perspective of an *incumbent* change, as the youngest example just mentioned drastically shows. But what it means to be "in" and what to be "out" is also not a fixed quantity given once and for all.

There is but one thing for which I would plead in all these displacements and in all the ongoing epistemic reconfigurations of present and future knowledge: the universities have to remain those spaces in which a kind of research continues to be possible and prevalent that draws its primary motivation from handling objects of *knowledge*, *epistemic* things as it were. What I plead for thus is the primacy of the interest of knowledge-*Erkenntnisinteresse* in German-in university research. This implies a plea for the creation of structures that allow future professors and students to follow knowledge interests also under present conditions of a rearticulation, such as that in the sciences of life, of the worlds of academia and business. As Derrida claims, we need to keep on being committed to the "idea that this space of an academic type would have to be preserved symbolically through a kind of absolute immunity" (Derrida 2001, 45).

There is one all-important reason for such obstinacy: in the past, and this will remain so in the future, genuinely new developments never have been and never will be definable from the perspective of goals that may be clearly anticipated at a given time. Instead, they simply *happen*, in the strongest sense of the word, they are unprecedented. They happen in the context of research trajectories which, as a rule, have a highly non-linear character and resist strict time regimes. Such trajectories need appropriate conditions to be able to unfold in the name of a science open toward a horizon that is not given at any time, but created in conjunction with the development of the sciences themselves.

The French philosopher of science Gaston Bachelard once claimed that the modern sciences as we know them are a veritable paradigm of historicity. "The history of the sciences," he concluded, "appears as the most irreversible of all histories" (Bachelard 1951, 27). He then went on to claim that from the moment the sciences began to have such a history, the scientific mind accordingly also had to assume a historically changing structure. To be scientific, then, does not mean finding the right place in an axiomatic structure, but rather being involved in a process of evolutions and revolutions. The scientific mind pluralizes and diversifies itself precisely in its concentration on particular issues. "Sitting in judgment," the scientific spirit "condemns its historic past. Its structure is its awareness of its historical errors. For science," Bachelard concludes, "truth is nothing other than a historical corrective to a persistent error" (Bachelard 1984, 172). The sciences find their final justification in the historical structure of their replaceability, which means that they are "by essence and not by accident, in a permanent state of crisis" (Bachelard 1984, 160).

Consequently, a veritable historical epistemology, conceived as the effort to assess and understand the historical movement of the sciences, must be in a permanent state of crisis itself. Permanent crisis in the production of knowledge and permanent crisis in the reflection of knowledge thus imply each other. The universities are the privileged places in our societies where this double crisis, of knowledge production and of knowledge reflection, is perceived and felt, acted out, kept going - and confined. Derrida means exactly this when he talks about the "privileged position of the philosophical, inside and outside the humanities, from where the university thinks itself and imagines itself" (Derrida 2001, 64). Universities, as the privileged institutions of knowledge production, are thus themselves critical institutions, institutions in permanent crisis, and with the need to be understood as such. Knowledge societies not only can afford, they are indeed critically dependent on such places of fermentation.

Universities are often seen, first and foremost, as places of knowledge transmission and there surely exist, at any given time, stocks of knowledge that need to be handed over, transmitted. But there is no productive transmission without transformation. This was the great idea of Wilhelm von Humboldt: a university whose structure simply functions according to the canon of knowledge of the time is a place that cannot cope with the future. In order to be able to cope with the future, a university is needed whose structure functions according to research, that is, according to the *not yet known*. Claude Bernard once claimed: "It is the vague, the unknown that moves the world" (Bernard 1954, 26). In other words, there is positivity in ignorance (Merton 1987), and it is the noblest task of the university to enable its students to deal, at the frontiers of present knowledge, with the unknown.

Thus to study, in an emphatic sense, does not mean to absorb the quantum of existing knowledge, but rather to take note and make use of that knowledge in order to push its frontiers a step forward into the as yet unknown. It is in this sense that universities, at their very center, must deal with and connect to what I have called “epistemic things” (Rheinberger 1997), the things of the not yet, the things of the yet to come, the things that will happen in research. They are the things whose advent, whose adventure we should say time and again, “presupposes an irruption or an outburst that blows up the horizon, that interrupts every performative regulation, any convention and any context dominated whatsoever by conventionality” (Derrida 2001, 72). It is the privilege of the university to be that place in which, in the realm of the *epistemic*, such unconventionality is made possible in a non-chaotic and channeled manner. Scientific research in general can be seen as being the movement in which the sciences realize this un-controllable immanent transcendence in a regulative fashion. Experimentation is nothing but the synonym of that movement.

Few philosophers have expressed this more clearly than Martin Heidegger. In his *Die Zeit des Weltbildes* he put it in a nutshell:

> The essence of what we today call science is research. In what does the essence of research consist? In the fact that knowing establishes itself as a procedure within some realm of what is, in nature or in history. Procedure does not mean here merely method or methodology, for every procedure already requires an open sphere in which it moves. It is precisely the opening up of such a sphere that is the fundamental event in research (Heidegger 1977, 118).

