"Hermann Cohen's History and Philosophy of Science"

Lydia Patton
Department of Philosophy
McGill University, Montreal

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ABSTRACT

In my dissertation, I present Hermann Cohen's foundation for the history and philosophy of science. My investigation begins with Cohen's formulation of a neo-Kantian epistemology. I analyze Cohen's early work, especially his contributions to 19th century debates about the theory of knowledge. I conclude by examining Cohen's mature theory of science in two works, *The Principle of the Infinitesimal Method and its History* of 1883, and Cohen's extensive 1914 *Introduction* to Friedrich Lange's *History of Materialism.* In the former, Cohen gives an historical and philosophical analysis of the foundations of the infinitesimal method in mathematics. In the latter, Cohen presents a detailed account of Heinrich Hertz's *Principles of Mechanics* of 1894. Hertz considers a series of possible foundations for mechanics, in the interest of finding a secure conceptual basis for mechanical theories. Cohen argues that Hertz's analysis can be completed, and his goal achieved, by means of a philosophical examination of the role of mathematical principles and fundamental concepts in scientific theories.

Ma dissertation porte sur la fondation de l'histoire et la philosophie de la science présentée par Hermann Cohen. Mon enquête début avec l'articulation de Cohen d'une épistémologie néo-kantienne. Dans ce contexte, j'analyse le travail de Cohen au début de sa carrière, particulièrement ses contributions aux débats de la 19ème siècle à propos des théories de la connaissance. En conclusion, j'examine la théorie de la science mature de Cohen, dans ses deux travails : *Le principe de la méthode infinitésimale et son histoire* de 1883, et son *Introduction* de 1914 au livre de Friedrich Lange, *L'Histoire de la Matérialisme.* Dans le premier, Cohen donne une analyse philosophique et historique de la méthode infinitésimale dans le mathématique. Dans son *Introduction,* Cohen présente une perspective détaillée sûr le livre de Heinrich Hertz de 1894, *Les principes de la mécanique.* Hertz considère une série des fondations possibles pour la mécanique, avec le but de trouver une fondation conceptuelle. Cohen pense que l'analyse de Hertz peut être complétée avec une analyse philosophique du rôle des principes de la mathématique, et des concepts fondamentaux, dans les théories scientifiques.

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PREFACE

Hermann Cohen was born on the 7th of April 1842 in Coswig. His father, Gerson Cohen, was cantor at the synagogue and teacher at the local Jewish school, and was in fact Cohen's first teacher.[1] The senior Cohen was the only teacher at the yeschiva in Coswig.[2] Cohen belonged to one of the first generations of Jewish students in Coswig to whom a secular education was available.[3] In 1853 Cohen attended the gymnasium in nearby Dessau, supported by the income from his mother's hat shop. In 1857 he moved to the rabbinical seminary in Breslau, from which he soon departed in favor of secular philosophy at the Breslauer Universität.[4] There he had

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[1] In the following biographical sketch I have taken the following as basic sources: Orlik 1992, Sieg 2003, Cohen 1939. Also see Kinkel 1924, Liebeschütz 1970, Rosenzweig 1937, Cohen 1928. All translations from these sources are my own.

[2] Heymann Steinthal, one of the founders of Völkerpsychologie, was also a student of Gerson Cohen's. He undertook a study of the Coswig yeschiva upon Hermann Cohen's death, and has the following to say : "Der Vater hatte eine Jeschiwah besucht und war in profanen Wissenschaften ein Autodidakt. Er hatte diese aber so gründlich betrieben, daß er französischen Unterricht ebensogut erteilte wie hebräischen und deutschen, daß er *le guide des égarés* ebenso flott las wie den *More newuchim* [that is, he read Maimonides' *Guide for the Perplexed* in French as well as in Hebrew]… Gerson Cohen, der Vater des großen Philosophen, lehrte aber nicht nur fleißig, sondern er 'lernte' noch viel mehr. Wenn man ihn nicht in einem großen Folianten vertieft fand, so war er mit einem deutschen Klassiker oder mit einem naturwissenschaftlichen Buche beschäftigt. Er war in politischer Beziehung Demokrat… Er war aber auch Sozialist: Sein Dienstmädchen mußte am Familientische die Mahlzeiten mit einnehmen. Endlich war er Patriot. Im Jahre 1870 ging er trotz seiner großen Frommigkeit an dem bei Beginn des Krieges abgehaltenen Bettag in die Evangelische Kirche, da für die nur noch wenigen jüdischen Familien in der Synagogue kein Gottesdienst abgehalten werden konnte" (Steinthal 1918, 223).

[3] Orlik 1992, 11. It was not just "available," however. According to Ernst Werner's *Geschichte der Stadt Coswig-Anhalt*: "Die sogen. Emanzipation der Juden hat in Anhalt-Bernburg und somit auch in Coswig mit dem 1. Januar 1810 begonnen. Die Annahme erblicher [sic] Familiennamen wurde ihnen zur Pflicht gemacht; so nannte sich der 1809 noch Levi Moses hieß im Jahre 1810 Blumenthal. Sterbelisten der jüdischen Gemeinde wurden geführt, die Kinder mußten irgendwelche Schulen besuchen und die unter 16 Jahre alten mußten sich einem ordentlichen bürgerlichen Beruf zuwenden" (cited in Orlik 1992, 12).

[4] We can make the general point that the position of Jewish thinkers in Prussia in the mid to late 19th century was at best uneasy. Sieg notes, in a passage worth quoting in full: "Im Selbstverständnis jüdischer Philosophen vor 1871 lag aus heutiger Perpektive weit Entferntes dicht beieinander: Bekenntnis zu aufklärerischen Werten und romantische Verherrlichung der deutschen Kulturnation. Vor diesem Hintergrund entwickelten Moritz Lazarus und Heymann Steinthal die Völkerpsychologie: ideengeschichtlich betrachtet, ein eigentümliches Amalgam aus spätromantischem Ganzheitsdenken, Herbartscher Erkenntniskritik und empiristische verstandener Psychologie, dem keine lange Entfaltungszeit beschieden war" (Sieg 1996, 611). The enlightenment values of the salons, the influence of Schleiermacher and Schiller, were felt still. But in this connection compare Hans Liebeschütz's essay on German Jewish academics, where Liebeschütz observes that the late 1870s were: "Eine[r] Zeit, als die Richtung von Denken und Handeln der Mittelklasse durch Begriffe liberaler Zielsetzungen umschrieben werden konnte. Der Umbau von Institutionen mit alter Tradition

an early success, studying mostly with the idealist Julius Braniß, who had been an associate of Schleiermacher's. In 1863 Cohen presented a Prize Essay in which, according to the prize question:

> Plato's doctrine of the essence and nature of the human soul [*Seele*] as developed in the dialogues *Phaedo, Philebus*, the *Politeia* and the *Timaeus*, and Aristotle's psychology (as set forth in the books on the soul [*Seele*] and further developed in passages of the *Nicomachean Ethics* and in the *Metaphysics* Book Lambda) should be presented alongside each other, compared with respect to their agreement and disagreement and finally judged according to their scientific value.[5]

Cohen's work won the faculty prize, and was judged as finding "the true value of Aristotelian psychology in the specialized continuation of Platonic thought."[6] This was not only an early success, but also an early sign of Cohen's later doctrine that a unification of Aristotle and Plato was to be found in an interpretation of Aristotelian logic in light of the Platonic ideas.

Cohen's success in Breslau was not unalloyed. In a competence examination (*Reifeprüfung*) of 1864 he showed weaknesses in classical languages, but was judged mature enough for further study.[7] The judges recommended that Cohen expend significant effort improving his knowledge of classical languages and literatures. While Cohen was interested in classical philosophy, he did not remain in Breslau to

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wurde zur Forderung des Tages. Überlieferte Verschiedenheiten und Abgrenzungen von Gruppen schienen in dem neuen Leben, als daß man schon damals den Aufstieg der Technik empfand, ihre Bedeutung verloren zu haben. Als der junge Cohen die psychologischen Ursprünge des Erkennens untersuchte und dabei den Anfängen der Kultur im Mythos nachging, lag ihm der Gedanke fern, daß seine theoretischen Fragestellungen und die Folgerungen, die sich daraus für das Tun des Menschen ergeben, auch irgendwie durch seine Situation als Jude geformt waren" (Liebeschütz 1970, 8).

[5] In the original: "Platons Lehre von dem Wesen und der Natur der menschlichen Seele aus den Dialogen Phädon, Philebos, Politeia und Timäos entwickelt, und die Psychologie des Aristoteles, wie sie in den Büchern von der Seele dargelegt und durch Stellen der Nikomachicshen Ethik und des Buches Lambda der Metaphysik ergänzt wird, sollen einander gegenübergestellt, in Bezug auf Uebereinstimmung und Abweichung verglichen und hinsichtlich ihres wissenschaftlichen Werthes beurtheilt werden." Cited in Orlik 1992, 25. Source: UB Marburg: XX B 49 oi. *Bericht der Facultäten,* published by Grass, Barth & Comp., 1863, in Breslau. 11 pgs.

