Chapter 4
Ernst Cassirer, Kurt Lewin, and Hans Reichenbach

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There has recently been an upsurge in interest in Ernst Cassirer, the neo-Kantian-trained German philosopher whose philosophical and scholarly writings spanned the first four decades of the twentieth century. Historians of philosophy have come to recognize the influence that Cassirer and other Neo-Kantians had on early analytic philosophers. Most prominently, Alan Richardson, Michael Friedman, and André Carus have all argued that Cassirer was a significant influence on Carnap's work up through the *Aufbau*.[1] There are good reasons for historians to emphasize the relationship between Cassirer and the early work of Carnap and other members of the Vienna Circle. Carnap wrote his dissertation under a Neo-Kantian, Bruno Bauch, defending the viability of a broadly Kantian philosophy of space. Moreover, Cassirer was one of the most prominent—if not the most prominent—German philosopher of the exact sciences in the opening decades of the twentieth century. Easily the most subtle and mathematically well-informed of the Neo-Kantians, he was among the vanguard of early twentieth century philosophers seeking to understand the philosophical significance of the revolutionary advances made in logic, mathematics, and physics. Not only did Cassirer write some of the earliest philosophical works on general relativity,[2] but he was one of the first German academic philosophers to give serious attention to Russell's logicism and the new

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[1]Friedman (1999, Ch. 6), Friedman (2000), Richardson (1998), and Carus (2008). For a helpful (though by now a bit out of date) overview of this literature, see Ferrari (1997).

[2]Cassirer (1921/1923).

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logic,[3] Dedekind's foundations of arithmetic, and to Hilbert's axiomatic foundation of geometry.[4] Cassirer's commitment to a philosophy of science that engaged with cutting edge science ran deep and was widely known. For example, as a letter from Reichenbach makes clear, Cassirer was the only philosopher to sign onto a petition, composed by Reichenbach in 1931 on behalf of the Gesellschaft für empirische Philosophie, petitioning the German government to create a chair in the philosophy of science.[5] It is not surprising, then, that the younger generation of philosophers of science, such as Carnap, would look to Cassirer's work as an inspiration (and target).

In fact, as John Michael Krois has discovered, Cassirer felt intellectually closer to the philosophers of the Vienna Circle than to any other school. In an unpublished work from in the late 1930s, Cassirer writes:

> In "worldview," in what I see as the ethos of philosophy, I believe that I stand closer to the thinkers of the Vienna Circle than to any other "school"—
>
> The striving for determinateness, for exactitude, for the elimination of the merely subjective and the "Philosophy of feeling;" the application of the analytic method, strict conceptual analysis—
>
> These are all demands that I recognize completely—[6]

However, there is good evidence that the historical connection between Cassirer and the Berlin Group is at least as strong, if not stronger, than that between Cassirer, Carnap, and his other Viennese colleagues. In fact, in the short article entitled "Historical Remarks" from the first volume of the journal *Erkenntnis*, Cassirer is given as an influence on the Berlin Group (though he is not mentioned in connection with the Vienna Circle):

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[3]Cassirer (1907).

[4]Cassirer (1910/1923, Chs. 2–3).

[5]Letter from Reichenbach to Cassirer, 5 June 1931, HR [025-11-04]; 15 June 1931, HR [025-11-03]. These letters are reproduced in the CD-ROM accompanying Cassirer (2009). The other signers were prominent scientists, such as Hilbert and Einstein, and prominent industrialists. The 15 June letter concerns the best way of formulating the petition, with Reichenbach noting that Hilbert wanted him to present the petitioned chair as an oppositional counterweight to the unfortunate trend in German philosophy away from the philosophy of science. Reichenbach then commented to Cassirer: "I believe that you could not imagine how deep and widespread the animosity is among natural scientists to the prevailing trend in philosophy; it is in fact only your name that is excepted from this judgment."

[6]This text is from a document titled "Zur 'Relativität der Bezugssysteme'," housed in the Cassirer papers at Yale University. The text is quoted in Krois (2000). I don't believe that this quotation shows that Cassirer felt a closer affinity to the Vienna Circle than to the philosophers of the Berlin Group. First, it is not clear whether Cassirer is distinguishing Reichenbach from the Viennese philosophers in this quotation. Second, the Berlin philosophers never formed a 'school' in the way that the Vienna Circle did. Third, all of the values that Cassirer attributes to the Vienna Circle and claims for his own appear just as clearly in the work of Reichenbach and Lewin. (Moreover, many of the *doctrines* that are distinctive to the Vienna Circle—contained for instance in Carnap et al's "The Scientific Conception of the World"(1929/1973)—Cassirer rejected out of hand.)

> The [Berlin Group] concentrates above all on problems from logic and physics as a starting point of an epistemological critique (starting points in Kantianism and Friesianism, influence of Cassirer and Nelson).[7]

In this chapter, I will substantiate this thesis by exploring the influence of Cassirer's thought on two Berlin philosophers: Kurt Lewin and Hans Reichenbach. I choose these two figures in particular for two reasons. First, both took courses with Cassirer while students in Berlin—Lewin in 1910,[8] and Reichenbach in 1913.[9] Second, both explicitly discuss Cassirer's philosophy on multiple occasions in their writings.

The chapter will have four sections. After giving some historical and biographical material about Cassirer, Lewin, and Reichenbach, I'll discuss in Sect. 4.2 the ways that their conceptions of philosophy—as giving an analysis of the sciences—overlap. In the third section, I'll address the relationship between Cassirer's philosophy of science and Lewin's own, emphasizing how Cassirer's philosophy of science provided a theoretical framework for Lewin's research program in experimental psychology. In the fourth and final section, I'll look briefly at the relationship between Cassirer's and Reichenbach's evolving conceptions of the *a priori*.

4.1 Some Biographical Material

Cassirer, Reichenbach, and Lewin enjoyed close personal and intellectual relations throughout their careers. Their interactions began at the University of Berlin, where Cassirer—having completed his dissertation under Hermann Cohen in Marburg in 1899—was a Privatdozent from 1906 to 1919. He moved to Hamburg in 1919, when anti-Semitic academic policies were loosened under the Weimar Republic, and he remained there until 1933, when the Nazi take over forced him into exile. In 1910, Kurt Lewin—a first year graduate student interested in biology and the philosophy of science—took a philosophy of science course with Cassirer, who was just that year publishing *Substance and Function*. In reading a course paper Lewin wrote, Cassirer challenged him to check one of his philosophical claims to see if it was true about psychology. This piqued Lewin's interest, and Lewin soon switched to psychology, completing his dissertation in psychology under Carl Stumpf.[10] After the Great War, he became a Privatdozent in Berlin in 1921. (Like Cassirer, he was kept from a Professorship because he was Jewish.) He joined the Psychological Institute in Berlin, where he stayed until 1933, when he went to America. (The Psychological Institute in Berlin was during the 1920s

[7] Neurath (1930, 312).

[8] Marrow (1969, 9).

[9] See the autobiographical remarks in Reichenbach's 1916 dissertation: Reichenbach (2008, 149).

[10] Marrow (1969, 6).

the home of Gestalt Psychology, and its senior members were the leading Gestalt psychologists, Köhler and Wertheimer.) During his Berlin years, Lewin published works both in experimental psychology and in the philosophy of science, and his teaching alternated between psychology and philosophy seminars. Indeed, as we will see shortly, since he was a working psychologist, his philosophy of science was integrated into his experimental work.