As far as the humanities are concerned-not only the humanities but the humanities in particular, to put it somewhat paradoxically-a good portion of history is needed to open new spheres in relevant ways. In other words: in order to look ahead, we also have to look back. To the surprise of many, toward the end of his pronouncement on the universities, Derrida framed this call in the form of a confession:

> The humanities of tomorrow will have, in the totality of their areas of expertise, to study their history; the history of the concepts that construed the respective disciplines, and that will say, the concepts that founded these disciplines and were coextensive with them (Derrida 2001, 66).

For Derrida, this is the history of an “as if,” the history of research and, with that, “of a certain structure of scientific objects in general” (Derrida 2001, 30). For a historian of science like myself, it is deeply satisfying to see Derrida in his late reflections on the university having recourse to the kind of historical epistemology Georges Canguilhem was promoting in his time, and to whom Derrida, as it happens, had been an assistant.

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Chapter 25
Can Science Make Peace with the Environment? Science, Power, Exploitation

Angelo Baracca

> *Higher intelligence is an error of evolution, unable to survive for more than a short instant in the evolutionary history (Ernst Mayr)*
> *The sleep of reason produces monsters (Francisco Goya)*
> *Nature to be commanded must be obeyed (Francis Bacon)*

Abstract This chapter develops a criticism of the current ideological conception of science as a purely cognitive activity, one which objectively reflects the true structure of nature and is superior to other forms of knowledge as the result of its rigorous method. Science is, instead, a historical process, arising from concrete persons who act in concrete environments. Modern science is a product of Western society in its capitalistic phase of development and thus has subsumed the same logic of exploitation both of nature and of the human labour force. The vast majority of scientists under every regime have acted as accomplices of power. This attitude is exacerbated by the falling rate of profit in the current crisis and is becoming absolutely unsustainable. Technique has become a "second nature," deeply conditioning our lives and acting as a diaphragm with respect to nature. To orient science toward human development and truly ecological purposes, it is currently more important to analyse the limits, rather than the undeniable power, of science, its drawbacks rather than its benefits. An appalling portion of scientists work on war programs, making war increasingly terrible, and they are not denounced inside the scientific community. In this framework, the chapter analyses in particular the threat to global human health conditions—the "Epidemiological Revolution of the 20th century," the inadequacy of the prevailing reductionist medical paradigm, and the need for a new biomedical paradigm and practice.

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Keywords Science as a historical process • Science, technology and nature • Scientific ideology and exploitation, scientists as accomplices of power, crisis • Environmental issues and epidemiological transition

25.1 Blatant Contradictions of the Ideology of Science and Progress

I wonder why, especially in the conditions of the present economic and social crises, nobody seems to perceive and denounce the blatant contradiction between the promises lavished for decades by the ideology of scientific "progress" and reality! According to the stereotype, Science and its applications should irreversibly increase well-being and standards of living and health, lighten the burden of work, and solve the main problems impending on humankind. The contrast with reality could hardly be more striking: the living conditions of hundreds of millions of people are dramatically deteriorating, unemployment spreads, labour is precarious, young generations have very scanty perspectives, health conditions are getting worse in many developed countries (and more so in underdeveloped ones), not to mention the impending challenges of the impoverishment of basic resources, the global crises, and the persisting danger of a nuclear war!

Something has to be deeply revised in the standard conceptions (or stereotypes). The very concept of *progress* needs to be put under close scrutiny, as should that of *development*, usually conceived in purely quantitative economic terms of gross domestic product (GDP), a concept that hardly overlaps with the real well-being of people. One should recall that on 18 March 1968, in a talk delivered at the University of Kansas, Robert Kennedy voiced the criticism that the concept of GDP measured everything "except that which makes life worthwhile" (Kennedy 2008). In the absence of a serious critical analysis of these concepts, one can hardly wonder about the mounting of antiscientific positions, against science *tout court*.

In fact, any kind of critical remark about science and progress is usually received by the scientific community and the establishment as an *irrational*, antiscientific position. On the contrary, a genuine scientific rational attitude calls for a deep analysis of the limits of Science, whereas an uncritical acceptance of a stereotype (or ideology), with unlimited confidence in it, is exactly the opposite and is at the basis of many of the epochal problems humankind has currently to tackle. Any approach acquires its full reliability and efficacy if one knows its limits, its field of application: only then can one be confident about its uses and applications. Otherwise, the risks of improper applications, of unexpected and undesired results or side effects, are very concrete.

Left-wing forces in the industrialized countries have adopted, beyond scientific ideology, a concept of *productive forces* as independent from the *relations of production*. At the basis of this conception there is literally the *sell-off of work to capital*. The concept of productive forces relies on the interpretation of (Western) Science and Technology (capital letters are deliberate) as absolutely objective, intrinsically progressive, capable of themselves to solve any problem.[1] Such interpretation offers the justification, although inevitably partial, for every kind of intervention on nature, provided that it is performed on the basis of "scientific," "rigorous" methods: man aims to rule nature, its mechanisms and equilibriums, in a delirium of omnipotence. Some scientists have claimed to be "better than God," directing biological evolution, in contrast to nature, which proceeds rashly. The tricky attribute "ecological" is ascribed to artefacts, as if "ecological" cars or televisions grew from trees!