[6] In the original: "[D]en wahren Werth der Aristotelischen Psychologie in der specialisierenden Fortführung des Platonischen Gedankens," cited in Orlik 1992, 26.

[7] Orlik 1992, 24.

pursue that interest. Instead, in 1864 he moved to Berlin to study with the philosophical faculty at the Humboldt University.

Two of Cohen's most significant professors in Berlin were Heymann (Chajim) Steinthal[8] and Adolf Trendelenburg.[9] In these professors Cohen found a contemporary manifestation of the important conflict between Platonic and Aristotelian thought in Germany, as well as two very different examples of the beginnings of the new discipline of *Erkenntnistheorie*, an early form of epistemology. Trendelenburg was a famous neo-Aristotelian who had studied with Reinhold, Schleiermacher, and the logician Twesten.[10] Trendelenburg's doctoral dissertation was on the Platonic Ideas, and his *Habilitationschrift* on Aristotle's categories. Some of Trendelenburg's most important works were on Aristotle and Plato: *Elements of Aristotle's Logic* (1836); *On the Structure of Plato's Philebus* (1837); and *Commentary on the Elements of Aristotle's Logic* (1842).[11] Trendelenburg would become a transitional figure for Cohen, and in matters that went beyond the particular study of classical philosophy. Trendelenburg developed an independent system of *Erkenntnistheorie* in the seminal work *Logical Investigations* (*Logische Untersuchungen*), the first edition of which came out in 1840. He was a determined opponent of Hegelianism in history as well.[12]

Though Cohen studied with Trendelenburg at the Humboldt, he did not end up forging a career with him. Relations were broken off almost entirely after Cohen submitted the Prize Essay "Chance and Necessity" ("Zufall und Notwendigkeit") to Trendelenburg in 1865. Trendelenburg was known for his demanding judgments, and his assessment of Cohen was mostly negative.[13] Trendelenburg observes that Cohen's essay indulges in speculation instead of restricting itself to evaluation of the

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[8] Steinthal went by both names; Chajim was his given Hebrew name, and Heymann the Germanized version.

[9] Cohen's dissertation was submitted with the following list of professors: "Scholis usus sum virorum Ill. Boeckh, Du Bois Reymond, Haupt, Steinthal, Trendelenburg, Werder" (Kinkel 1924, 39).

[10] Wesseling 1997.

[11] *Elementa logicae Aristotelicae*, *De Platonis Philebi consilio*, and *Erll. Z. den Elementen der aristotelischen Logik*.

[12] See, e.g., *Die Logische Frage in Hegels System. Zwei Streitschriften* of 1843.

[13] See Geiger 1918 for an account of Cohen as „Erneurer," also see, e.g., Sieg 2003. As Sieg notes, Trendelenburg's judgment can be found in the archive: UAHU Berlin (the archive of the Humboldt University), Phil. Fak. 1504, fol. 312r-313v.

given texts. In particular, he faults Cohen for presenting "the greater part of the *Critique of Pure Reason*" in his critique of ancient philosophers.[14] It is interesting that Trendelenburg had faulted Cohen for a trait that would later bring him renown (and to be fair, also criticism!) as the renewer ("Erneurer") of the Platonic philosophy through Kantian critique.[15]

After his Prize Essay had failed to win, Cohen submitted his doctoral dissertation not to Trendelenburg but to the philosophical faculty in Halle. Here he passed an oral examination only with great effort, amid accusations that his thought in general was "unclear."[16] Cast down by his difficulties, Cohen begins a letter to his friend and later brother-in-law Hermann Lewandowsky with a statement of deepest chagrin: "Until now I haven't done anything, just run around, listened to colleagues, visited people, and went back to my apartment slowly – heavy at heart and empty! I can only tell you, I feel very unhappy and upset and, I'm afraid, chronically so."[17] Several pages later, though, Cohen takes comfort from Trendelenburg's judgment of his Prize Essay: "Soon I'll send you Trendelenburg's judgment, it's very perceptive" (*Ibid.* 19). Cohen emphasizes the positive aspects of Trendelenburg's judgment, and given the opening lines of the letter it's likely he was not trying simply to put a good

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14 "[E]in großen Teil der *Kritik der reinen Vernunft*."

15 Several factors would seem to absolve Trendelenburg. First, the rules were that the *Preisschrift* had to be written in Latin, a language in which Cohen's abilities were below average for a German university student. Further, the records from this time show Cohen in two lights. First, he was full of self-confidence: unusually so for someone so early on in his career, and with so little solid work behind him (Sieg observes this as well, but also see Cohen 1939). Cohen had found his method: the analysis of Platonic Ideas through the Kantian critique. He was working on this method already. If it had not succeeded yet, that was no reason to lose faith in the method in whose virtues Cohen was persuaded. Second, Cohen was of an irreducibly combative temperament. At the age of 18, Cohen sent a letter to Samson Raphael Hirsch, the leader of the Orthodox separatist movement in Breslau, who was engaged in a conflict with the conservative leader of the Breslau rabbinical seminary Zacharias Frankel. Cohen was Frankel's student, and defended Frankel's piety against Hirsch's attacks in his letter. Much to Cohen's surprise, Hirsch promptly published parts of the letter in his journal (Cohen 1939, Introduction 6). Cohen's combative temperament continued undeterred, and later is noted by his student Paul Natorp as an "ausgesprochene Kampfesphysiognomie" (Natorp 1918, 3). With these facts behind us, we can make an educated speculation: Cohen would not have cut his sails to the prevailing winds, and may have presented an iconoclastic text either on purpose or because his interest was in shaking up the received wisdom. That would explain Trendelenburg's puzzlement with the direction and focus of Cohen's work (he objects that it was not focussed on "the assigned task").

16 Cited in Orlik 1992, 30.

17 "Bis jetzt habe ich Nichts gearbeitet, mich umhergetrieben, Collegien gehört, Besuche gemacht, nach Wohnung langweilig gesucht – öde und leer!... Ich kann Dir nur sagen, ich fühle mich sehr unbehaglich und mißgestimmt und ich fürchte, chronisch." Berlin, 8. Nov. 1865, in Cohen 1939, 17.

face on things to his friend. Cohen would have been worried, perhaps, about Trendelenburg's generally negative judgment of his work. However, Trendelenburg concedes that the essay "has much that is praiseworthy" (*Ibid.* 19).

In his difficult material conditions and intellectual crisis in Berlin, Cohen was grateful for the friendship and instruction of someone who had been a fellow student of his father's in Coswig and who was now Extraordinarius in Berlin: Heymann Steinthal. Steinthal, as Cohen himself reports, was his professor in "linguistic psychology and comparative mythology," which reveals Steinthal's unconventional turn of thought.[18] The importance of Steinthal and of his fellow founder of Völkerpsychologie, Moritz (Moses) Lazarus, for Cohen's early career is beginning to receive more notice.[19] Cohen spent a brief period of his early career in Berlin working with the nascent Völkerpsychologie movement, through which Lazarus and Steinthal adapted Herbart's psychology to a broader set of purposes including comparative anthropology, history and linguistics. Cohen was influenced personally by Steinthal, as evidenced by a letter of the period: "Now I'll tell you again that the man is very profound, that he has worked up to a rare clarity and depth."[20]

During the period of Cohen's association with Lazarus and Steinthal he published a number of essays in the journal they founded, the *Journal of Völkerpsychologie and Linguistics*.[21] The most significant essay for Cohen's early philosophical career appeared in 1871, on the subject of a debate over Kant's transcendental idealism, the Trendelenburg-Fischer debate. Cohen's former professor Trendelenburg had raised

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[18] Cohen 1928, 441.

[19] See among others Köhnke 2001; Sieg 2003; Adelmann 1997; Belke 1986. There are several reasons why that influence would have been played down. Sieg points out the following: "Bis auf den heutigen Tag erscheint die Beziehung Hermann Cohens zur Völkerpsychologie rätselhaft. Trotz vermehrter Forschungsanstrengungen ist nicht recht deutlich geworden, warum der junge Philosoph sich für die Ideen der Völkerpsychologie interessierte. Gewiß, ihre Hauptvertreter Moritz Lazarus und Heymann Steinthal besaßen ein beträchtliches Ansehen. Und die von ihnen begründete *Zeitschrift für Völkerpsychologie und Sprachwissenschaft* galt schon bald als innovatives Organ für fächübergreifende Fragen. Dennoch bleibt es seltsam, daß der spätere Großmeister axiomatischen Philosophierens nicht weniger als fünf Jahre im Banne einer empirisch ausgerichteten Denkschule gearbeitet haben soll" (Sieg 2003, 461). I will not pretend to answer the "puzzle" here. In the story I've told thus far it remains unclear why Cohen would have chosen Steinthal's interpretation of the Platonic Ideas, for example, as opposed to anyone else's.