Kurt Lewin is known today as one of the twentieth century's most influential and innovative psychologists, commonly regarded as the founder of modern social psychology. Though he wrote widely in the philosophy of science during his Berlin years,[11] he is best known among historians of philosophy because of his interactions with Reichenbach and the other members of his circle. Lewin's relationship to the Berlin Group is a bit complex. He was not a founding member of the Berlin Group; or at least he is not in the list that Reichenbach gives in his 1936 paper "Logical Empiricism in Germany"—a list that includes only Reichenbach, Dubislav, Herzberg, and Grelling.[12] But Lewin was listed in 1931 (though not 1930) as one of six members of the Executive Committee ("Vorstand") of the "Society for Empirical Philosophy."[13] Lewin delivered a paper to the Society in 1930, which was later published in the first volume of *Erkenntnis*.[14]

Lewin always acknowledged his intellectual debt to Cassirer. This influence is clear in Lewin's philosophical writings, beginning with his earliest writings from his student days. It is patent, for instance, in an early manuscript from 1912, "Erhaltung, Identität und Veränderung in Physik und Psychologie." In this work, which contains in germ two of the main ideas of Lewin's mature philosophy of science—the project of comparative philosophy of science and the notion of "genidentity"[15]—Lewin refers freely and repeatedly to Cassirer's texts and ideas. Moreover, Cassirer's influence extended beyond Lewin's overtly philosophical writings to his experimental psychological research as well. In a 1949 paper for Cassirer's Schilpp volume, Lewin wrote

> [S]carcely a year passed when I did not have specific reason to acknowledge the help which Cassirer's views on the nature of science and research offered. The value of Cassirer's philosophy for psychology lies, I feel, less in his treatment of specific problems of psychology—although his contribution in this field and particularly his recent contributions are of great interest—than in his analysis of the methodology and concept-formation of the natural sciences.[16]

Reichenbach's personal and intellectual relationship with Cassirer goes well beyond the fact that his 1920 Habilitation thesis gave a kind of Neo-Kantian

[11] These writings fill two hefty volumes of Lewin's *Werkausgabe*.

[12] Reichenbach (1936, 143).

[13] Neurath et al. (1931, 310).

[14] Lewin (1931a). This paper was translated into English, with some deletions and occasional additions, as Lewin (1931b). (Citations to this paper will be from the reprint in Lewin 1999.)

[15] On "genidentity," see Lewin (1922).

[16] Lewin (1949). (Citations will be from the reprint in Lewin 1999). See p. 23.

philosophical interpretation of General Relativity. Already in 1914, Cassirer recommended Reichenbach (who could not write his dissertation under the Privatdozent Cassirer) to his mentor and fellow Marburg Neo-Kantian Paul Natorp.[17] The extant correspondence between the two, contained principally in the Reichenbach papers at the Archive of Scientific Philosophy at Pittsburgh and reproduced as part of Cassirer's *Nachgelassene Manuskripte und Texte*, shows a personal and professional relationship that lasted throughout their academic lifetimes. The first extant letter, dated 14 June 1915, concerns Cassirer's (unsuccessful) attempt to convince the editors of *Kant Studien* to publish Reichenbach's dissertation.[18] In his last letter to Cassirer, dated 2 April 1945 (11 days before Cassirer's death), Reichenbach—then a professor at the University of California, Los Angeles—tries to convince Cassirer to take up a position with him in the University of California.[19] Letters from the 1920s document Cassirer's repeated (unsuccessful) efforts to help Reichenbach find a professorship in a philosophy department, and in a 30 September 1930 letter, Reichenbach asks Cassirer to consider sending a paper to the new journal *Erkenntnis*.

4.2 Lewin, Reichenbach, and Cassirer on the Logical Analysis of Science

Let's begin with some words about Cassirer's, Reichenbach's, and Lewin's conception of philosophical methodology. In Reichenbach's 1936 paper, he argued that the "sole significant advance" made by the logical empiricists in Germany was the development of a new "philosophical method in the form of an analysis of science."[20] This method, Reichenbach claimed, was first put forward in his 1920 book, *The Theory of Relativity and A Priori Knowledge*. This method, Reichenbach contends, is akin to Kant's regressive method, but aims to give an analysis of our best scientific knowledge instead of an analysis of our reasoning faculty.

> The program for a philosophical method in the form of an analysis of science was first published, within the context of the movement under discussion, by the author in 1920. What he demanded was the introduction of a method of analysis of science (wissenschaftsanalytische Methode) into philosophy. This was opposed to the Kantian conception of philosophy as a method of establishing conclusions by an analysis of "reason." It was maintained that the Kantian method at its best was nothing else than an analysis of Newtonian mechanics in the guise of a system of pure reason. According to the new view put forward, "reason" was to

[17] Frederick Eberhardt and Clark Glymour, introduction to Reichenbach (2008, 2).

[18] Thanks to Simon Huttegger and Sabine Kunrath for helping me to decipher Cassirer's handwriting in this letter.

[19] Reichenbach wrote: "I have here [at UCLA] a group of talented students interested in my ideas, and they would all be pleased to study with you." These letters are reproduced in the CD-ROM accompanying Cassirer (2009).

[20] Reichenbach (1936, 142).

> be grasped only in the concrete form of scientific statements [. . .] Advancing immediately to realize this program, the present writer entered into a detailed analysis of Einstein's theory.[21]

In a footnote from Reichenbach's 1920 book, he claims that a method akin to his own logical analysis of science appears in the work of two other philosophers: Kurt Lewin, whose "scientific orientation" is the same as Reichenbach's own, and Ernst Cassirer.[22]

Not surprisingly, Lewin casts a similar vision for the philosophy of science (what he calls "Wissenschaftslehre" [theory of science]) in his programmatic 1925 essay, "On the Idea and Task of Comparative Wissenschaftslehre." He writes:

> The theory of science, as opposed to the theory of knowledge [Erkenntnislehre], is *not* a science of *researching* as such (thus a science of perceiving and proving, of intuition and systematic investigation), but rather of the *sciences* themselves, as systems of sentences [. . .] The theory of science is not the theory of "the" science as a sum total of research- and cognitive-acts in the sense of the theory of knowledge . . . It is also not the theory of science as a plurality of cultural-historical events [Gegebenheiten], but rather the theory of the individual sciences as structures of propositions and problems or doctrinal systems.[23]

Like Reichenbach, Lewin advocates a philosophy of science whose initial data—as it were the observational basis of Wissenschaftslehre as a science[24]—are the concrete individual sciences themselves. This approach is opposed to a theory that begins with the psychological acts or mental faculties (reason, intuition, perception) of individual thinkers, and is equally opposed to a merely historical or sociological approach that looks at the genesis of sciences and scientific theories as historical events calling for historical explanations. Each science is understood instead as a system of concepts and propositions, in abstraction from the psychological events or historical events that brought them into being.[25]

Now compare Cassirer's description of Kant's critical project:

> Only now do we fully understand Kant's statement, that the torch of the critique of reason does not light up the objects unknown to us beyond the sense world, but rather the shadowy place of our own understanding. The 'understanding' here is not to be taken in the empirical

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[21]Reichenbach (1936, 142–143); cf. Reichenbach (1920/1965, 72–73).

[22]Reichenbach (1920/1965, 114).

[23]Lewin (1925). I cite from the reprint in Lewin (1981).

[24]Lewin (1925, 53) advocated that philosophers of science focus on description of the various sciences instead of *deduction* (Lewin 1925, 61; Lewin 1927, 279, translated as Lewin 1992). Compare Reichenbach's advocating an "inductive" over a "deductive" method in the philosophy of science (Reichenbach 1920/1965, 75).

[25]Lewin (1949, 25–26): "Doubtless the researcher is deeply influenced by the culture in which he lives and by its technical and economic abilities. Not these problems of cultural history, however, are in question when the social psychologist has to make up his mind whether or not 'experiments with groups' are scientifically meaningful, or what procedure he may follow for developing better concepts of personality, of leadership, or of other aspects of group life. Not historical, but conceptual and methodological problems are to be answered, questions about what is scientifically right or wrong, adequate or inadequate; although this correctness may be specific to a special developmental stage of a science and may not hold for a previous or a later stage. In other words, the term "scientific development" refers to levels of scientific maturity, to levels of concepts and theories in the sense of philosophy rather than of human history or psychology."