25.2 Specificity and Historicity of Scientific Knowledge as a Human Activity

A first mystification is the concept of Science as *the* superior form of knowledge as a result of its rigorous quantitative method and rigorous experiments. Science is instead just *one of many forms of knowledge*, endowed with its own specificity, irreplaceable in its field of application, but even dangerous when extrapolated outside that field (as often happens). Science cannot be considered superior to, for instance, philosophy, without which Science would be blind, or art, without which life would be very sad! The quantitative mathematical approach is not superior in itself: each approach is suitable, and necessary, under certain circumstances, and for certain tasks. It is nonsense to evaluate the "quality of life" with purely quantitative parameters.

Our everyday behaviours and choices are based on empirical experience: only Science needs exact measurements of rigorously defined quantities, but this would be useless, and counterproductive, in common life. Scientists themselves do not apply the same attitude to all aspects of their life (and adopt scientifically wrong expressions, such as the Ptolemaic expressions of "sunrise" and "sunset").

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[1] I recall the prevailing positions in the Italian Communist Party and the Unions in favour of nuclear power, opposed to the majority of the Italian population, which in two occasions (1987 and 2011) voted massively against it.

A recent instance of dogmatic acceptance of the dominant scientific ideology is given by the resolution approved in November 2012 by the Spanish left-wing political coalition *Izquierda Unida*, which rejected *all* "natural" therapies, inasmuch as not "scientifically based." The decision indiscriminately bundled up such disciplines as Bach's Flowers, osteopathy, homeopathy, acupuncture, reflexology, and the traditional Chinese medicine. The latter is based on thousands of years of rigorous observation and checks, although it is based on "scientific" criteria that are different, but not necessarily worse, from the modern scientific approach: ignoring that Western medicine, apart from being associated with *one* particular "scientific" approach, supports the big interests of the medical class and the pharmaceutical industry. In contrast, in Cuba—where the modern biomedical and biotechnological sector underwent a big development at top world levels—"natural," or "green," medicine has been promoted, especially in the deep economic difficulties following the collapse of the Soviet Union.

Science, moreover, is not an absolute, ahistoric category, but a very specific product of the activity of persons who have a peculiar social role, operate in historically determined conditions, and participate in (and are conditioned by) the cultural currents and the problems and aspirations of their time. The contents, methods, and paradigms of Science have deeply transformed (not simply deepened) in the course of time. For example, the eighteenth-century mechanical worldview was but a conceptual "cage," which reflected a cultural paradigm and a level of cultural and social development[2] that was overcome not merely for scientific reasons, but when the problems and perspectives arising from the productive and social changes brought about the need for new scientific approaches.

In fact, Science does not "reflect" Nature, conceived as something immutable existing beyond Man (obviously not intended in a gender sense) in absolute and objective terms. Nature is never captured by human experience in an "immediate" way, that is, without mediation.[3] Man, instead, as a member of a specific economic social formation, relates to Nature in historically determined ways, which define both the context of the forms and level of knowledge and the level of exploitation of Nature.[4] Science is not a purely speculative activity but is a product of human (social) activity incorporated in practically every artefact, from the "carved stones" of our early ancestors up to the atomic bomb, or drones. It cannot be artificially separated from the purposes and social role of such artefacts: an integral, reliable assessment of science cannot leave aside its uses, applications, and implications.

25.3 "Western" Science and the Exploitation of Nature and the Human Labour Force

Modern Science, quantitative and mathematical, is a product of Western society, in its capitalistic phase of development. Although other social formations had produced previously very advanced scientific knowledge, they did not feel the need for comparable quantitative approaches. *Having developed in the context of the capitalistic economic social formation, (Western) Science has subsumed in its structure and methods the same logic of exploitation of nature and of human labour force, peculiar to capitalistic society*. Other economic social formations developed other forms of scientific knowledge and skills, adopting completely different attitudes, and producing qualitatively different results, with different potentialities. Just to

[2] I refer to the essay of Baracca et al. (1979).

[3] Marx and Engels wrote in *The German Ideology* (1845), polemizing with Feuerbach: "the sensuous world ... is, not a thing given directly from all eternity, remaining ever the same, but the product of industry and of the state of society ... in the sense that it is a historical product"; "The cherry-tree, like almost all fruit-trees was, as is well known, transplanted by commerce into our zone, and therefore only by this action of a definite society in a definite age [it has become "sensuous certainty" for Feuerbach]" (Marx and Engels 1970).

[4] These aspects were developed in details in an Italian essay: Baracca and Rossi (1976).

quote one example, traditional Chinese science chose a *holistic* rather than a *reductionist* attitude, aiming not at the productive transformation of nature, but at the knowledge and preservation of the complex and delicate equilibria of, and between, man and Nature[5]: the traditional Chinese medicine, for instance, is an intrinsically preventive one.

(Western) science has adopted the attitude of explaining natural processes to master, modify, and exploit them. Its "rigorous" method has reached a tremendous efficacy: conceived as a superior form of knowledge, it has legitimized any artificial transformation of nature. In a true delirium of omnipotence, the scientist (implicitly or explicitly) harbours the presumption of "being better than God," because he is able to guide those transformations which happen blindly, accidentally, or inefficiently in nature. This attitude is more exacerbated in present unbridled neoliberalism, for which the erosion of the margins of profit has led to the extreme exploitation of every resource, natural as well as social, including social services and "rights."