[20] Cited in Kinkel 1924, 39.

objections to Kantian epistemology. Cohen's essay was "decisive" to the debate.[22] Later that year he published *Kant's Theory of Experience*, which detailed the consequences of the debate for Kantian doctrine.[23]

For the moment, in the early 1870's, Cohen was again in career difficulty. He used *Kant's Theory of Experience* as his Habilitationsschrift, which would have qualified him for a position as professor. In the text Cohen criticized Trendelenburg's Kant interpretation sharply. It was hardly surprising, then, that Trendelenburg and the others on the committee rejected his petition.[24] After Trendelenburg died, Cohen tried again in 1873, this time appending his text *Kant's Groundwork for Ethics* and thus submitting two-thirds of his re-casting of Kant's entire critical system. (The series would also include *Kant's Groundwork for Aesthetics*.)[25] Again Cohen's submission was rejected, this time by Trendelenburg's successor, the prominent historian of philosophy Eduard Zeller, among others.

Fatefully, and fortunately for Cohen, he decided to send a copy of *Kant's Theory of Experience* to the neo-Kantian (*Ordinarius*) professor Friedrich Albert Lange in Marburg. Lange was persuaded of the merit of Cohen's work on first reading.[26] Lange was a political dissident and was sympathetic to Cohen's professional near-ostracizing. With Lange's support, Cohen successfully submitted a Habilitations-schrift in Marburg, after some resistance from Kuno Fischer, whose oxen Cohen had gored in his essay on the Trendelenburg-Fischer debate. Lange made many efforts to get Cohen an academic place, but to no avail. When Lange died in 1875, the faculty needed someone to carry on his work, and Cohen had been associated closely with Lange. There were objections on religious grounds from the botanist Albert Wigand and the theologian Ernst Ranke. Nonetheless, in 1876 Cohen was appointed to Lange's former professorship.

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[21] I have not translated "Völkerpsychologie" here or elsewhere. "Folk psychology" and "ethnopsychology" are associated with distinct movements, thus are inappropriate, and a clear translation isn't easy to find.

[22] Holzhey 1972, 48.

[23] *Kants Theorie der Erfahrung*. Cited as Cohen 1871.

[24] See Sieg 2003, as well as Köhnke 1986, 131-135.

[25] *Kants Begründung der Ethik* appeared in 1877, and *Kants Begründung der Ästhetik* in 1889.

[26] See Cohen 1939, 34 and following.

Between the 1880's and the first decade of the 1900's Cohen's intellectual life began to take definite shape, determined by two forces: he began to work with Paul Natorp, and he began to turn back to Jewish thought and community life, partly in response to a rise in anti-Semitism in Germany.[27] He composed some of his most significant systematic texts during this period, including *The Principle of the Infinitesimal Method and its History* (1883), the *Logic of Pure Cognition* (1902), the *Ethics of Pure Will* (1904), and the *Aesthetics of Pure Feeling* (1912). These texts are cornerstones of the Marburg School of Neo-Kantianism, which Cohen and Natorp founded on the basis of Lange's earlier work in Marburg. Cohen's work in Jewish philosophy flourished in the 1880s and 1890s as well. Further, Cohen began to take part in the public debates over the role of German Jews in society, which would become one of his most lasting intellectual contributions. In 1879 the historian Heinrich von Treitschke published the pamphlet *Unsere Aussichten,* a scathing polemic chastely clothed in academic prose, which questioned the possibility of Jewish assimilation into and contributions to German society. Cohen and Steinthal took opposing positions in the ensuing debate. Cohen argued that the Christian and Jewish intellectual traditions in Germany have a common conceptual and historical (he called it "ideal") basis and that the social traditions, therefore, could be reconciled to each other. Steinthal considered this line of thought to be assimilationist. Worse, he considered Cohen to have knuckled under to von Treitschke's anti-Semitic claims to the effect that what is distinctive about Jewish society and thought must be repressed in the interest of social and political harmony in Germany. The so-called "Berlin Antisemitismusstreit" was the end of the friendship between Steinthal and Cohen.[28]

The "Marburg School" of neo-Kantianism that Cohen founded became one of the most influential schools of philosophy in Germany. The historian and philosopher of science Ernst Cassirer was Cohen's student, and the logician Paul Natorp was his colleague; both were members of the Marburg School. The school included August Stadler, Albert Görland and Arthur Liebert as well. Cohen

[27] Here I am following Ollig 1979, 32-3.

[28] For a reprinting of most of the original source material of the debate, including Steinthal's, Lazarus' and Cohen's writings, see Boehlich 1965. For an exchange of letters between Lazarus, Steinthal and their associates on the subject, see Belke 1986.

influenced scholars and pupils as diverse as Bruno Bauch (before he defected to the rival Southwest School) and a young Boris Pasternak, who came to Marburg to study with the "genius Cohen" (but did not complete his studies). Pasternak's characteristically perceptive remarks are worth citing here:

> A creation of the genius, Cohen, prepared by his predecessor in the Chair, Frederick [sic] Albert Lange, famous to us for his *History of Materialism*, the Marburg School attracted me by its two characteristics. In the first place it was independent; it uprooted everything from its first rudiments and built on a clear space. It did not accept the lazy routine of all conceivable "isms," which always cling to their stock of omniscience at tenth hand, are always ignorant, and always for some reason or another afraid of a revision in the fresh air of age-old culture... If current philosophy tells what this or that writer thinks, and current psychology, of how the average man thinks; if formal logic teaches us how to think in a baker's so as to get the right change, then the Marburg School was interested in how science thinks in its twenty-five centuries of uninterrupted authorship, at the burning commencements and conclusions of the world's discoveries. In such a disposition, authorized, as it were, by history itself, philosophy was unrecognisably rejuvenated and made wise, transformed out of a problematic discipline into an immemorial discipline of problems, which is what it ought to be. The second characteristic of the Marburg School derived directly from the first and consisted in its selective and exacting attitude to historical development...They knew history in its entirety at Marburg, and were never weary of dragging treasure after treasure from the archives of the Italian Renaissance, from French and Scottish Rationalism and other badly studied schools. At Marburg they gazed at history through both of Hegel's eyes, i.e., with brilliant universality, but at the same time within the exact boundaries of a judicious verisimilitude. So for instance, the school did not speak of the stages in the development of the "Weltgeist," but, say, of the postal correspondence of the Bernoulli family, though it knew that every thought of

> however distant a time, surprised in its place and at its task, must be laid bare to our logical commentary.[29]

Between 1876 and 1912 Cohen and his Marburg School had a profound influence on German academia.[30] Cohen's students included August Stadler, Albert Görland, and the philosopher of law Rudolf Stammler. The Marburg School's socialist theory was a foundation for the theories of Karl Vorländer and Franz Staudinger. This tradition of Marburg socialist theory came to influence Eduard Bernstein (a student of Marx's), Ludwig Woltmann (a Marxist socio-biologist), and Hans Kelsen (a legal theorist) as well. In his history of German philosophy, Friedrich Überweg traces the influence of the Marburg School in the early thought of Kurt Sternberg,[31] Erwin Schneider, Hans Reichenbach, and the Hegelian Berthold Kern.[32]

By 1912 Cohen had held an *Ordinarius* professorship for 36 years. He had never been asked to be rector of the University, though he had become famous and made the name of Marburg famous with him.[33] This honor was usually considered a formality for a prominent professor. It was a humiliating blow to Cohen when, after he became Emeritus, an unknown physiologist was chosen to replace him instead of Ernst Cassirer, his chosen successor. The blow was made worse given that the survival of the Marburg School of philosophy was endangered. Cohen left Marburg in 1912, to teach at a Jewish school in Berlin. Pasternak remembers his "farewell words before his retirement were on his faithfulness to great philosophy, delivered to the university in such a way that among the benches, where there were many young listeners, handkerchiefs gleamed."[34] One of Cohen's most famous works of Jewish

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[29] Pasternak 1958, 41-2.

[30] In the account of the Marburg School's influence that follows I am summarizing Friedrich Überweg's account in Überweg 1951, §40, 446 and following.

[31] Sternberg was the author of *Staatsphilosophie* (first edition 1923), *Philosophische Probleme im biblischen und apokryphen Schrifttum der Juden* (1938), *Zur Logik der Geschichtswissenschaft* (second edition 1925), and *Beiträge zur Interpretation der kritischen Ethik* (1912).

[32] Überweg 1951, 447-8. Überweg cites as evidence for the Marburg influence on Reichenbach the work *Der Begriff der Wahrscheinlichkeit für die mathematische Darstellung der Wirklichkeit*, in the *Zeitschrift für Philosophie und [philosophische] Kritik*, Bd. 161.

[33] See Ollig 1979, 34 and following.

[34] Pasternak 1958, 73, trans. Babette Deutsch.

philosophy, the *Religion of Reason from the Sources of Judaism*, appeared posthumously in 1919.

Hermann Cohen died in 1918. A plaque erected for him in his birthplace, Coswig, remains in its original place undisturbed, one of the few remaining public monuments to Jews that survived from before World War II.

Review of the literature

Cohen's publications in Marburg fall into four general categories. First are the above-mentioned writings on Kant's system. Second are his own systematic writings, according to the Kantian model: the *Logic of Pure Cognition* (1902), the *Ethics of Pure Will* (1904), and the *Aesthetics of Pure Feeling* (1912). Third are Cohen's writings on science, logic and his epistemology, which he called *Erkenntniskritik* or knowledge-critique; among these writings are *The Principle of the Infinitesimal Method and its History* (1883) and his introduction to Lange's *History of Materialism* (1896). Fourth are the writings on Jewish philosophy: the controversial *Deutschtum und Judentum* (which is well-nigh untranslatable), and the enduring *Religion of Reason from the Sources of Judaism* (published posthumously in 1919 from Cohen's notes).