> sense, as the psychological power of human thought, but rather in the purely transcendental sense, as the whole of intellectual and spiritual culture. It stands directly for that entity which we designate by the name ‘science’ and for its axiomatic presuppositions, but further in an extended sense, for all those orders of an intellectual, ethical, or aesthetic kind demonstrable in reason and perfected by it.[26]

A proper Kantianism, then, for Cassirer accords with Reichenbach’s and Lewin’s projects: it focuses on an analysis of science, considered as an axiomatic system of concepts and propositions, in abstraction from the psychological acts and historical events that brought it into being.

Reichenbach was thus aware that a method similar to the one he described was already put forward by Cassirer and the other members of the Marburg School. Indeed, compare the following two descriptions of the proper philosophical method. The first is from Reichenbach 1920:

> The results discovered by the positive sciences in continuous contact with experience presuppose [coordinating] principles the detection of which by means of logical analysis is the task of philosophy. [. . .] There is no other method for epistemology than to discover the principles actually employed in knowledge.[27]

Here Reichenbach is arguing that the primary task of the logical analysis of science is to isolate the a priori elements in our current best physical theories. (Reichenbach’s conception of the primary task of an analysis of physics was constant even as Reichenbach’s conception of the a priori shifted toward conventionalism, and coordinating principles became coordinating definitions.)[28] The second description is from Cassirer 1906:

> The task, which is posed to philosophy in every single phase of its development, consists always anew in this, to single out in a concrete, historical sum total of determinate scientific concepts and principles the general logical functions of cognition in general. This sum total can change and has changed since Newton: but there remains the question whether or not in the new content [*Gehalt*] that now emerges some most general relations, on which alone the critical analysis directs its gaze, present themselves under a different form [*Gestalt*] and covering.[29]

In this latter passage, Cassirer is expressing his commitment to what the Marburg Neo-Kantians called the “transcendental method” or the method of “transcendental logic.”[30] According to this approach, the proper object of philosophy is our best current mathematical sciences of nature. These sciences are the “fact” whose preconditions (“the general logical functions”) it is the task of philosophy to study.[31]

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[26] Cassirer (1918/1981, 154–155).

[27] Reichenbach (1920/1965, 74–75).

[28] See, for instance, Reichenbach (1924/1969, pp. xii–iv).

[29] Cassirer (1906/1922, 16).

[30] See Cassirer (1912), and Natorp (1912). A nice recent discussion is Richardson (2006).

[31] On the Marburg reading of Kant, Kant first isolated the transcendental method and applied it to Newtonian science; in fact, he mistakenly thought that the transcendental preconditions of Newtonian science were the fixed preconditions for all scientific cognition in all times (Cassirer 1906/1922, 18).

"Science" here is no abstract generality: it is the particular sciences and particular theories contained in the writings of the scientists themselves. The preconditions of these sciences are not understood psychologically, as primitive acts innate in the human mind. Such a psychologistic approach is doomed to failure.[32] Rather the goal of the analysis of science is to isolate the highest concepts and principles of our sciences.

Reichenbach was correct, then, to identify Lewin and Cassirer as two other proponents of his conception of the philosophy of science. Still, though, there were differences among the three philosophers. First, the three disagreed on the historical question whether Kant himself had anticipated the project of a logical analysis of science. Reichenbach thought that Kant's project was still psychological. Cassirer argued the opposite,[33] while Lewin articulated a middle position.[34] Second, Reichenbach became convinced that logical analysis could reliably distinguish the factual from the logical elements in a theory only if the target physical theory was given a mathematically exact axiomatization. This view contrasted with a "historical method" such as Cassirer's (and to a lesser extent, Lewin's), which analyzed the logical structure of a theory through a historical analysis of the theoretical scientific work that it grew out of.[35] In fact, Lewin, unlike Reichenbach, showed little interest in distinguishing the a priori and empirical elements in empirical theories. Third, Lewin advocated a "comparative description" of the various concrete sciences. While Reichenbach focused during the 1920s on the analysis of one theory in one science—Einstein's theory of relativity—Lewin wrote works comparing the fundamental concepts and principles of psychology, biology, and physics. Though Lewin found inspiration for this work in Cassirer's writing,[36] this emphasis on comparison is unique to Lewin's Wissenschaftslehre. Since productive comparison requires a critical mass of data, Lewin advocated a focus on observing the various

[32]Cohen (1902, 17).

[33]Cassirer read Kant as a proponent of the method. Reichenbach, however, thought that Kant's philosophy confusedly mixed together questions about the logical structure of the sciences with psychological questions. See Reichenbach (1920/1965, 55ff.) and Reichenbach (1922/1981, 29). Schlick agreed with Reichenbach; see Schlick (1921/1979, 331): "Kant certainly wanted to purge [pure intuition] of everything psychological—but I shall never be able to persuade myself that he succeeded." Cassirer defended his reading of Kant against Schlick in Cassirer (1921/1923, 451).

[34]See Lewin (1927, 279): "The Copernican Turn, with which Kant changed the question "*Whether* knowledge is possible" into the question "*How* knowledge is possible," is *one* step"—though not the final step!—"in the development of the theory of knowledge from a speculative science into an observational science. Into a science, therefore, that begins with the investigation of the concrete objects lying before us, instead of a few concepts given ahead of time."

[35]See Reichenbach (1924/1969, xiii).

[36]See Lewin (1949, 26): "A ... reason why I feel Cassirer's approach is so valuable to the scientist is his comparative procedure. Although Cassirer has not developed what might be called a systematic *comparative theory of the sciences,* he took important steps in this direction. His treatment of mathematics, physics, and chemistry, of historical and systematic disciplines is essentially of a comparative nature. Cassirer shows an unusual ability to blend the analysis of general characteristics of scientific methodology with the analysis of a specific branch of science."

sciences, rather than theorizing before the data were all in. Kantian philosophies of science, including presumably Cassirer's own, are too quick to introduce a theoretical superstructure on this data.[37]

4.3 Cassirer and Lewin

In this section my goal is to understand how Lewin's philosophy of science, derived in important ways from Cassirer's own, informed his experimental work as a working psychologist. Lewin outlined the theoretical underpinnings of his psychological research program in a 1931 paper from *Erkenntnis*, "Der Übergang von aristotelischen zum galileischen Denken in Biologie und Psychologie"—a work that draws explicitly and repeatedly from Cassirer. This paper was widely read by psychologists, and was quickly translated into English and published in the English language *Journal of General Psychology*. My organization will be a bit non-standard: instead of explaining one at a time Cassirer's philosophy of science, Lewin's philosophy of science, and Lewin's psychological research, I'll follow Lewin's own presentation in his 1931 paper and discuss all three simultaneously. I'll isolate eight features of Lewin's experimental work. The first four features, I hope to show, can profitably be seen in the context of Cassirer's and the Berlin Group's related projects of giving a logical analysis of science. The last four features can profitably be seen in the context of Cassirer's famous contrast between substance-concepts and function-concepts.