25.4 The New Frontier of the Economic Exploitation of Nature: "Natural Capital" and "Biodiversity Offsetting"

The increasing warning against the unsustainability of the present exploitation of nature does not result in repentance. On the contrary, "Increasingly . . . nature *is* actually money. The contemporary moment of global crisis in both ecological and economic spheres is also the moment wherein 'Nature' is being refashioned as '*Natural Capital*.' Key interlocking elements are thus joining the previously rather separate domains of economics, business, and finance, with ecology, environmentalism, and conservation."[6] They serve the *commodification* of nature. Similar to carbon offsetting*, biodiversity offsetting* is the promise to replace nature destroyed and lost in one place with nature somewhere else, and relies on 'experts' to create dubious calculations that claim to make one piece of the earth equal to another: the introduction of biodiversity offsetting allows, or even encourages, environmental destruction with the promise that the habitat can be recreated elsewhere.[7]

Governments, as well as the World Bank,[8] are actively engaged in developing such projects, which obviously are presented as Science based. The nonsense of

[5] Along with Joseph Needham (1900–1995) (Needham 1954), the peculiar social structure of "mandarinate" did not express the need for a quantitative kind of knowledge.

[6] *Sian Sullivan, The Natural Capital Myth*, http://ppel.arizona.edu/blog/2013/03/15/natural-capital-myth#_edn1

[7] "No to Biodiversity Offsetting!", http://no-biodiversity-offsets.makenoise.org/

[8] For example, for the UK, http://www.fern.org/Ukbiodiversityconsultation; the EU No net loss initiative, http://ec.europa.eu/environment/nature/biodiversity/nnl/pdf/Subgroup_NNL_Scope_Objectives.pdf; the World Bank Study of Biodiversity Offsets, http://www.profor.info/sites/profor.info/files/docs/OFFSETS-PUBLIC%20INFORMATION%20NOTE.pd8f

such claims is easily unmasked, if one simply considers that a huge number of species is still unknown, and the concept of biodiversity is consequently ill defined and impossible to quantify.[9]

25.5 Scientists: A Social "Caste," Accomplices of Power

Scientists operate at a social level as a "corporation," a *lobby* (even a "caste"), which purports to be the depository of a superior form of knowledge, acknowledged to be the "truth," and which explores its powerful implications deriving from this, reinforced by a deep practical power, inaccessible to common people. *The "caste" of scientists has acted (with a few, but praiseworthy exceptions) as an accomplice of the dominant entrepreneurial class*. In every phase of capitalistic development its activity was directed towards the increasing exploitation of labour force and natural resources, and the solution of problems and contradictions arising in stages of crisis, through scientific and technical innovations. *The exploitation of man and of nature are just two sides of the same coin*.

A few examples: scientific changes in the organization of labour and in productive technologies have constantly increased productivity, assisting recovery from economic crises, reinforcing the subordination of the working class to the capitalistic cycle and its exploitation, and disregarding the safety and health of workers. The (ab)use of chemicals in agriculture has greatly increased incomes and profits while at the same time impoverishing both the fertility of land and the nutritional quality of foods as well as harming human health. An emblematic case is the multimillionaire, practising Catholic, unscrupulous Swiss citizen Stephan Schmidheiny, owner of the Eternit empire, who is responsible for the major part of the worldwide pollution and deaths from asbestos, and who has actively supported the scientific lobby that has concealed the truth about its deadly effects.[10] The scientifically "programmed obsolescence" of commodities is one more example: no scientist informs consumers of these tricks.

Even war is a powerful means of conquest, the extension and reinforcement of capitalistic domination. It is seldom recalled that an impressive percentage of the scientific community works in, or for, the industrial military complex, exclusively engaged in designing and producing new and more deadly armaments (even suggesting new conflicts). Striking examples are the atomic bomb, or *drones*, which are extensively used for illegal murders, transforming outstanding political

[9] Literature on the subject is countless; see *86 Percent of Earth's Species Still Unknown?* for example, http://news.nationalgeographic.com/news/2011/08/110824-earths-species-8-7-million-biology-planet-animals-science/ ;*Most Earth species 'still unknown', Brazil expert says, Feb 26, 2013,* http://phys.org/news/2013-02-earth-species-unknown-brazil-expert.html

[10] An historical verdict, the first in the world, from the Court of First Instance of Turin (Italy) on 13/02/2012 convicted the top brass of Eternit to 16 years of jail. They have obviously appealed against the verdict.

leaders in the world into true criminals, although protected by the international political community.[11] It is hardly necessary to recall the fundamental contributions made by Nobel laureates to basic military innovations: now that the dramatic events in Syria have brought to public attention the case of chemical weapons, we can recall that the early chemical stockpile of the German empire, on the eve of the First World War, was achieved under the direction of the next Nobel Laureate in chemistry (1918), Fritz Haber, with the collaboration of the Noble Laureate (1920) Walther Nernst (the case of nuclear armaments is better known).