The first secondary literature on Cohen was written within the Marburg School. A special issue of *Kantstudien* was produced for 1918, the year he died. Paul Natorp, Ernst Cassirer and Albert Görland all published articles in the issue evaluating the importance of Kant for the Marburg School and for Cohen's philosophy specifically. In the same year Natorp produced a short book on Cohen's system, called *Cohen's Philosophical Project from a Systematic Perspective.* Walter Kinkel's *Hermann Cohen* was published in 1924. Kinkel's book is a short introduction to Cohen's work and a very good intellectual biography.

Systematic Cohen interpretation had a new beginning in Germany with the publication of Helmut Holzhey's *Cohen und Natorp* in 1986. Holzhey's book presented Cohen's and Natorp's philosophical systems, reprinted a number of letters, and provided an extensive bibliography. Holzhey is the administrator of the Cohen-Archiv in Zurich, which is producing a series of editions of Cohen's works. Holzhey has also produced a number of editions of secondary critical essays on and excerpts from the primary texts of the major schools of Neokantianism, including among others: *Erkenntnistheorie und Logik im Neukantianismus* with Werner Flach (1980, Gerstenberg), and *Neukantianismus. Perspektiven und Probleme* with E. W. Orth (1994, Würzburg).

In 1986, Klaus Christian Köhnke published *The Rise and Fall of Neo-Kantianism* (Köhnke 1986). Köhnke's work is more sociological in focus than Holzhey's *Cohen und Natorp*. He uncovered new historical and contextual territory, including a detailed map of the influence of Trendelenburg and the beginnings of *Erkenntnistheorie* in late Kantian and Hegelian idealism. In 1988, Geert Edel published a monograph *From the Critique of Reason to the Logic of Knowledge (Von der Vernunftkritik zur Erkenntnislogik)*, which tracks the development of Cohen's system from Kant-interpretation to Cohen's own theory. Ulrich Sieg has written a book of history on the Marburg School in general, *The Rise and Fall of Marburg Neo-Kantianism* (*Entstehung und Aufstieg des Marburg Neukantianismus*), as well as a number of books and articles dealing with Cohen in part (for instance, *Jüdische Intellektuelle im ersten Weltkrieg*).

Franz Rosenzweig was among the first to comment on the connection between Cohen and Völkerpsychologie, which is of renewed interest in Germany (Rosenzweig 1937). Klaus Köhnke wrote a recent article on the relationship (Köhnke 2002). Ulrich Sieg (2003), Dieter Adelmann (1997) and Winrich de Schmidt (1976) have written on Steinthal and Cohen. De Schmidt's book is devoted entirely to the relationship between Völkerpsychologie and the systematic thought of Cohen and Natorp. Cohen himself commented briefly on his relationship with Steinthal in a memorial for his pupil August Stadler, whom Steinthal had sent to study with him (Cohen 1928 [1910], 440-1).

A controversy between Michael Dummett and Hans Sluga over the philosophical context to Frege's work led to more rigorous historical study of the psychological and neo-Kantian philosophical movements. Dummett's *Frege: Philosophy of Language*, first published in 1973, was followed by the more reflective *Origins of Analytical Philosophy* of 1993 and *Frege and Other Philosophers* of 1991. Sluga's commentary appears in *Gottlob Frege* of 1980 and *The Philosophy of Frege* of 1993.

In North America J. Alberto Coffa's 1991 book *The Semantic Tradition from Kant to Carnap: To the Vienna Station* introduced a new generation of scholars to the Marburg School. Michael Friedman, in his *Kant and the Exact Sciences* (1994), *Dynamics of Reason*

(2001) and *A Parting of the Ways* (2000) and Don Howard (in, for instance, "Relativity, *Eindeutigkeit*, and Monomorphism" 1996) have written on the Marburg School, especially Cassirer.

My study of Cohen focuses on his own systematic thought, and specifically on his writings on epistemology, which he identified with philosophy of science and called "Erkenntniskritik" or knowledge-critique – the third category above. Thus I am not interested primarily in Cohen's writings on Kant's systematic philosophy per se, but on his application of Kant's ideas to contemporary problems. In this particular focus my work is distinct from that of many commentators on Cohen. My work includes a detailed investigation of Cohen's account of the boundary between science and philosophy, and more generally on the relation between science and culture. With this focus I can locate Cohen's work in the contemporary work on the history (and philosophy) of science, on which discipline Cohen's student Cassirer was an early influence.[35] In that context, I examine Cohen's foundation for the so-called "infinitesimal method" in function theory and rational mechanics, and discuss at length his interpretation of Heinrich Hertz's *Principles of Mechanics*. No major study of Cohen's seminal work in this tradition has appeared as yet.

[35] According to Michael Friedman, Cassirer's fundamental contribution was to "develop a detailed reading of the scientific revolution as a whole in terms of the 'Platonic' idea that the thoroughgoing application of mathematics to nature…is the central and overarching achievement of this revolution," and this contribution "was acknowledged as such by the seminal historians, Edwin Burtt, E.J. Dijksterhuis, and Alexandre Koyré, who developed this theme later in the century in the course of establishing the discipline of history of science as we know it today" (Friedman 2002).

INTRODUCTION

The following is a systematic presentation of Hermann Cohen's history and philosophy of science. Cohen's version of the neo-Kantian turn "back to Kant" takes the relationship between philosophy and science to be the central problem of epistemology.[36] That problem took a particular form at the time that Cohen was working. The natural sciences made stunning progress and, more fatefully for philosophy, a significant number of the empirical scientists who were instrumental in that progress were also philosophers. The goals and methods of philosophy became progressively more identified with those of the sciences. Cohen's real contribution to the philosophy of science is his argument that philosophy can help to realize the "ideal goals" of the sciences. That is, Cohen argued that the purposes of philosophy and science can be identical without philosophy having to abandon the idealist position in favor of materialism, empiricism or "psychologism." Cohen's interpretation of, and radical revisions to, Kantian doctrine allowed him to develop an epistemology that did not require speculation beyond the "facts of science." Cohen argued that the best way to pursue his goal of philosophy as a complement to the sciences is to develop a method for the history and philosophy of science that takes as its evidence the facts established by scientific theories. In the brief introduction that follows, I will outline Cohen's history and philosophy of science as I present it.

My goal is to present the argumentative and conceptual support for Cohen's philosophy of science. My methodology is akin to the case study method. I take two intellectual problems on which Cohen developed a clear and decisive perspective as source material. The first was the debate in the early 1800s between Adolf Trendelenburg and Kuno Fischer over the Kantian philosophy of science. The second was the revision to Helmholtz's philosophical foundations of physics by his most brilliant student, Heinrich Hertz. Hertz proposed a mathematical "picture" theory of knowledge to replace Helmholtz's "sign" theory. I demonstrate that Cohen was among the first to work through the philosophical implications of the picture

36 See, among many others, Holzhey 1972, 49; Cassirer 1918, 252 and following.

theory. I present Cohen's method in the philosophy of science as manifest in his texts taking part in these debates. The two case studies I investigate are the bookends of Cohen's career. When he began to study philosophy, his concern was how to adopt a broadly Kantian theory to the advance in the sciences. By the end of his career his interests had broadened.[37] In *The Principle of the Infinitesimal Method* and especially in his introduction to the *History of Materialism*, his goal was to give a philosophical grounding for mathematical physics.

Before discussing the questions in more detail, I should make a few remarks about why I find the case study methodology useful in this context. First, it reveals clearly the limits and virtues of Cohen's account, because it shows through historical fact as well as bare argument the degree to which he was or was not able to deal with a given problem. Further, in Cohen's case the method is well advised because Cohen's career in philosophy is marked by conflict from the beginning. Anyone reading the primary texts of Cohen's work and, even more so, his biography, will see that some of his best and most enduring work was written in debate with other philosophers (in *Streitschriften* of various kinds).[38]

In character, then, the main texts I examine in the following are *Streitschriften*. The first problem I address was the question of how to go forward with Kantian theory in the philosophical climate post-1830, which was hostile to "speculative" idealism. Various explanations have been given for the hostility, among them the impact of Hegel's death and the brutal "realism" of society in general.[39] I present this issue in a

[37] Here at the outset, I should make it clear that no part of my discussion of Kantian doctrine should be taken as straightforward exegesis of Kant's texts. No major strain of neo-Kantianism was willing to limit itself to what many in the tradition referred to as "Kant-philology," or more scornfully still as "Kantian scholasticism" (See, among others, Lange 1877, 1-3; Natorp 1918, 196 and following.) Cohen in particular argued that Kant's texts are only tools to be used to solve problems. In general, the Kant interpretation of the Marburg school follows the principle that the house of Kant can be renewed only by being gutted and restored, perhaps with certain structural elements replaced entirely. My project is concerned solely with Cohen's own history and philosophy of science. As such, I do not address his works of Kant interpretation except when they are relevant to the works discussed here. When I do cite Cohen's Kant scholarship it is outside the scope of my project to evaluate whether Cohen is correct in his assessment. There is a robust literature on the subject, to which I refer the reader who is primarily interested in this tangential issue. (For instance, one could begin with the following: Edel 1988, Holzhey 1986, Ebbinghaus 1968, Hamburger 1873.)