4.3.1 Lewin's Psychological Research Program and Cassirer's Transcendental Method

4.3.1.1 Lewin Expanded the Domain of Psychological Research to Include Behavioral and Social Phenomena Commonly Thought to Be Inappropriate Objects of Psychological Research

Lewin's psychological work was dedicated to widening the subject matter of psychology to include a psychology of behavior and a psychology of social

[37]Lewin (1925, 61). "Even Neo-Kantianism has produced works (e.g., Cassirer 1910) that contained descriptions of a concreteness about the relevant objects that were still not sufficiently concrete. Neo-Kantianism remained too bound to an essentialy deductive 'System'; but it still attained a certain level of descriptive work within the frame of a system. With the question of 'possibility' the fundamental point of view of Kantianism remains the point of view of a not-descriptive theorizing; it remains directed toward *generalities*. The examples often carry the character of mere illustrations for thoughts that are derived from one or some few central ideas (above all from the idea of the unity of consciousness or of knowledge.)"

groups—subjects that many psychologists thought to be illegitimate subjects of psychological research. Indeed, he is today often considered the father of social psychology. In this respect, his work was like Freud's. But Lewin departed from Freud in seeking out experimental (as opposed to therapeutic) methods for testing various theories of individual and group behavior, and again unlike Freud, he wanted to discover mathematical laws for these domains.[38]

4.3.1.2 Lewin Thought That the Expansion of the Domain of Psychological Research Would Require New Concepts

He introduced what he called "Field Theory," expanding the holistic psychology that Gestalt theorists applied to perception to a subject's total place in her environment. A subject's needs and wants form what he calls "Tension systems." Objects in the environment have a "valence," steering the behavior of subjects in their environment. He transferred concepts developed in physics into psychology, introducing talk of "Behavioral Dynamics" and "Group Dynamics," and "vectors." He and his students, in exploring the conditions under which subjects set goals, introduced the concept of a "level of aspiration," a phrase, like his "group dynamics," that has entered the vernacular.[39]

Lewin claimed that his self-conscious conceptual innovation was inspired by Cassirer's philosophy of science. Cassirer recognized, Lewin said, that many of the most important developments in science have been conceptual innovations:

> To proceed beyond the limitations of a given level of knowledge the researcher, as a rule, has to break down methodological taboos that condemn as "unscientific" or "illogical" the very methods or concepts which later on prove to be basic for the next major progress. Cassirer has shown how this step by step revolution of what is "scientifically permissible" dominates the development of mathematics, physics, and chemistry throughout their history.[40]

The Marburg transcendental method involves carrying out anew for each stage in the history of science the project of Kant's first *Critique*. This Neo-Kantianism, unlike Kant's original writings, brings to the center of philosophical reflection the fact that sciences develop, and it seeks to discover how, why, and how it is possible that the sciences develop in the progressive way that they do. (As Lewin put the point, philosophy of science must investigate the Werden—the "becoming"—of the various sciences in their successive developmental stages.)[41] Implicit in the project of carrying out Kant's critical project for each stage in the development of science is the conviction that the advance of science has included more than a further accumulation of more facts or more powerful experimental methods. In each

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[38] See Lewin (1937).

[39] On these concepts, see the classic papers collected in Part II of Lewin (1999).

[40] Lewin (1949, 26).

[41] Lewin (1925, 75). This phrase echoes Natorp's claim (in Natorp 1912) that science is not just a "faktum," but also a "fieri."

stage of science, the fundamental categories and principles—which delimit the domain of what is thought to be physically possible—are overturned and replaced. As Cassirer's historical research has shown, the progress of science would then be hindered were philosophers (or scientists) to treat the conceptual scheme or fundamental ontology of science as fixed for all time.

4.3.1.3 Lewin Thought That Psychological Experimentation Was Hindered by Adopting the Pose of a Theory-Free "Fact-Collector"; That, Paradoxically, Effective Experimentation Requires Adopting a Theoretical Framework[42]

In fact, Lewin made his international reputation during his Berlin years less because of his experimental results and more because of his willingness to develop (in anticipation, as it were) a new theoretical apparatus for developing and interpreting laboratory experiments.[43]

In this, Cassirer was a clear inspiration. From Kant, Cassirer thinks we should learn not to be embarrassed to admit that we bring to our experiments a set of concepts and principles that make them possible.

> [W]hile a lone sensory perception or mere collection of such perceptions may be able to get along without the guidance of a plan of reason, it is still the latter that first makes experiment precise and possible, 'experience' in the sense of physical knowledge. [. . .] Before Galileo could measure the magnitude of acceleration in free fall, the conception of acceleration itself, as well as measuring apparatuses, had to exist, and it was this mathematical conception which once for all differentiated his unadorned way of putting the question from that of the medieval scholastic physics. [. . .] What Galileo laid down in advance, according to the plan of reason, is what initially made it possible for the experiment to be conceived and directed. (Cassirer 1918/1981, 164)[44]

As Lewin noted, Cassirer carried this Kantian thought over into his analysis of natural scientific experimentation. Even the most basic results of physical experimentation—measurements—require instruments whose behavior can only be interpreted through a background theory.[45]

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[42]Lewin (1949, 28).

[43]On this point, see Brown (1929).

[44]This passage is Cassirer's commentary on Kant, *Critique of Pure Reason (1998)*, Bxii: "[R]eason has insight only into that which it produces after a plan of its own, and that it must not be kept, as it were, in nature's leading strings, but must itself show the way with principles of judgment based upon fixed laws, constraining nature to give answers to questions of reason's own determining. Accidental observations, made in obedience to no previously thought-out plan, can never be made to yield a necessary law, which alone reason is concerned to discover."

[45]Lewin (1949, 27–28) cites with approval Cassirer (1910/1923, 144): "In truth, no physicist experiments and measures with the particular instrument that he has sensibly before his eyes; but he substitutes for it an ideal instrument in thought, from which all accidental defects, such as necessarily belong to the particular instrument, are excluded. . . . The corrections, which we make and must necessarily make with the use of every physical instrument, are themselves a work

4.3.1.4 Lewin's Method, Though It Relied on Analogies Between Physics and Psychology, Was Fundamentally Anti-reductionist

Lewin thought explanations could be given of psychological phenomena without reducing the objects, concepts, or laws of psychology to those of physics or physiology.[46] In this respect, Lewin is similar to Cassirer, who thought that each science in each of its phases required its own transcendental analysis. Each of the special sciences 'frames its own questions,' and answers them according to diverse and independent methodologies; none of the methods of the special sciences 'can simply be reduced to, or derived from the others.'[47] Indeed, Cassirer thought that the felt philosophical need for reductionism—even when the practice of the various special scientists did not support it—was motivated by a prior metaphysical conviction that the transcendental method requires us to reject.[48]

In the 1930s, Cassirer associated the metaphysically-motivated reductionism that he rejected with Carnap's claim (in two papers that appeared in *Erkenntnis*: Carnap 1932/1934 and 1932/1959, "Die physikalische Sprache als Universalsprache der Wissenschaft" and "Psychologie in physikalischer Sprache") that the language of physics is a universal language.[49] Lewin had no more sympathy with Carnap's arguments in these papers than Cassirer did. In fact, Reichenbach asked Lewin to write a response to these papers for *Erkenntnis*. Lewin politely declined, and a younger member of the Psychological Institute—Karl Duncker—was deputized instead.[50] Duncker there cited (against Carnap) Lewin's claim that the reducibility of concepts from the special sciences could not be a straightforward 1–1 mapping, but that biological or psychological concepts would at best map into complicated combinations or networks of physical concepts.[51] Elsewhere, Lewin himself argued that science is a unity only in the (comparatively weak) sense that the methods

of mathematical theory; to exclude these latter, is to deprive the observation itself of its meaning and value."

[46] Lewin (1931b, 37).

[47] Cassirer (1923/1955, 76, 77, 78).

[48] See Cassirer (1923/1955, 76): "The object cannot be regarded as a naked thing in itself, independent of the essential categories of natural science: for only within these categories which are required to constitute its form can it be described at all. . . . If the object of knowledge can be defined only through the medium of a particular logical and conceptual structure, we are forced to conclude that a variety of media will correspond to various structures of the object, to various meanings for 'objective' relations. The physical object is not the chemical object, nor is it the biological object, because physical, chemical, biological knowledge *frame their questions* each from its own particular standpoint and, in accordance with this standpoint, subject the phenomena to a special interpretation and formation."

[49] See Cassirer (1999 [written 1937], 6–7), and Cassirer (1942/2000, 41).

[50] Ash (1994, 95).

[51] Duncker (1932/1933, 176), citing Lewin (1922).

of individual sciences go through similar developmental stages—for instance, he argues, psychology in his time was passing from a stage akin to Aristotelian physics to a stage akin to Galilean physics.[52]

4.3.2 *Lewin on Substance-Concepts and Function-Concepts*

Lewin argued in his 1931 *Erkenntnis* paper that psychology up that point was reminiscent of Aristotelian physics: it considered only a subset of psychological phenomena to be subject to psychological laws, and in those cases rested content with categorizing subjects into types, with giving generalizations about "normal" cases and eschewing responsibility for explaining why particular cases are the way they are. Contemporary psychological dynamics is thus hindered by a methodology reminiscent of Aristotelian physics, and it needs to move, as physics did, into a Galileian phase.