Capitalism has generated and enhanced the contradiction between human labour force and natural forces,[12] *and science has been a powerful and obliging means towards* **the** *leading class. The contradiction between man and nature—which in my opinion is undeniable—is subordinated to the main contradiction between capital and labour.*

25.6 Limits of Scientific Knowledge

The dominant ideology only insists on the *power* of Science, on its unlimited potentialities, on its capability of solving every problem, which is equivalent to the presumed capability of the market to solve every contradiction. Given the deep power acquired by science, and the correspondingly great responsibility of scientists, it is instead more important to acknowledge and analyze its *limits*. This criticism does not mean to belittle its value, as any tool can be trusted only inside its validity range.

Science necessarily needs to circumscribe its field of study and limit the number of magnitudes to define and measure, in order to establish scientific laws. But this legitimate procedure unavoidably leaves outside an infinite quantity of other aspects, which can be progressively studied but never exhausted. The limits of the "scientific method" are therefore intrinsic to any discipline, and it is extremely serious to forget them.

I have emphasized the incompleteness of our definition of biodiversity. Another significant example is given by drugs: what do they mean, in fact, those "undesirable" "side effects" that are usually specified in the informative leaflets of drugs? (A term grotesquely extended to military actions.) A drug is studied for a specific symptom, and then it is realized that it affects, sometimes seriously, other functions

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[11] The "museum of horrors" of the development of innovative military developments goes beyond imagination: it sounds awful, as an example, that after the artificial drones, there is a race to develop "insect cyborgs," flying by remote control! (see Anthes 2013).

[12] "Labour is, in the first place, a process in which both man and Nature participate, and in which man of his own accord starts, regulates, and controls the material re-actions between himself and Nature. He opposes himself to Nature as one of her own forces, setting in motion arms and legs, head and hands, the natural forces of his body, in order to appropriate Nature's productions in a form adapted to his own needs. By thus acting on the external world and changing it, he at the same time changes his own nature." (K. Marx, *Capital*, 1867, Vol. I, Cap, 5, Sect. 1).

and organs. Such a procedure is exacerbated by the business attitude of the pharmaceutical industry. Drugs should be much more carefully tested before being commercialized:[13] "this industry uses its wealth and power to co-opt every institution that might stand in its way, including the US Congress, the FDA, academic medical centers, and the medical profession itself." (Angell 2004, 2009). It seems evident, however, that the "scientific procedure" provides a legitimization of such a practice.[14] In general, thousands of new synthetic molecules are produced and introduced in the market every year, very often without appropriate tests.

This basically *reductionist* approach is partially acknowledged by some scientific disciplines that try, at least in principle, to take into account from the outset the intrinsic *complexity* of every natural system. The need for a radically different general approach, that is, a *holistic* one, is increasingly acknowledged (e.g., Capra 1982).

25.7 Technique: An Artificial "Second Nature," a Diaphragm

In view of the violence exerted on nature, the increasingly wide and invasive applications of Western science, that is, technology, have built *an artificial "second nature" in which we live and act*, and which constitutes a *diaphragm* with respect to nature. Our daily and professional life depends on artificial products and devices: manufactured articles that keep a remote memory of their natural components, but incorporate artificial mechanisms that even seem to elude natural laws. This process is exacerbated by the creation of induced, unnecessary needs (through a brainwashing of invasive and deceptive advertising): happiness is falsely associated with futile, however sparkling, gadget-objects. Human relationships and introspection are substituted by bewildered wandering in shopping centres. We are proud of this artificial reality, but we run away from it whenever possible, in search for improbable "lost paradises," which are in turn masterpieces of artificiality. Even *virtual realities* are created, which lead (perhaps with the help of some psychedelic substance) one's mind to wander away from the chilling artificial reality. It has been ascertained that some people, if they are prevented from having access to the Internet during various days, suffer a sort of syndrome of abstinence and feel lost.

Nothing is really "natural" in our existence. Sounds, colours, smells, flavours, foods are artificial.[15] Children know animals (apart from dogs and cats) only

[13] The side effects of drugs and improper therapies are not a negligible cause of death (Fox News 2007; Maugh II 2008).

[14] Not to speak of patients, or entire populations, used as guinea pigs to test the effects of drugs or other substances.

[15] In China, for instance, sophisticated and expensive air-purifying systems and domes spread (Toh et al. 2013).

through television images. The green in our cities is a gloomy simulacrum of nature. Our contact with the outer world elapses in great part while shut up in "a metal box with wheels," and we feel proud, even if we are locked in a traffic jam. Our time flows with a frantic pace of running after business, without any relationship with the rhythms of nature: we should recover the value and pleasure of "wasted time." We are flooding the planet with all kinds of waste, among which technological-chemical-radioactive waste constitutes a permanent and insoluble problem (apart from feeding illegal trades). On the other hand, we have not been able to control old innovations such as cars, which under the powerful capitalistic business have turned from comfort to slavery (but one that we are not able to get rid of, notwithstanding the serious damages to health, mainly of children, ignored not only by politicians and administrators—*et pour cause*—but also by the public at large).

This increasingly impermeable and indecipherable diaphragm not only separates us from nature, but distorts it as does a deforming lens, even "denationalizing" it. In a "delirium of omnipotence" we feel proudly skilful handling complex electronic equipment, but we would hardly survive in a jungle (even after a "survival course"; with all our sophisticated technical means, we cannot rival "primitive" men who have never seen a computer or a mobile phone). In fact, we have lost the ability of reading the language of nature, we perceive nature as opposed, if it is not mediated through technique: we have to resort to extreme sports to artificially defy it.