[38] See the Preface for evidence of this, footnote 12 especially.

[39] For the first explanation see Schnädelbach 1983, 118-119: "The *Zeitgeist*...simply turned its face away from philosophy in general, in order to pursue science in a post-Hegelian sense." For the second explanation (the realism of society in general) see, e.g., Überweg 1951, 309: "The general trends of the

more detailed and specifically Kantian context than it is usually given. I argue that the post-Kantian philosophical systems of Hermann von Helmholtz and Johann Friedrich Herbart were the main streams of what was called *Erkenntnistheorie*, or what we would now call epistemology. I show how Cohen develops a neo-Kantian perspective on epistemology distinct from either Helmholtz's or Herbart's.

The text I examine in this context is Cohen's 1871 essay on the debate between an Aristotelian, Adolf Trendelenburg, and a Hegelian, Kuno Fischer, on the proper interpretation of Kant's Transcendental Aesthetic. Cohen puts the question in the broader context of the epistemological quest to find the principles of knowledge. He points out that recent Kantian theories had given only causal or descriptive accounts of spatial and temporal properties, and admonishes that the only way to construct a viable neo-Kantian theory is to draw the line at all psychology. As I will show in my review of Cohen's essay, in this way Cohen rules out any explanation of space, time or the categories that originates in the physical, physiological or psychological "subject" as first cause. This distinguishes Cohen's approach from Helmholtz's or Herbart's, and even distinguishes him from his mentor, Friedrich Albert Lange, who had extended Helmholtz's neo-Kantian approach.

Cohen's essay on the debate gave him a perfect opportunity to air his views on the future of neo-Kantian philosophy. Trendelenburg argued that a causal explanation could be reconciled with Kant's philosophy of science if only neo-Kantian philosophers were willing to abandon Kant's claim that spatial and temporal properties are not objective but originate in the subject's "cognitive faculties." Of course, the empirical fact that causal explanations for the phenomenon of representation were becoming more and more sophisticated at the time bolstered Trendelenburg's argument. Kuno Fischer retorted that to abandon Kant's doctrine of the pure subjectivity of space and time would be to abandon Kantian transcendental idealism and thus to jettison the core of his philosophy. However, Fischer thought that in his capacity as defender of Kant he had free rein to reconstruct Kant's philosophy as he chose, according to what he called the "freely

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time [the mid to late 19th century] were thoroughly un-idealistic in nature… Just as there was an inclination toward the form of realism in art, which reflected the realities of action, so in theoretical matters there was a turn away from all metaphysics toward the 'real,' that is, toward that which can be directly perceived."

constructed method" (*frei nachbildende Methode*). Thus Trendelenburg's challenge to Kant, and Fischer's response, provoked questions about the future of Kant's system, but also about the proper method of telling the history of philosophy.

Cohen argues that both Trendelenburg and Fischer are wrong, but for different reasons. He disagrees with Trendelenburg regarding the causal account of reasoning, and gives a provocative but mainly negative argument against such an account. However, he admits that he does not give a positive argument for a critical theory in the essay, leaving such an argument for his forthcoming books *Kant's Theory of Experience* and *The Principle of the Infinitesimal Method.* On the question of philosophical history, Cohen makes several interesting remarks. He sides with Trendelenburg's accusation that Fischer has not understood Kant, and agrees that it is partly due to Fischer's irresponsible attitude toward history. Cohen argues that Kant's philosophy is not a set of isolated doctrines that can be taken up and abandoned at will. Rather, according to Cohen's interpretation Kantian critical theory is a systematic *method* for philosophy, or as Cohen often puts it, for "philosophizing (*Philosophieren*)." Cohen accepts that neo-Kantianism must take scientific progress into account and must deal with the problems with the Kantian theory which Trendelenburg cites. However Cohen argues in addition that Trendelenburg did not engage with Kant's philosophy as a system.

Cohen's essay on the debate between Trendelenburg and Fischer is one of the first statements of his view that the Kantian philosophy should be understood as a system, as a method for philosophizing. In his later works, *The Principle of the Infinitesimal Method and its History* and his introduction to Lange's *History of Materialism*, Cohen replaces his initial understanding of Kant with his own systematic method for the philosophy of science. In Part Two, I emphasize how distinct Cohen's work is from the "logicist" tradition, which includes Trendelenburg. Cohen's approach is to use a historical method in conjunction with a hierarchy of the sciences. The idea is to start with the objective, proven facts of science, or of those sciences that operate at the "ground level," such as mechanics, chemistry, and experimental physics. Cohen takes the *fact* of our basic grasp of the results of scientific enquiry as the basis for analysis. The project is then to take that which gives scientific theories and propositions their unity and coherence, the conceptual functions and relations that make it possible for us to classify the phenomena and to unify them under general causal laws, as the subject of comparative evaluation.

Cohen's work in history and philosophy is a significant response to advances in electrodynamics and in mathematical physics in general. Part Two of the following work offers an evaluation of the relationship between Heinrich Hertz, a student of Helmholtz, and Cohen. Cohen was a philosophical observer of Hertz's response to Helmholtz's attempt to reconcile classical mechanics with electrodynamics. This moment is important for any understanding of 19^{th} century neo-Kantian philosophy of science, since all three (Helmholtz, Hertz and Cohen) were neo-Kantians. In my thesis I demonstrate how Cohen's theory, according to which the principles of construction of scientific images are mathematical and logical, allows him to develop a plausible perspective on the scientific debates of the time. In this context, in Part Two, I present a discussion of a little-known but significant controversy, the Lübeck controversy.

In 1895 there was a major conference of scientists at Lübeck in Germany. Wilhelm Ostwald presented a controversial paper at the conference in which he argued against the atomistic picture and claimed that all physical processes were in reality transformations of energy. This view became known as energetics. Ostwald's argument, which was echoed by Ernst Mach, was that since atoms had not been observed, the business of physics should be to find general laws for the transformation of *energy* instead of the motion of atoms, in terms of variables such as pressure, temperature, and volume that could be directly verified. Ostwald soon found himself embroiled in arguments with many of the leading scientists of the time, including Ludwig Boltzmann but also, and surprisingly, including Helmholtz. Surprisingly, because Helmholtz had recently formulated and defended the law of *conservation* of energy within a closed system. Ostwald's theory went beyond the empirical data supporting the theory that energy is conserved in any given system; he argued that energy is in fact the substrate of any conversion: in other words, that energy is the basic building block of the universe. Helmholtz, on the other hand, still relied on the equations and hypotheses of Newtonian mechanics. He was able to give a complete and satisfactory description of the available data appealing to the motion of material particles. However according to Ostwald and Mach the atomic material particles to which Helmholtz appealed were not empirically verifiable: that is, they were not among the phenomena accessible to empirical scientists.

Heinrich Hertz, a student of Helmholtz's and a neo-Kantian, later argued that the conceptual functions embedded in Helmholtz's and Mach's theories are in fact formally equivalent. Hertz showed that the Mach-Ostwald energetics and Helmholtz's atomism were functionally equivalent: that is, that they both preserved the essential formal relations for constructing an adequate model of a physical theory.

Cohen argues that, whether or not Hertz is ultimately correct, he has identified the only secure basis for an evaluation of the principles of mechanics. Cohen believes that philosophical research should limit itself to examining the "presuppositions and foundations" inherent in the facts of science. He includes, among these presuppositions and foundations, the mathematical principles that are the ultimate grounding for scientific theories. Cohen argues that the philosophy of science can contribute to the progress of science, and of mathematics, by clarifying the relationship between the two, and by evaluating the goals they have in common.

PART ONE

COHEN AND EARLY NEO-KANTIAN PHILOSOPHY OF SCIENCE

Chapter One

Questions of Epistemology: Genetic and Critical Accounts of Representation

In the 1830's and shortly thereafter an "autonomous discipline called *Erkenntnistheorie,*" or what we call epistemology, began to gain currency in German philosophy.[40] At the time, the discipline was Kantian in origin and in its methodology and assumptions.[41] In what follows I will give a brief sketch of the central problems and preoccupations of the discipline.

Cohen's student Ernst Cassirer observes that around the middle of the nineteenth century Cohen was concerned with the question of how to interpret Kant's epistemology. Cohen saw Kantian theory as asking the right questions:

> For Cohen, Kant's system answers the truly fateful question of philosophy in general: the question of the relation between philosophy and science. The reconstruction of this system from its original impulses takes us into the midst of the historical debate over the continuation of philosophy itself. The value of Kant's doctrine is that this debate is found in his work in its sharpest, most concise expression: it appears as the quintessential revelation of his thought, the pivotal significance of which is attached to no single time and no single school.[42]

The question of how to interpret Kant's epistemology, and in particular his account of the relation between philosophy and empirical science, was crucial for Cohen's early theory. Cassirer observes that to understand Cohen's motivations we need to look at the context of epistemology in Germany at the time:

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[40] Köhnke 1991, 36.