The language in this paper is unmistakably Cassirerian. The main argument of Cassirer's *Erkenntnisproblem*, vols.1–2, is that the development of modern science and philosophy from the late Renaissance to Kant is a working out of the Galilean ideal of science. Cassirer's *Substance and Function* further argues that modern logic and epistemology have remained wedded to an Aristotelian theory of concepts—grounded in a metaphysics of substances—long after the Aristotelian metaphysics and epistemology were supplanted with the modern science initiated by Galileo. The contrast that forms the main theme of the book—that between substance-concepts and function-concepts—is complex and multi-faceted, and I will try over the course of this chapter to lay out some of the main elements in this distinction.[53] Lewin not only cites Cassirer's book frequently in the essay; he on a few occasions says explicitly that his goal is to initiate a switch in psychology from substance-concepts to function-concepts.[54]

4.3.2.1 Lewin Thought That Psychology Needed to Look for Strict, Exceptionless Laws That Could Unite Psychological Phenomena That Differ Prima Facie

Pre-Galileian physics was hindered by the assumption that only super-lunary phenomena are subject to law and that laws are simply expressions of what happens

[52]Lewin (1925, 50–51). There he argues that in the various stages of its historical development one and the same science will require different methods, and that different sciences in the same relative stage of their development will often employ the same method. He concludes: "In view of the fundamental tools [Grundzüge] of the method (also only in this sense) one can speak in the end of a 'unity (better: homogeneity) of all knowledge.'"

[53]For a more detailed discussion of the contrast between "*Substanzbegriff*" and "*Funktionsbegriff*," see Heis (201?).

[54]Lewin (1931b, 40, 44).

in general. Just as post-Galileian physics has done, so too should psychology attempt to find exceptionless laws that apply to psychological phenomena generally.

On Cassirer's view, the Aristotelian notion of laws was intertwined with the Aristotelian view of experience and the Aristotelian metaphysics. For Aristotle, our knowledge of objects of experience was explained by the fact that the forms of objects were perceptible and could be directly transferred from the objects to the mind in perception. Thus, the conceptual repertoire of an empirical scientist is fixed by the nature of empirical objects (substances) and by the nature of our minds to receive these substantial forms. Cassirer thought that Kant had shown that this whole picture is illusory. Even the experience of objects requires a set of concepts, whose meaning is expressed in fundamental laws. No experience is "direct;" it is thus only through the introduction of laws that objects can be experienced at all. And so the domain of law is universal.

4.3.2.2 Lewin Thought That the Development of Psychology Would Require a New Use of Mathematics, and He Thought That the Function of Mathematics Is to Allow Psychologists to Develop General Laws That Can Explain Why a Particular Case Is the Way It Is

Lewin was critical of the way his contemporaries used mathematics in psychology. He thought that the use of precise measurements to determine statistical averages—for instance, in intelligence testing or in determining the properties of the "average 4 year old" —was not very valuable because it did not allow researchers to do as Galileian physicists did—that is, to explain why a particular case has the particular features it does. For instance, compare a simple non-mathematical generalization, like "Every flash of lightning is followed by thunder," with an equation that expresses the temporal interval between seeing the light and hearing the thunder as a function of the distance between the lightning and the observer. In the latter case, but not the former, the law will tell not just that this case of thunder is like all other cases in being accompanied by thunder; it will also say why the temporal interval between lightning and thunder in this particular case differs from the corresponding interval in that case. This is what Lewin wanted. He wanted psychological laws that explained why this 4 year old is doing this now; he did not want to rest content with hearing what an average 4 year old would do—no matter how precisely this average can be measured. A truly Galileian psychology would not just use mathematics—psychologists in 1931 were already doing that—but it would use mathematics for just this purpose.

Again Lewin is sounding a theme from Cassirer. What a mathematical function does—Cassirer argued in *Substance and Function* Ch. 1[55]—is to allow for completely general laws that can capture not only what every event has in common, but also how precisely the individual cases differ from one another. Aristotelian concepts—which express only the common features of a set of objects—do not allow us to recapture the differences among cases.[56]

4.3.2.3 Lewin Thought That the Development of Psychology Was Hindered by a Fear of Introducing "Hidden Variables" That Would Bring Together and Explain a Wide Variety of Psychological Phenomena That, Prima Facie, Are Unrelated

Aristotle thought that the physical world came in fundamental types (each with its own distinctive behavior) and that the type of the object could be ascertained by direct observation. Galileo thought that all physical objects—stars, falling bodies, fluids, etc.—could be explained with the same few laws. But the ultimate success of this program required thinking of objects as composed of homogeneous, microscopic stuff. In the same way, Lewin thought that introducing "tension systems" and psychological vectors would help explain a wide variety of psychological phenomena, even if these tension systems or social forces were not reducible to "directly" observable phenomena.

Cassirer had argued that philosophical scruples about hidden variables and unobservable entities were philosophically on a par with the now discredited Aristotelian metaphysics and epistemology. On the Aristotelian view, philosophers can identify the fundamental forms of objects—the kinds of substances there are—once and for all. And given the Aristotelian theory of perception, it is not surprising that the ontologically privileged set includes only those objects that can be "directly" perceived—that is, not microscopic atoms, nor super-personal psychological entities like groups and social forces. But, given the connection between law and objecthood that Cassirer highlights, the set of objects in our scientific ontologies will develop as our fundamental laws do; and it is a lesson of the transcendental method that we cannot prescribe ahead of time what kind of fundamental laws science might propose.

[55]Cassirer (1910/1923, 19–20).

[56]See Lewin (1927, § IV), which cites Cassirer to support the claim that the goal of an experiment is not to find very many equal cases, but rather to find a systematic variation among a sum total of different cases. He argues that, if one thinks of a law as a regularity, a rule, then one thinks that one proves that there is a law by finding the greatest number of equal cases [gleicher Fälle]. But this rests on a faulty theory of induction, refuted already by Cassirer.

4.3.2.4 Lewin Thought That—Unlike Previous Associationist Psychology and Theories of Instincts[57]—A Proper Psychological Dynamics Would Require Viewing Behavior as a Function of Both Environment and the Person[58]

Aristotelian physics explained the dynamics of a physical object by the unchanging nature of the object. Air goes up, no matter its circumstances. In modern physics, though, the direction and velocity of an object depends on its relation to the other objects in its environment. Similarly, in introducing field psychology, Lewin argued that the psychic forces acting on a subject depend crucially on the situation; this dependency is expressed in his now famous equation (often called "Lewin's equation") $B = f(p, e)$.[59] Similarly, in a series of famous experiments from the 1920s he showed that a subject's memory of a fact depends not on past repetition, but on the relevance of the fact to some ongoing task, and he was able to identify the factors in a particular situation that would cause a subject to become angry.[60]

The influence from Cassirer is clear. A metaphysics of substances tries to explain empirical phenomena ultimately in terms of the internal nature of individual objects. Cassirer argued that, in modern physics, empirical phenomena are explained through laws that, through mathematical functions, express the relations between different objects and magnitudes. A physicist then would say that a kind of object exists, not necessarily because we can directly observe it, but because an equation referring to that kind of object allows us to explain the relations among objects that can be more directly perceived. Thus, it is not unchanging objects (or substances) that science is after, but constant laws that express unchanging relations among phenomena.

4.4 Reichenbach and Cassirer on the A Priori

Even this brief survey of Lewin's philosophical writings shows that Lewin—though he shared in broad outlines Reichenbach's goal of a 'logical analysis of science'—did not see his work as supporting empiricism.[61] In fact, answering the question whether natural science presupposes a priori principles—and the accompanying task of systematically sorting the propositions of a theory into the

[57] See Lewin (1926) for a criticism of associationist explanations.