Needless to say, as with every other aspect of capitalistic society, there is another side to this reality, that is, the millions of people who are excluded, and marginalized, from this false but sparkling well-being: those who were the "reserve army" but are at present destined to unemployment.

25.8 Myths and Reality of "Progress"

The dominant ideology insists on the improvement of well-being and all social indices resulting from "progress" and growth, such as, for instance, the prolongation of life expectancy, or the defeat of diseases and epidemics that mowed down so many people in the past. I will leave aside such well-known scourges as world famine, or Third World diseases such as malaria (and the lack of access to the lifesaving drugs in underdeveloped countries).[16]

[16] Reservations concerning the claimed benefits of human "progress" are not limited to industrial societies. For instance, the "Neolithic revolution," with the transition of humankind from gathering and hunting to agriculture, usually considered as the first stage of human civilization, has been deemed by the anthropologist Jared Diamond as "the worst error in the history of human species" (Diamond 1997). Another anthropologist, Tom Sandage, confirms that this transition worsened human life, because the hunters-pickers had a more balanced diet and were healthier than the farmers (life expectancy is estimated to have lowered from 26 years for the former to 19 for the latter (Sandage 2009).

Populations living in environmental conditions of strong pollution, or workers employed in the presence of dangerous substances or in conditions of strong stress, show considerable increase of disease and shortening of life expectancy. Moreover, if a prolongation of life expectancy in developed societies can be true in statistical terms, some important aspects must be specified. In the first place, this index strongly depends on the social, economic, and environmental conditions, and definitely worsens when the latter worsen, even for specific classes of citizens or workers. After the applauded collapse of the Soviet Union, life expectancy in Russia dropped considerably for the next 15 years.[17] The same is happening among the popular classes of the countries most strongly hit by the economic crisis (Nikolas 2013; van Dijk 2014), beginning with Greece.[18] In the United States, average life expectancy is considerably shorter among the coloured population in comparison with the white population. But for white women and men lacking a high-school diploma, life expectancy was shortened by 5 and 3 years, respectively, between 1990 and 2008 (Tavernise 2012).

Moreover, official statistics hide other aspects. Even if life expectancy has increased in the past decades in developed countries, *in Italy the expectancy for a healthy life has decreased* (Gennaro et al. 2012). Another astonishing example, from an authoritative journal: "The life expectancy table (Salomon et al. 2012) ranks healthy life expectancy at birth for men in Israel at ninth worldwide, compared with 86th for men in the neighbouring Occupied Palestinian Territory. Corresponding ranks for women are 12th and 97th respectively. This astonishing gap highlights yet again the apartheid-like regime that is in place in the Occupied Palestinian Territories" (Shahin 2013).

25.9 New Concept of the Health Effects of Environmental Changes: The "Epidemiological Transition/Revolution of the Twentieth Century" and Threat to Global Human Health

A radically new approach is necessary to tackle the problem of health conditions and the development of diseases in connection with the worsening of global and local environmental conditions.[19] In fact, the widely prevailing paradigm of official

[17] "Life expectancy of the Russian Federation since 1950." Demoscope.ru. 26 April 2011. Retrieved 14 May 2011; Stuckler et al. (2009), http://www.thelancet.com/journals/lancet/article/PIIS0140-6736%2809%2960005-2/fulltext

[18] Greece, life expectancy at birth, http://www.thelancet.com/journals/lancet/article/PIIS0140-6736%2809%2960005-2/fulltext

[19] A systematic discussion, with an extremely wide bibliography, is given in an Italian monograph by Ernsto Burgio addressed to a medical organization, from which we have extracted the biomedical information synthesized in this paper (Burgio 2013). An English revised edition is forthcoming.

medicine deeply underestimates the effects of the environmental factors on health: an attitude that provides a strong support to the big interests of the pharmaceutical industry. However, a new medical paradigm is being developed, although still a minority, and facing strong resistance (besides diffuse conservatism and ignorance), based on careful observations and on the newest discoveries of biological sciences, which are revolutionizing the previous concept based for half a century on the "fundamental dogma," according to which information flows from DNA towards proteins and phenotype but not the other way around. A radically new systemic *epigenetic* conception is developing, of a dynamic, plastic, and interactive genome as a network in which the DNA contains the *potential genetic program* (a sort of *hardware*) shaped during millions of years of biological evolution, and surrounded by an extremely complex cloud of molecules (a sort of *software*), which is continuously changing in response and adapting to the hexogen information and stimuli from the (external and internal) environment. These reactive/adaptive changes induce the concrete expression and the phenotypic actualization of the pluripotent potential information contained in the DNA.

In this new framework, the deep and rapid (local and global) changes produced by human "civilization" in the environment must necessarily have a direct and permanent effect on health conditions, although they have been neglected or deeply undervalued by the medical community. Not only are children obviously the most affected but also evidence is accumulating that these effects are already produced in the foetus, whose cells and organs are under formation and rapid change, and moreover these effects can be transmitted even to future generations (transgenerational).