[41] For instance, see Köhnke 1991, 36 and following. The first wave of post-Kantian texts associated with the tradition appeared in the 1830's: Friedrich Beneke's *Kant und die philosophische Aufgabe unserer Zeit*, Schleiermacher's *Lectures on Dialectics* given from 1811 onward and first published in 1839, and Reinhold's *Theorie des menschlichen Erkenntnisvermögens*, first published in 1832. Köhnke's text also gives an admirable sketch of the history of the earliest uses of the term "Erkenntnistheorie" by Reinhold, Schleiermacher and Tennemann. Friedrich Überweg observes that the influence of Kantian thought had a broad influence on German epistemology in general, beyond neo-Kantianism: "This intellectual movement [*Erkenntnistheorie*] had by far the greatest [historical] influence and ultimately had a decisive influence on the renewed vigor of philosophical thought. This turn toward the theory of knowledge brought a renewed connection to Kant along with it. Of all earlier thinkers he had by far the greatest, most extraordinarily profound influence. Not only did Neokantianism spring up and become the most powerful philosophical movement by the end of the century -- all other movements had to deal with Kant as well" (Überweg 1951, 310, my translation).

[42] Cassirer 1918, 253, my translation.

> To sense the full importance of this way of looking at the problem one must place oneself back in the era in which Cohen's studies of Kant began. At the time the basic questions of philosophy appeared to be solved, insofar as they were shared with the disciplines of the natural sciences and were absorbed by them. Any independent methodological awareness of the fundamental presuppositions of knowledge was now regarded as a relapse into dialectic, and the now mature discipline [of philosophy] believed itself to be free, finally, of the demands of dialectic. The meaning and content of knowledge should be determined by specific empirical methods and by the empirical results of particular sciences, rather than by the abstract generalities of speculative reflection.[43]

Cohen's challenge, as we will see, was to recast the problem of knowledge in the Kantian context so that philosophy could make a real, independent contribution to epistemology. To see how he achieved his goal, we will need to look at the rival systems and then at the influences on Cohen's early theory.

The overarching goal of epistemology at the time was to be able to give a single system of principles for philosophy and for empirical science.[44] That is, the point was to be able to explain all knowledge on the model of physics or mechanics, in which a plethora of natural processes are accounted for by a single set of causal laws. This project was meant to provide a justification of the philosophy of science that was as secure as the justification for physics or mechanics.

There are two goals of this first wave of *Erkenntnistheorie*. The first was to banish dialectic from logic, by restricting logic to reflections on the data of empirical science.[45] The idea was that logic should be restricted to those relations of thought

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[43] Cassirer 1918, 253, my translation.

[44] For support of this view see, for instance, Helmholtz 1971 [1878], 368-9; Windelband 1899, 402; Köhnke 1986, 70.

[45] For instance, in a Festschrift for the logician Christoph Sigwart, Heinrich Maier writes that "it has become usual [üblich] in modern logic to conceive of the foundations of logical investigation with respect to the doctrine of method [*Methodenlehre*]. Certainly no one contents himself with tracking down elementary thought in the positive sciences and the use of simple forms of thought in the latter. This was also possible on the basis of traditional [logical] theory. And even the most abstract formal logic, which could have been used by a student of Kant's or Herbart's, can be counted as a *Methodenlehre* or as 'applied' logic in this sense. The 'reform of logic' that has taken place in the last few decades is more profound. It has broken with tradition and led to a new foundation of science on

used to achieve or explain scientific knowledge.[46] Cohen's student Paul Natorp has observed that logicist methods "start with fundamental and irreducible concepts and indemonstrable propositions, and strive toward judgments of identity ('analytic' judgments, in Kant's sense)."[47] The use of logic as a synthetic discipline, capable of making independent contributions to science, was associated with dialectic and the excesses of "speculative idealism." The logicist project of *Erkenntnistheorie* would (so the reasoning went) yield an account of logic as a set of analytic propositions and general principles that describe the facts of science.

The second aim was to give an explanation of space, time and the categories of thought as the results of a natural, causal process.[48] These "genetic" accounts were usually physiological, psychological or physical in nature. Both of the above goals were meant to contribute to the overarching aim of unifying the principles and methods of philosophy and of science, or as Adolf Trendelenburg put it, to "turn the method of science into a science."[49]

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another basis. Its guiding idea is to seek out logical thought in scientific knowledge [*Erkennen*], in scientific methods. Thus logic becomes a doctrine of the *structures and laws of scientific thought*. There is good reason for putting the project in this particular way. Today an agreeable unanimity has been reached, to the effect that the primary object of logic is that type of thought that finds expression in acts of judgment. This excludes from logical interest a considerable number of the functions that one usually includes in the description of thought. Involuntary appearances and spontaneous visitations of perceptions or representations, the uncontrolled play of the course of an idea and voluntary reproduction, the intentional dissolution of series of thoughts for some reason, intuition that withdraws from discursive connections, if there is any such, and whatever else one may consider as "thought" – all this belongs in the pre-logical sphere of thought" (Maier 1900, 219-21, my translation).

[46] In this connection Trendelenburg remarked "One certainly speaks of the methods peculiar to particular sciences, in the sense of different methods, [for instance] of the logic of mathematics, of natural science, or of jurisprudence. These distinct paths are forged by the one thought [*das Eine Denken*], which in various forms always nestles up to the object to grasp it. In the sciences the one thought is only given a push in various ways to find ever new skills, which must give up the object as if captured. But only one skill reveals itself through all these [methods], and in all of them thought is revealed to be only one thing, an entity that can be powerful with few tools. In all [methods] thought seeks the necessary; never does it bring contradiction with it, but on the way [thought] itself uses contradiction to determine the necessary. If we want to develop a path to necessity or to come closer to the path of knowledge of the necessary and to name the degree of resemblance to necessity using rigorous methods, that is how to make the method of science into a science. And if the methods appear in the object of science, but are not given in it, but rather have their general basis in the thought that works through the objects: this works toward the task of finding its origin in the being of thought. In this way the metaphysics of each science leads to *logic*, to the investigation of thought which produces science" (Trendelenburg 1862, 9-10, my translation).

[47] Natorp 1918, 196. My correction of a draft translation by R. Pates.

[48] In this context see Überweg 1868, 1-3; Windelband 1899, 379 and 402; and Hönigswald 1900, 178.

[49] Trendelenburg 1862, 10.

Cohen shared the goal of unifying the methods and purposes of science and philosophy. However, he rejected both the above assumptions of the then contemporary epistemology. As Natorp points out, both logicism and the genetic theory of perception were directly opposed to the critical method of the Marburg School, which began with Cohen and which, Natorp says, has two characteristic elements. The materials present for analysis are "the present and historically provable facts of science, of ethics, of art and religion." The goal is "to prove the foundation for the possibility and thus for the justification of each fact, that is, to work out the lawful foundation, the unity of logos in all creative acts of culture."[50] In what follows, I will show how, in Cohen's early work, he took on the task of giving a secure foundation for a Kantian epistemology. In this chapter, I will present the debates on Kant's epistemology that began with Helmholtz's Kantian interpretation of his own research into the physiology of perception, and reached a zenith with the debate between Trendelenburg and Kuno Fischer.

1.1 Hermann von Helmholtz's sign theory

Helmholtz identifies the two fundamental questions of epistemology as: "*What is true in our sense perceptions and thought?* and *In what way do our ideas correspond to reality?*"[51] In what follows I will sketch Helmholtz's answers to these two questions. I will conclude with a presentation of Helmholtz's empirical theory of sense perception, in which Helmholtz gives what I will call a *genetic* theory of spatial position. A genetic theory is one that explains space, time and the categories as derived from prior experience (as *a posteriori*), and explains that derivation by appeal to the subject's physiology or psychology. My presentation of Helmholtz's epistemology is intended to give the background to the debate between the neo-Kantian Friedrich Albert Lange, and the neo-Aristotelian Friedrich Überweg, over Helmholtz's theory of space. This debate is interesting for our purposes because it prefigures the debate between Adolf Trendelenburg and Kuno Fischer shortly thereafter. The debate between Lange and Überweg was one of the first occasions for philosophical

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50 Natorp 1918, 196. See above.

51 Helmholtz 1971 [1878], 368.

consideration of the epistemological implications of Helmholtz's genetic theory. The question of the *origin* of space, time and the categories, that is, the issue of whether and how one can derive knowledge from them *a priori*, will be a fundamental problem of the Trendelenburg-Fischer debate, and a central issue of Cohen's system overall.

Helmholtz claims that his fundamental questions of epistemology are "common problems" of philosophy and of science, though the two disciplines "attack these questions from opposite directions." While philosophy describes mental relations that are independent from reality, natural science tries to isolate the "laws of reality" from our ideas, that is, from "definitions, systems of symbols, patterns of representation, and hypotheses."[52] Helmholtz's "sign theory" answers the first question, "*What is true in our sense perceptions and thought?*" According to the sign theory, sense perceptions and concepts are signs, not images, of their objects.[53] Helmholtz answers the second question—"*In what way do our ideas correspond to reality?*"—by arguing that "conformity to law is the only condition which something must satisfy in order to be real."[54] The general law of causality is for Helmholtz a transcendental law, an *a priori* condition for constructing any theory that corresponds to reality.[55]

Helmholtz argues that what is *real* in our perceptions and thoughts is the "lawful regularity of phenomena."[56] One can represent this lawful regularity by means of signs, and then interpret the signs as part of a scientific theory. The account of knowledge in Helmholtz's epistemology depends on the claim that the causal relations of the external world can be represented by means of signs. Helmholtz was enough of a Kantian to claim that our sense perceptions are the signs of their objects, rather than direct images of them. He claims that we have access to reality only through representations. He argues, nonetheless, that intuitions and concepts

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52 Helmholtz 1971 [1878], 368.