[58] Lewin (1931b, 64–65).

[59] This equation, implicit in earlier works, was first introduced in Lewin (1936).

[60] See Marrow (1969, Ch.5). Again, the contrast is with associationism and Freudianism, which try to explain behavior in terms of past experiences rather than through the interaction with the present environment.

[61] Nowhere in Lewin (1931b) does he express any affinity for empiricism. Indeed, the only mention Lewin makes of empiricism is to point out that—paradoxically—the real advance in Galilean physics required introducing unobservable idealized objects like frictionless planes and perfect spheres (Lewin 1931b, 44–45).

a priori and empirical elements—was not part of Lewin's project. It is of course, however, precisely the difference between empiricism and idealism that forms the center of the evolving discussion between Reichenbach and Cassirer over the proper philosophical interpretation of relativity. It is to this topic that I now turn.

4.4.1 Reichenbach on Coordinating Principles

In the first volume of *Erkenntnis*, Reichenbach wrote that with the "method of the analysis of science the Society positions itself in conscious opposition to all pretensions of a philosophy that claims a special right of reason and wants to set up propositions of a priori validity that are not subject to scientific critique."[62] Similarly, in his 1928 *Philosophy of Space and Time*, Reichenbach took himself to have refuted the "philosophy of the a priori,"[63] and he argued that there is a patent contradiction between the theory of space in General Relativity and the philosophy not only of Kant, but also of the various more permissive Neo-Kantians.[64] As is well-known, this was not the rhetorical stance that Reichenbach had taken in his 1920 book, where he argued that Einstein's theory is inconsistent with empiricism and confirms a kind of critical philosophy.[65] This revised Kantianism required separating out two meanings of Kant's a priori principles: as necessarily and unrevisably valid principles, and as principles 'constitutive of the object of knowledge'—that is, at some stage in the history of science.

> Because of the rejection of Kant's analysis of reason, one of its meanings, namely, that the a priori statement is to be eternally true, independently of experience, can no longer be maintained. The more important does its second meaning become: that the a priori principles constitute the world of experience. Indeed there cannot be a single physical judgment that goes beyond the sate of immediate perception unless certain assumptions about the description of the object in terms of a space-time manifold and its functional connection with other objects are made.[66]

Reichenbach rejects the possibility of a priori principles in the first sense, since there is a conflict within General Relativity between the empirical fact that inertial and gravitational mass are equivalent and the purportedly self-evident principle that the metric of physical space is Euclidean.[67] The proper moral of this surprising disconfirmation of a coordinating principle previously thought to be "eternally valid" is not to find a new self-evident principle to take its place, but to reject out of hand the project of identifying apodictic a priori principles.[68]

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[62]Neurath et al. (1930, 72).

[63]Reichenbach (1928/1957, 67).

[64]Reichenbach (1928/1957, 36).

[65]Reichenbach (1920/1965, ch. 8; 1922/1981, note 21).

[66]Reichenbach (1920/1965, 77).

[67]Reichenbach (1922/1981, 37; 1920/1965, 31).

[68]Reichenbach (1922/1981, 39; 1920/1965, 79).

These considerations, however, do not touch the necessity of a priori principles in Reichenbach's second sense. Indeed, Reichenbach thought that General Relativity well illustrated that special principles are required to coordinate physical objects with the implicitly defined mathematical concepts appearing in the axioms of physics.[69] (Indeed, in 1920/1965 he argues that these principles—which he calls "axioms of coordination"—"constitute the world of experience" (77), since the objects coordinated with our concepts are completely undefined outside of their coordination with our concepts.) In particular, Reichenbach argues throughout the 1920s that the determination of the spatial and temporal metrics presupposes such principles of coordination.

In a famous exchange that has become widely discussed since Alberto Coffa's 1991 book, Schlick argued that what Reichenbach was calling "axioms of coordination" are better understood simply as conventions, and thus as *analytic* a priori principles.[70] Reichenbach was initially unpersuaded, for reasons that he laid out in his 1922 paper *The Present State of the Discussion of Relativity*:

> In the first place, conventionalism does not recognize, as Kant did, that these 'conventions' determine the concept of object, that the particular thing or law is defined only by their help and not by reality alone. Secondly, the term 'convention' overemphasizes the arbitrary elements in the principles of knowledge; as we have shown, their *combination* is no longer arbitrary.[71]

Reichenbach argued throughout the 1920s that coordinating principles function in physical theories only in groups, and that a coordinating principle that allows for a unique or consistent coordination of concepts with things in conjunction with one set of principles might in fact be inconsistent with experience when conjoined with other coordinating principles. In this respect, coordinating principles differ from standard cases of linguistic conventions, whose arbitrariness is unconstrained—a fact patent to any one who has tried to learn a new language as an adult, and run up against the language's frustrating lack of consistency. But considering that Reichenbach's whole point was that these coordinating principles—being relative and not apodictic—do not entirely fit the standard characterization of Kant's "synthetic a priori" principles either, the question whether one should call these coordinating principles "axioms" or "conventions" threatens to become—as Reichenbach seemed to recognize in his 1922 paper[72]—simply a verbal question.

Cassirer himself never consented to labeling the a priori principles of physics as conventions. But given the threat of a terminological draw, it is not clear right away whether Cassirer's opposition to full-fledged conventionalism is itself simply an issue of word choice or rhetorical emphasis. Indeed, both Schlick and Reichenbach worried that the Kantianism in Cassirer's philosophy of physics had been so

[69] Reichenbach (1920/1965, 36–37; 54).

[70] Schlick's letter is from November 1920, and is discussed in Coffa (1991, 201–202). Schlick made his criticism in print in Schlick (1921/1979).

[71] Reichenbach (1922/1981, 38–39).

[72] Reichenbach (1922/1981, note 21).

weakened as only to differ verbally from their own empiricism.[73] In the remaining pages of the paper, I will argue that the dispute between Cassirer, Reichenbach, and Schlick was not merely verbal, and that it turned on some of the deepest features of Cassirer's epistemology of science.[74]

4.4.2 Reichenbach's Criticism of Cassirer

Unfortunately, we will find little help in locating the real differences (should there be any) between Cassirer and Reichenbach by looking at Reichenbach's criticisms of Cassirer's interpretation of relativity. In his 1922 review article, Reichenbach praises Cassirer for having "awakened Neo-Kantianism from its 'dogmatic slumber,' while its other adherents carefully tried to shield it from any disturbance by the theory of relativity." But while Cassirer deserves credit for clearly articulating the inconsistency of Einstein's theory with Kant's theory of space, nevertheless Cassirer's "approach is tantamount to a denial of synthetic *a priori* principles, and ... there is no other remedy but to renounce the apodictic character of epistemological statements."[75] This criticism is misplaced (Ferrari 2003, 99ff). According to the Marburg reading, Kant had given a defense of the non-empirical truth of both Euclidean geometry and the principle of causality by showing that they are preconditions of the possibility of Newtonian science. In the same way, Cassirer claims that Riemannian differential geometry and Einstein's principle of general covariance are conditions of the possibility of general relativity.[76] However, Cassirer explicitly denies that these principles, or any other, are apodictic, certain, or self-evident, and he denies that we have any conclusive reason to think that these principles will be constitutive principles in our future physics.[77]

Indeed, many interpreters have claimed that Cassirer's theory of the a priori is an anticipation of the theory of the relativized a priori later articulated by Reichenbach in 1920.[78] Already in 1906, Cassirer wrote:

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[73]Schlick (1921/1979, 326), and Reichenbach (1928/1957, 36ff).

[74]There is another aspect to Cassirer's resistance to Schlick and Reichenbach's conventionalism, an aspect that I will mention but not further explore. Cassirer argued (against Schlick explicitly) that labeling linguistic meanings as conventions does not explain the prior question How is meaning possible at all? In fact, Schlick can settle for empiricism only because he (mistakenly) thinks that by labeling meanings as conventions he can avoid answering the question altogether. See Cassirer (1927, 136).