In this context, a completely different view of the health situation arises. On the basis of increasing evidence and data, the concept of the *Epidemiological Transition/Revolution of the 20th–21st Centuries* has been proposed. A radical change is taking place, in which on the one hand there is really a dramatic reduction of the acute, infective, and parasitic pathologies, which have been devastating human life for millennia, and shaping our defence systems; but, on the other hand, a rapid and consistent increase (accompanied by an increasingly precocious appearance) of chronic-degenerative, inflammatory, and neoplastic pathologies has been taking place in past decades, first in the North of Europe and the United States of America, and then at a global level, presently representing by far the prevailing causes of death. Indeed, one century ago almost 45 % to 50 % of deaths were caused by infectious diseases (tuberculosis, 10 %; respiratory infections, 20 %; diarrhoea and acute gastroenteric diseases, 5 %; other infectious diseases, 13 %), in contrast with almost 13 % from cardiovascular pathologies and only 2 % to cancers (McMichael 2001). After a century the epidemiological situation appears reversed, at least in the north of the planet, or better in proportion with the degree of development (WHO 2010). Indeed, now deaths caused by cardiovascular pathologies amount to almost 30 %, and those caused by cancer seem to be nearing that proportion even as the age of their onset is becoming lower (WHO 2003), whereas the "classical" infectious diseases have almost disappeared (although experts and health authorities call attention to new infectious emergencies, such as dengue, West Nile virus, and Chikungunya, and to the potentially pandemic influential *orthomyxovirus*).

Moreover, the most recent data show a dramatic increase on endocrine-metabolic (obesity and diabetic pandemics) and neural-degenerative (Alzheimer) pathologies, allergies, and neurodevelopmental (autism) and neuropsychiatric diseases.

In November 2006, an article published in *The Lancet* by a paediatrician and an epidemiologist of the Harvard School of Public Health (Grandjean and Landrigan 2006) denounced, among general indifference, a possible connection between the continuous release into the environment of potentially neurotoxic molecules and agents, and "silent pandemics" of neural-psychical damages involving 10 % of the children of the so-called First World (a problem that some researchers had pointed out since the early 1960s). Not to mention the food frauds, and more generally the (legal) use of artificial substances in foods, in particular in children's snacks.

In what concerns scientific criteria, standard methodologies of epidemiological and toxicological risk assessment are not sufficient for understanding the ongoing epidemiological revolution, and thus inevitably undervalue health effects. Toxicology, on the one hand, mainly considers the health effects of single agents, although we are simultaneously exposed to the synergic effects of innumerable pollutants. Moreover, it is increasingly evident that several agents are toxic even in infinitesimal doses, because of their mechanisms of action (including, in particular, cumulative epigenetic mechanisms), whereas higher doses seriously damage cells, causing the onset of death mechanisms. On the other hand, epidemiology was mainly conceived for the evaluation of the effects of massive, accidental expositions, while it seems utterly inadequate to assess a situation of collective, daily, and most of all indirect (transplacental and transgenerational) exposure (acting mainly in particular subjects, including children) to an enormous variety of chemical and physical artificial agents, which in their majority have never been adequately tested against their possible health effects. With respect to carcinogenesis, the widely prevailing medical theory during the past two decades is genetic origin.

25.10 Towards a New Biomedical Scientific Paradigm and Practice

We are brought back to the already discussed reductionist approach of science: it is increasingly evident that a deep change of scientific paradigm and practice is necessary and urgent. In an extreme synthesis (Burgio 2013, and references therein), the fundamental evidence is that the specific genomic program of the individual is formed during the 9 months of embryonic-foetal development, on the (reactive-adaptive) basis of the information coming from the environment. The environmental factors induce therefore an (epi)genetic stress, which promotes an altered foetal programming in the development of tissues: the latter belatedly shows itself as diseases, such as cancer, but is also at the basis of the worldwide worrying pandemics such as obesity and autism. It is interesting to remark that these views represent a *neo-Lamarckian* point of view, in contrast with the dominant *Darwinian* concept.

Our present scientific attitude towards health is well represented by the fact that we have undertaken a "war" against viruses and bacteria: a "war" that mankind risks losing and can turn out to be seriously counterproductive. The (ab)use of antibiotics, for instance, is increasing resistance, which can hardly be coped with by continuous new drugs[20] (Sommer et al. 2009; de Quetteville 2012; Martínez 2012): obviously the big interests of the pharmaceutical industry strongly push in this direction.

There is a more fundamental reason for this nonsense. Microorganisms are actually the essence of the biosphere, which has originated from them and could not survive without them (they are 60–90 % of all biomass in weight, depending on the inclusion of cellulose, and 90 % of the cells of the human body). Actually, we are not "individuals," but complex symbiotic systems, in which the microbial system coevolves with us, and unrolls basic functions, not only at a nutritional/metabolic level, but even for the correct performance of our immunocompetent system (Burgio 2013, Chap. 18; Gilbert 2005; Lederberg 2006). Further, 8 % of our genome is constituted of retroviral endogen sequences, and more than 50 % operates in strict connection with these sequences.

A real primary prevention would be a true revolution in medicine, much more effective and cheaper than present medical practice, but it would drastically clash with the interests of Big Pharma.