53 Helmholtz glosses the sign theory as the claim that "our sensations are qualitatively only *signs* [*Zeichen*] of the external objects, and certainly not *images* [*Abbilder*] with any degree of similarity" (Helmholtz 1968 [1869], 56. He argues that the signs that are our sensations need not resemble the objects they symbolize, any more than the words of a natural language must resemble their objects. Rather, we learn through effort to interpret the signs as we would a language.

54 Helmholtz 1971 [1878], 388.

55 *Ibid.*, 389.

56 *Ibid.*, 386.

can represent causal relations directly, and that therefore we have access to real lawful relations between objects:

> I need not go into the fact that it is a *contradictio in abjecto* to try to present the actual (*das Reele*) or Kant's [thing in itself] *Ding an sich* in positive statements without comprehending it in our forms of representation. This fact has been pointed out enough already. What we can attain, however, is knowledge of the lawful order in the realm of reality, since this can actually be presented in the sign system of our sense impressions.[57]

According to Helmholtz's account, spatial and temporal properties are not properties of the objects themselves, but are means of organizing signs. This aspect of Helmholtz's theory merits closer examination in this context, since his theory of space and time will be particularly germane to later neo-Kantian response.

Helmholtz explains spatial properties by means of a genetic account, according to which the spatial properties of our representations are produced by the physiological process of perceiving. These properties are certainly signs of their objects, but they do not refer directly to properties of external objects. In what follows I will outline Helmholtz's physiological account of perception and will sketch his empiricist theory of separation in space.[58]

In an 1869 address Helmholtz rejects the "copy" theory of sensation according to which the sense-organs provide us with "Abbilder," images or likenesses, which directly resemble their objects. His mentor Johannes Müller had observed in research that the same stimulus could produce different responses in each sense organ, and that these responses did not combine with each other to form a single system. Instead, Müller observed, each sense organ has its own mechanism, distinct from the others, that determines the quality of sensations.[59] Müller explained this by arguing that there is a "specific nerve-energy" for each nerve in the body. The ear is sensitive to sound waves of a certain frequency, for instance, and the eye is sensitive to light

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57 Helmholtz 1971 [1878], 388.

58 For detailed and persuasive accounts of Helmholtz's views on perception and epistemology, see Hatfield 1990, DiSalle 1994.

59 Helmholtz [1869] 1968, 56.

waves. The discovery of specific energies raised hopes high for what has been called a "projection" theory, according to which the properties of the objects in the world are communicated directly to the specific nerve configured to receive such signals. As Müller points out, the projection theory fails to persuade, because there are powerful counter-examples. For instance, images on the retina are upside down, and we see only one image even though we have two eyes. Trying to infer directly from the nature of our sense-organs to the real properties of objects, then, does not do a good job of explaining the phenomena.

Helmholtz argues that even spatial position, often used as a criterion to individuate objects, is an interpretation of our sensations, and not their immediate result. Again, stereoscopic vision shows that what may appear, to us, as a single image is in fact two images resolved into one. Perspective can distort size, as when one puts a finger in front of the moon.[60] Helmholtz believes that we learn how to interpret spatial concepts through experience, which means that he has what he calls an empirical theory of spatial perception. This theory coexists, in Helmholtz's epistemology, with his above commitment to the sign theory, according to which spatial properties are properties of representations. Helmholtz's epistemology commits him to the view that representations arise in a physical process, but are merely signs and not copies of their objects. According to Helmholtz's explanation of the physiology of perception, even such relations as separation in space and relative spatial position are not real properties of objects:

> It is easy to see that by moving our fingers over an object, we can learn the sequences in which impressions of it present themselves and that these sequences are unchanging, regardless which finger we use. It is thus that our knowledge of the spatial arrangement of objects is attained. Judgments concerning their size result from observations of the congruence of our hand

[60] Helmholtz was inspired by the theory of Hermann Lotze in his explanation of these phenomena. For Lotze, Helmholtz observes, "to the sensations from spatially distinct nerve endings correspond various determinate *Localzeichen* [literally: place signs], whose spatial meaning is *learned* by us" (Helmholtz [1869] 1968, 57). My various sensations of my finger are originally unrelated, but I can relate them to each other by means of the concept "my finger," which serves as a mental *Localzeichen* that contains the data of all the sensations. Space itself is a very general *Localzeichen* that relates all possible sensations to each other. The usefulness of Lotze's theory is that all psychological sensations are mapped directly onto mental concepts, and even space becomes a tool for constructing an interpretation of sense-data, akin to a language.

with parts or points of an object's surface, or from the congruence of the retina with parts or points of the retinal image. A strange consequence, characteristic of the ideas in the minds of individuals with at least some experience, follows from the fact that the perceived spatial ordering of things originates in the sequences in which the qualities of sensations are presented by our moving sense organs: the objects in the space around us appear to possess the qualities of our sensations. They appear to be red or green, cold or warm, to have an odour or a taste, and so on. Yet these qualities of sensations belong only to our nervous system and do not extend at all into the space around us. Even when we know this, however, the illusion does not cease, for it is the primary and fundamental truth. The illusion is quite simply the sensations which are given to us in spatial order to begin with.[61]

One might reasonably ask the question, why does Helmholtz not consider such concepts as separation in space to be objective? After all, as Helmholtz observes, a well founded judgment about relative spatial position seems to go beyond the mere excitation of nerves. For instance, someone might grasp a pen in his fingers. He cannot infer directly from the sensation of the pen that it is in one place, because each finger feels only the position of the pen relative to the finger itself. He would have exactly the same sensations if his fingers were touching two or three different pens, separated in space. The belief that the pen is in one place only is based on his knowledge that his fingers are close enough together that only one pen will fit between them. As Helmholtz remarks,

> When two different parts of the skin are touched at the same time, two different sensitive nerves are excited, but the local separation between these two nerves is not a sufficient ground for our recognition of the two parts which have been touched as distinct, and for the conception of two different external objects which follows. Indeed, this conception will vary according to circumstances. If we touch the table with two fingers, and feel under each a grain of sand, we suppose that there are two separate grains of sand; but if we place two fingers one against the other, and a grain of sand between

[61] Helmholtz 1971 [1878], 376-7.

them, we may have the same sensations of touch in the same two nerves as before, and yet, under these circumstances, we suppose that there is only a single grain. In this case, our consciousness of the position of the fingers has obviously an influence upon the result at which the mind arrives… What, then, is it which comes to help the anatomical distinction in locality between the different sensitive nerves, and, in cases like those I have mentioned, produces the notion of separation in space?[62]

Helmholtz argues that properties such as separation in space are well-founded inferences from two sources of knowledge: first, our experience, and second, the properties of our sense organs. As he states in the passage above, Helmholtz believes that knowledge of the way our physiology works in perception is essential to any epistemological account of spatial properties. Hence Helmholtz answers the question posed at the end of this passage by giving a foundation for two theories: the above-mentioned "sign theory," according to which our representations are symbols of their objects, and a theory of the physiology of perception.

Helmholtz's empirical account of how we learn to interpret spatial signs coexists with his view that the signs do not refer to objective properties, but only to the signs themselves. That is because, as we saw above, Helmholtz argues that the real in perception is only the "law-like regularity of the phenomena," that is, the way they represent fundamental causal relations. Helmholtz's view is that although signs (representations) are empirical phenomena produced by the properties peculiar to our physiology, nonetheless real causal relations can be represented by means of signs.

For the case of visual perception, Helmholtz argues that the eye functions as a camera obscura. "Camera obscura" means dark room, and originally the meaning was quite literal. Early camerae obscurae were simply enclosed rooms in which a hole had been cut, through which the light waves that the subject (the moon, for instance) emanated would filter. An upside-down image of the object would then be projected onto the wall of the room. Later camerae obscurae were in the shape of boxes and came to be approximately the size of a large modern single-lens camera. The

[62] Helmholtz 1995 [1868], 175-6.

principle was the same: light is filtered through a small hole and projected onto a surface. The camera obscura is a device that alters the objects it represents in predictable ways. For instance, like images on the retina, the images of a camera obscura are upside down. Further, the projection surface (called the "table") of the camera obscura is an artificial "space." We can speak of images being to the left or right of each other in the image, but these positions are not real properties of the objects in any strict sense. Rather, they are properties of the image.

Helmholtz argues that the more we know about the physiology of perception, the more accurate our inferences about our experience will be. In the case of a person grasping a pen or touching a grain of sand, for instance, Helmholtz argues that we become aware that the object touched is a *single* object by studying the position of our sense organs: the nerve endings in our fingers, in this case. Similarly, the insight that the eye functions as a camera obscura allows Helmholtz to give a more complete answer to his first question of epistemology: What is true in our sense perceptions and thought?