[75]Reichenbach (1922/1981, 30).

[76]Cassirer (1921/1923, 415).

[77]See Cassirer (1910/1923, 269).

[78]Richardson (1998, ch.5), Ryckman (2005, ch.2), and Padovani (2011).

> In [science] we find only a *relative* stopping point, [and] we therefore have to treat the *categories*, under which we consider the historical process itself, themselves as variable and capable of change.[79]

Like the relativized a priori program of Reichenbach's *Theory of Relativity and A Priori Knowledge*, the Marburg school's transcendental method requires determining which concepts and laws play the role of Kant's categories and principles at some given stage in the history of science. But there exists within Cassirer's philosophy a second, complementary theory of the a priori as those concepts and principles that remain invariant throughout the entire history of science.

> The goal of critical analysis would be reached, if we succeeded in isolating in this way the ultimate common element of all possible forms of scientific experience; *i.e.*, if we succeeded in conceptually defining these moments, which persist in the advance from theory to theory because they are the conditions of any theory. At no given stage of knowledge can this goal be perfectly achieved; nevertheless, it remains as a demand, and prescribes a fixed direction to the continuous unfolding and evolution of the systems of experience.
>
> From this point of view, the strictly limited meaning of the "*a priori*" is clearly evident. Only those ultimate logical invariants can be called *a priori*, which lie at the basis of any determination of a connection according to natural law. A cognition is called *a priori* not in any sense as if it were *prior* to experience, but because and in so far as it is contained as a necessary premise in every valid judgment concerning facts.[80]

As Cassirer makes clear in surrounding passages, these invariant a priori cognitions include invariant concepts—such as magnitude, number, space, time, and functional correlation—and a priori principles for the formation and selection of theories—such as the principle that physical laws should be simple and of wide scope.

This theory of the a priori is distinct from—and supplementary to—the relativized constitutive a priori that also appears in Reichenbach's early writings. On that conception, a priori principles can change as theories do and a careful logical analysis of our best current theories can isolate these principles successfully. A priori concepts and principles in the second sense of Cassirer's two-part theory of the a priori[81] are absolute, not relative, remaining invariant throughout the history of science. Furthermore, no amount of careful analysis of our current theories will give us anything more than an "educated guess"[82] about what these invariant principles

[79] Cassirer (1906/1922, 16).

[80] Cassirer (1910/1923, 269).

[81] Opposed readings of Cassirer's theory of the a priori are given by Friedman—who recognizes only the second, absolute theory of the a priori in Cassirer and denies that he holds to the first (Friedman 2000, 115 ff.),—and by Richardson—who finds both theories in Cassirer's writings but claims that their conjunction is inconsistent (Richardson 1998, ch. 5). In fact, as I argue, the second theory is not inconsistent with the first theory, but necessitated by it.

[82] I owe this phrase to Friedman (2001, 66). This is Friedman's gloss on Cassirer's claim that the philosophical analysis whose goal it is to isolate these a priori elements "at no given stage can be perfectly achieved."

are, for the simple reason that we cannot foresee how science will develop in the future. Perhaps a *metaphysical* "critical analysis" could arrive once for all at a settled list of a priori cognitions, since (on such a view of philosophy) the nature of knowledge or the nature of reality would be presumably fixed and open to philosophical investigation, and its analysis would not have to wait for the results of the projected final science. (Similarly for a *psychological* "critical analysis" of knowledge, since the psychological character of the mind would not evolve as science does.) But forsaking metaphysical and psychological methods for a "logical analysis of science," Cassirer can pretend to no more certainty about these a priori elements than can be offered by our current best (though still fallible!) physical theories.

Why does Cassirer think that we need a second kind of a priori concepts and principles in addition to the first? Cassirer believes that a theory with only relative a priori principles will threaten the objectivity of scientific theory changes. Relativized a priori principles function like Kantian categories: they provide the conditions of the possibility (and thereby also the *limits*) of empirical meaning. Now, if, as Reichenbach claimed in 1920, the objects coordinated with our concepts are *completely undefined* outside of their coordination with our scientific concepts and our scientific concepts are *empty* without being coordinated by a priori principles to experience, then there will be no common stock of meanings—no shared concepts—between two scientists at different times (or worse yet: between two scientists who disagree on a new theory). But this would undermine the objectivity of theory change, turning the history of science into a sequence of logically and semantically isolated belief systems. But we know that the history of science is a progressive history, moving asymptotically toward the truth about the natural world. A philosophy of science with only relative a priori principles would threaten the fundamental truth that various scientists at various stages in the history of science are all trying to understand one and the same natural world.[83] Given the undeniable changeability of Kantian categories, then, a historically progressive science is possible only if there are some a priori cognitions that remain invariant through all stages of the history of science.[84]

[83]Cassirer (1910/1923, 321–322): 'Going back to such supreme guiding principles [i.e., the '*form* of experience' that persists in all stages of the asymptotic progression toward the fully empirically adequate theory] insures an inner homogeneity of empirical knowledge, by virtue of which all its various phases are combined in the expression of *one* object. The 'object' is thus exactly as true and as necessary as the logical unity of empirical knowledge;—but also no truer or more necessary . . . We need, not the objectivity of absolute things, but rather the objective determinateness of the *method of experience*.'

[84]See Cassirer (1906/1922, 16): "The concept of the *history of science* itself already contains in itself the thought of the *maintenance of a general logical structure* in the entire sequence of special conceptual systems."

It is the principles that are a priori in this second sense[85] that Cassirer cannot relabel as conventions. It simply makes no sense to talk of the very same conventions being laid down throughout the history of science, and it makes even less sense to say that there are conventions that we cannot in principle identify for certain. And it is clear that Reichenbach was never tempted to adopt this theory of the a priori. Why not? The answer reveals deep differences between Reichenbach's and Cassirer's epistemologies of physics. Cassirer introduced the theory of the invariant a priori to address concerns about the objectivity of theory changes. Reichenbach himself recognized the epistemological pitfalls introduced by allowing the constitutive principles of physics to change, but he argued in 1920 that these problems could be solved through the "method of successive approximations." On this method, old coordinating principles are to be replaced by new principles such that "for certain approximately realized cases the new principle is to converge toward the old principle with an exactness corresponding to the approximation of these cases"—as Einstein's theory coincides within the limits of observation to the Newtonian theory in small regions. This method "represents the essential point in the refutation of Kant's doctrine of the a priori" because it shows the adoption of new a priori principles follows an objective rule and can be justified in terms that an adherent to the old principles could understand.[86]

A fuller reply to Cassirer's theory of the invariant a priori is outlined four years later in Reichenbach's book *Axiomatization of the Theory of Relativity*. There he argued that General Relativity could be derived from coordinating definitions, mathematical axioms, and what he called "elementary facts." Even these facts—which he calls "objective coincidences"—are not directly perceived but require the imposition of very simple coordinating definitions. Objective coincidences are distinct from "subjective coincidences," which are certain, immediately given, and independent of interpretation.

> Both methods—the observation of subjective and of objective coincidences—are used in physics. But there is a great difference between them. With respect to the second kind, coincidence is inferred, not perceived. Perceptually speaking, a totality of qualities is given, such as dark and bright spots or sound impressions; from these qualities the objective coincidence of things is inferred. When two billiard balls collide with each other, we hear a characteristic noise; but that this experience *noise* constitutes a coincidence of balls is a logical construction. [. . .] It cannot be maintained, therefore, that the point events of the

[85]There is a further question: Could Cassirer relabel the *relative* a priori principles as conventions (as Reichenbach did)? Again, the answer is No, as Cassirer argues (for instance) at (Cassirer 1910/1923, 186–187) with regard to Newton's principle of inertia. I hope to explain in a future work why Cassirer rejects conventionalism even in the relativized case.