25.11 Humankind on the Brink of Self-Extinction?

The few aspects I have very schematically mentioned should reinforce doubts about the concept of so-called "progress," and its claimed benefits, in particular in terms of technical scientific advancement. The epidemiological revolution, the silent pandemics, and so on, could reliably be the dramatic consequences of the rapid and deep changes of the environment produced by humans, and the consequent attempts by the embryo-foetus of epigenetic (reactive-adaptive) reprogramming of organs and tissues.

Indeed, *we are performing the biggest experiment of (man-made) transformation of the biosphere*, one with no return! We are artificially transforming, in the course of decades, the "genosphere," the genetic heritage of 4 billion years of biological evolution on our planet. These transformations add to the other epochal global challenges that humankind is facing—such as climatic change, or the danger of nuclear war (Baracca 2012), topics I do not discuss in this chapter.

[20] In 2008 it was calculated that almost 90–180 million kg/year are used, enough for 25 billion complete treatments: they are mainly (ab)used in the agricultural/zootechnical field.

25.12 Genetics, Biotech Industries, Biological Weapons, Bioterrorism, and Pandemics

The development of biotechnologies is widely supported by an extremely powerful pool of multinationals (Monsanto, Syngenta, Bayer, Dow, DuPont) with the purpose of controlling the global seed market and posing a great threat to the life of humankind and of the planet (Shiva et al. 2012).

The inconsiderate ("scientifically" planned) development of genetic and biotechnologies may constitute one of the main risks for humankind, one that is wiping out the boundary with military and terrorist applications. As I have mentioned, the "sorcerer's apprentice," "bio-Strangelove," presuming to be "better than God," is deeply manipulating the basic molecules and systems that regulate life and which have been naturally evolving throughout 4 billion years: these abrupt artificial modifications, for good or evil, risk triggering transformations that nobody will be able to control.[21] "The true danger at present is that a biological global war breaks out without anybody being able to prevent it, rather than for the deliberate will of somebody. ... [It is impossible] to distinguish between defensive and offensive uses of researches on micro-organisms and, at least since the 1980s, the huge interests connected with the new sector of genetic biotechnologies." (Wright 2002).

The great "repented biotechnologist" Mae-Wan Ho has denounced the relationships between research on biological weapons and recurrent pandemics[22]: "These and other experiments in manipulating viral genomes are now routine. It shows how easy it is to create new viruses that jump host species in the laboratory, in the course of apparently legitimate experiments in genetic engineering. ... *geneticists can now greatly speed up evolution in the laboratory to create viruses and bacteria that have never existed in all the billions of years of evolution on earth*. ... The dangers for the biosphere do not stem from a bad use of biotech, i.e., from bioterrorism and biological wars, but from a technology which breaks the species-specific barriers

[21] As I know, there are not many analyses with this approach. My direct reference is again to Ernesto Burgio (2003). Some further references are given in the following.

[22] Concerning pandemics, it is convenient to remark that, in the situation in which we are, the occurrence of a true and deadly one, which could mow down millions in death, caused by the rapid adaptation and changes of the *orthomyxovirus* strain (which originated the "Spanish influenza" that caused between 20 and 40 million deaths in 1918), and the possibility of its "species jump," is only a matter of probability, that is, of time. In this respect one has to denounce once more the irresponsible behavior of *Biopharma* and the authorities which support its business, in declaring fake pandemic warnings, inducing the preparation and selling of millions of vaccines: when the true pandemics explode, the vaccine could be late in arriving, and moreover could be in part frustrated by further rapid transformations of the virus. Much more effective against the spread and the virulence of the disease, and beneficial for the whole community (but evidently opposed to the main economic interests), would be the worldwide reinforcement of health structures, hygiene, and prevention, beginning with the poor and underdeveloped countries.

that nature has built in defence of single living species." (Ho, no date, 2003; Ho et al. 2005).

In connection with these aspects, it is worth mentioning a quite disquieting circumstance. On July 18, 2003, it caused a sensation in the world media when Dr. David Kelly, 59, a biological warfare weapons specialist, with a senior post at the Ministry of Defence, was found dead after seemingly slashing his wrist in a wooded area near his home at Southmoor, Oxfordshire. This doubtful case is far from isolated. Evidence has been provided of as many 88 scientists and microbiologists found dead from 1982 to 2005 (Harper 2005; this number increased to almost 100 to 2006 (Harper 2006): "While some of these deaths may be purely coincidental and seem to pose no connection, many of these deaths are highly suspicious and appear not to be random acts of violence. Many are just plain murders." One could add the more than 310 Iraqi scientists who have perished since the fall of Baghdad to U.S. troops in April 2003: the suspicion is directed towards the agents of MOSSAD (Israeli Secret Service).

Science is paradoxically at the same time the most powerful means to find solutions for the big problems, and the cause of the creation of new and increasingly uncontrollable ones, at a rhythm perhaps greater and at a deeper level than the problems they solve. It is urgent to embark on a deep discussion of these aspects—which kind of (basic and applied) research, which methodological and practical approach, which goals, and so on—with the information and involvement of the wide public, all around the world, because the fate of everybody is at stake.

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