[86]Reichenbach (1920/1965, 69–70). Padovani (2011) argues that Reichenbach in fact does distinguish in 1920 between relative a priori principles and higher level, meta-principles (such as the principle of probability and the principle of genidentity). This reading of Reichenbach brings him closer to Cassirer. Still though, what becomes of these principles after Reichenbach takes his turn to conventionalism? Are they also conventions? As Padovani argues, Reichenbach has no clear, worked out view after 1920.

> theory of relativity are ultimate facts. Only subjective coincidence has the character of the immediately given; subjective coincidence alone is independent of all interpretations and is a necessary condition for all physical observations.[87]

In particular, Reichenbach identifies three kinds of objective coincidences as basic for the theory of relativity: the coincidences of light signals, the readings of a "natural" clock, and the coincidences of a body with the end points of a rigid measuring rod. All of the theorems of the theory can then be derived from coordinating definitions and simple observational facts about lights, clocks, and rods.

By distinguishing objective and subjective coincidences, Reichenbach's immediate goal is to separate his theory from a simple-minded phenomenalist interpretation—like that of Petzoldt—that reads Einstein's dictum that "all our physical experience can be reduced to [space-time] coincidences" as a Machian view that physics can be grounded in the mere co-presence of sensory qualities.[88] A Machian interpretation errs in identifying subjective and objective coincidences. Instead, Reichenbach argues, the objective coincidences involving lights, clocks, and rods are then *inferred* (defeasibly) from subjective coincidences together with some very simple coordinating coincidences. However, though Reichenbach's immediate target is phenomenalism, his distinction allows him to avoid falling into the opposite extreme. After granting—in proper Kantian fashion—that all statements about objective coincidences "contain some measure of theory," Reichenbach considers the following objection:

> Will it still be advantageous, under such circumstances, to start an axiomatization with so-called empirical facts? Are we permitted to consider such particular facts to be more certain than their confirming theory? Does there exist any confirmation other than that of the theory as a whole? Many answer this question negatively, but they are mistaken. There is a way out which is peculiar to all factual knowledge and which rests on the possibility of approximation.[89]

This "way out" has two elements. First, the subjective coincidences are themselves immediately certain, theory-neutral, and form the inductive basis for objective coincidences. The confirmation of these subjective facts is therefore not holistic. Second, though objective coincidences can be inferred from subjective coincidences only with the help of coordinating definitions, the coordinating definitions required to confirm statements about clocks, rods, and lights are themselves *independent* of the differences between relativistic and pre-relativistic physics.[90] These statements

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[87]Reichenbach (1924/1969, 17–18).

[88]The quotation is from section 3 of Einstein's (1916) "Die Grundlage der allgemeine Relativitätstheorie." Petzold interprets this passage in a Machian way in Petzold 1921, 64. Reichenbach had earlier criticized Petzold's reading in Reichenbach 1922/1981, 17 ff., following Cassirer's criticism from Cassirer 1921/1923, 392–393. Reichenbach refers to Petzold's reading (without giving his name) in Reichenbach 1924/1969, 16. The historical background to these debate is laid out in Ryckman 1992, cf. Ryckman 1994.

[89]Reichenbach (1924/1969, 5–6).

[90]Reichenbach (1924/1969, 6–7, 19).

are then "relatively invariant with respect to a great variety of interpretations." To infer from the presence of sensations of light to the intersection of two light beams requires coordinating definitions; but the particular very simple definitions needed to grasp this "elementary fact" are available to every party in the dispute over Einstein's theory. Granted, there are differences in how the two theories conceive of the travel of a light beam—in straight lines, or along geodesics that are not necessarily straight—but these differences are irrelevant (by the method of successive approximations) in the small regions that are directly perceivable. In this way, Reichenbach claims that the confirmation of these objective coincidences, though mediated by theory in this limited way, is not holistic.

On this view, then, the subjective coincidences are theory-neutral and can thus form a common, shared basis from which our physical theories can be derived using definitions and principles available to all parties. Reichenbach thus secures the objectivity of adopting Einstein's theory without bringing in Cassirer's invariant a priori. Moreover, Cassirer worried that without some constant element in all stages of our science, we would be unable to claim that a new theory presents a better way of understanding the same world as a previous theory. On Reichenbach's view, the theory-neutrality of subjective coincidences explains this: all parties infer their theories ultimately (though defeasibly) from the same, shared intersubjectively available facts. The invariance of the subjective basis and the simple coordinating definitions takes the place of Cassirer's invariant a priori.

Cassirer, however, denied that there could be such subjective coincidences that are both theory neutral and ground our physical theories even in the mediated and defeasible way Reichenbach describes. Along with his teachers Cohen and Natorp, he always denied that there could be "bare impressions" or "simple sensations" that would be available to any subject no matter what concepts or theoretical commitments she possesses.

> Idealism urges that ... all measurement, however, presupposes certain theoretical principles and in the latter certain universal functions of connection, of shaping and coordination. We never measure mere sensations, and we never measure with mere sensations, but in general to gain any sort of relations of measurement we must transcend the "given" of perception and replace it by a conceptual symbol, which possesses no copy in what is immediately sensed.[91]

In fact, when Cassirer criticizes Machian interpretations of Einstein, he draws a crucially stronger conclusion than Reichenbach does:

> [T]he givenness of 'bare' sensations in which abstraction is made in principle from all elements of form and connection, proves to sharper analysis to be a fiction.[92]

Indeed, this rejection is a central component in Cassirer's attack on the Aristotelian substance-concepts that was our concern in Sect. 4.3 of this chapter. The Aristotelian metaphysics of substantial forms underwrote a theory of perception

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[91]Cassirer (1921/1923, 427).

[92]Cassirer (1921/1923, 390).

according to which the properties of objects could be transferred to a subject simply through causal contact. However, the fact that two subjects are situated in the same world and are receiving impressions from the same physical objects is not sufficient to provide them with a common intersubjective basis. The objectivity of their representations requires not just shared substances in their environment, but common principles of interpretation—what Kantians might call a shared function of synthesis. This requirement drives Cassirer to adopt a confirmation holism that Reichenbach finds objectionable, and it is this requirement that ultimately leads Cassirer, but not Reichenbach, to postulate a non-conventional and non-relativized a priori. It is this difference that fundamentally distinguishes Neo-Kantians like Cassirer from the empiricists of the Berlin Group.

Needless to say, this is not the place to determine the relative merits of Cassirer's and Reichenbach's interpretations of relativity. Still, though, it is clear that the dispute between Reichenbach and Cassirer over whether a priori concepts and principles are simply conventions rests on a prior dispute over the necessity of positing as a transcendental precondition of the objectivity of science a set of invariant a priori cognitions. This dispute itself, then, rests on a fundamental difference over the role of sensations in simple perceptual knowledge and the possibility of shielding off our measurements from our other theoretical beliefs.

This incomplete story of the relations between Lewin, Reichenbach, and Cassirer demonstrates the depth and the diversity of the personal and philosophical interactions between Cassirer and the members of the Berlin Group. Lewin and Reichenbach, though they both studied with Cassirer, drew differently on his work. As a working empirical researcher, Lewin drew from Cassirer's analyses of the history of the sciences morals about the proper methodology for the psychology of his day, and he used these analyses as inspiration for a deeply original and consequential psychological research program. Reichenbach, not himself an empirical researcher, engaged with Cassirer's work in answering a core and perennial philosophical question, the possibility of empiricism. Though all three shared a commitment to a philosophy of science based in the analysis of the actual sciences, they used this method for different purposes and to draw different conclusions. Indeed, it is telling that the very point on which the dispute between Cassirer's and Reichenbach's interpretations of relativity rests—on the possibility of non-holistic confirmation of elementary measurements—Lewin sides with Cassirer and not with Reichenbach. As we saw above, Lewin drew inspiration for his conceptual innovation within experimental psychology from his opposition to the view of the researcher as "fact collector"—an opposition that Lewin drew from Cassirer, citing for support the very passages defending a holistic, Duhemian theory of measurement that Reichenbach ultimately rejected.[93]

[93] See note 45.

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