In the next section we will see how the response to Helmholtz's epistemology led to an early debate on the Kantian theory of space, between Friedrich Überweg and Cohen's mentor, Friedrich Albert Lange. This debate anticipates the main points of contention of the slightly later debate between Überweg's mentor Adolf Trendelenburg and Kuno Fischer. The most crucial question is whether space and time are subjective or objective properties of our representations, or both.

1.2 A debate on Helmholtz's theory of space

Helmholtz's arguments about the mechanism of perception and his hybrid empiricist and idealist explanation had a deep impact on contemporary views about space and time. Even though Helmholtz maintained an idealist epistemology, his theory had serious consequences for Kantianism. If Helmholtz could explain even separation in space empirically, then the Kantian view of space and time as *a priori*, purely subjective forms of intuition would suffer a blow, and indeed Helmholtz himself rejected this Kantian tenet. Helmholtz argued that he was a Kantian in spirit, but that he retained transcendental idealism only to the extent that he agreed that representations are signs of their objects. A debate in the middle of the eighteenth century between the earliest Marburg neo-Kantian, Friedrich Albert Lange, and Friedrich Überweg, centered on the problem of the consequences of Helmholtz's doctrine of space and time.

Friedrich Überweg was a protégé of Adolf Trendelenburg's, and a student of Friedrich Beneke, another of the founders of the "new discipline" of *Erkenntnistheorie*. Überweg's focus was on the logical relation between particular and general. Überweg's proposed strategy for *Erkenntnistheorie* was to analyze particular results of the sciences, such as physiology and physics, to reveal the general logical relations between them. In the works I will examine, Überweg argues that Helmholtz's results in physiology justify a change in the Kantian epistemology.

Überweg takes up Helmholtz's research eagerly, arguing that Helmholtz had left room for an epistemological argument that space and time are real properties of objects. Überweg argues that the results of Helmholtz and others in physiology and physics justify an inference from the spatial and temporal properties of our representations to the properties of the objects themselves. The inference is not direct—Helmholtz had argued successfully against simply projecting the properties of our sensations onto external objects. However, Überweg thinks that progress in science makes possible a persuasive description of how external events cause our representations—and are even the effective cause of the spatial and temporal organization of our representations:

> Modern physics and physiology, because they trace sound, warmth, and colour back to the perception of vibrations of air and of aether, smell and taste to the perception of certain motions connected with chemical occurrences, prove the dependence of the content of perception on motions, i.e. on changes belonging to the forms of space and time.[63]

Überweg argues that there are good grounds for arguing that our experience of space and time *depends* on the properties of external objects, and hence, that space and time are objective.

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[63] Überweg 1871, §38, 80-81.

Überweg's argument is best described through an adaptation of his own example, which he took from Helmholtz.[64] (This example refers to space, but Überweg's arguments apply equally to time.) Remember the example of the camera obscura from the previous section. Imagine, further, that the camera pictures two objects, one square and the other circular, on its table:

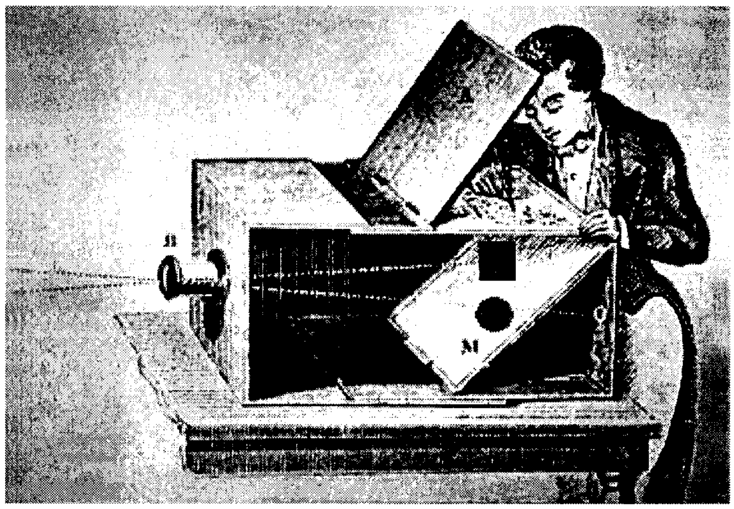

Figure 1, Gernsheim 1969

We can describe the relative spatial position of the two objects as follows: the square is above and to the left of the circle. Now imagine that, in the next moment, the camera reveals an image on the table in which the square and the circle are reversed. What sort of story will we tell about the reversal? Überweg points out that it is implausible to say that changes in the subjective forms of space and time *cause* change of position. Surely the better explanation in this case is that the camera stayed in place, and the objects moved. Überweg argues that the motion of external objects causes changes to the spatial organization on the table of the camera. Hence, according to Überweg, this organization reflects properties of objective events.

[64] In his article "On the Theory of Visual Orientation," Überweg follows Helmholtz in arguing that our perceptions are like the images produced by a camera obscura (Überweg 1858, 275-6). Such a camera projects its images on a table, but cannot represent its own mechanism of perceiving. Its subjective activity of perception can be manifest only in the images themselves or in inferences from their relations to each other. Überweg draws from the camera obscura example that all space is the space of our consciousness and that objects are related to each other in that space. Such a conclusion is fairly evident: if a camera obscura is in essence a machine that diffracts light rays through a pinhole and throws images onto a table, then it cannot perceive images in a space other than that table, and cannot experience objects as standing in some other relation than that in which they are presented on the table.

Überweg argues further that if he is correct, then the Kantian system cannot fulfill the requirements for an *Erkenntnistheorie.* One of the desiderata for an *Erkenntnistheorie,* as I've presented it, is to use logic only to make analytic claims about the evidence uncovered by empirical science, and not to speculate beyond those claims. According to Überweg, Kant appeals to a notion of space that goes well beyond the empirical evidence. In the Transcendental Aesthetic, Kant gives two expositions of the concept of space. The first exposition, which Kant calls the "Metaphysical Exposition," corresponds to Überweg's conception of space as the medium for our representations. It is similar to what might otherwise be called phenomenological space, that is, the space of our experience.[65] The second exposition is the transcendental exposition. As Überweg remarks, here Kant appeals to the possibility of synthetic geometrical proofs *a priori.*[66] The space of the transcendental exposition resembles what may be called topological space. That is to say, the geometrical analysis of topological spatial relations determines global facts about space itself. The transcendental exposition of space is Kant's support for the possibility of such global analysis of space, independently of physical sensations.

According to Überweg's interpretation, Kant uses the transcendental exposition of space to support the argument that space cannot be objective at all, but must be only subjective.[67] Überweg thinks that the empirical facts do not support Kant's inference. For Überweg, an account of the global properties of space in general must begin with the local experience of space as it appears in our representations. Kant could agree that this is where the account should *begin.* But Überweg continues that Kant, in giving space a "transcendental" exposition as only subjective, has divorced his account from the facts on the ground. For Überweg, any account of the global

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[65] I am grateful to Stephen Menn for suggesting the following comparison, between Überweg's account of the metaphysical and transcendental expositions and the phenomenological and topological character of space.

[66] "In der 'transscendentalen Erörterung des Begriffs vom Raum,' unter der Kant die Erklärung desselben als eines Princips, woraus die Möglichkeit anderer synthetischer Erkenntnisse a priori eingesehen werden könne, versteht, führt Kant die Behauptung durch, die Vorstellung des Raumes müsse eine Anschauung a priori sein, wenn es möglich sein solle, dass die Geometrie die Eigenschaften desselben synthetisch und doch a priori bestimme" (Überweg 1896, 281).

[67] "Der Raum gilt demnach Kant als eine Anschauung a priori, die vor aller Wahrnehmung eines Gegenstandes in uns angetroffen werde, und zwar als die formale Beschaffenheit des Gemüthes, von Objecten afficirt zu werden, oder als die Form des äusseren Sinnes überhaupt" (Überweg 1896, 282).

properties of space must be supported by a causal account of how we become aware of those properties. Such an account, he believes, can be given only by the empirical sciences, especially physiology and physics.

Überweg argues that Kant is mistaken to argue that (transcendental) space is only subjective. Überweg contends that Kant's doctrine of space implies that spatial properties are determined in a mental process wholly independent of experience. To see the force of this objection, contrast the camera obscura model of representation with a fortune telling machine at the circus. Such a machine accepts a quarter and prints out a card with the customer's fortune. The quarter is the occasion for the machine to print out a fortune, but that does not justify the customer's thinking that the fortune actually applies to him. In fact, the fortune was produced in an obscure process indifferent to the consumer, somewhere in the bowels of the machine. In contrast, the camera obscura is sensitive to its environment. It reflects, and reveals, the effects of external motions such as changes in light and position. Überweg's position is that the Kantian explanation of how spatial concepts are constructed seems as clumsy and self-referential as the fortune-telling machine. He argues that since physiological explanations of sensation have advanced to become more sensitive to the relation between perception and external events, there is now an opportunity to refine Kant's epistemology as well.

In his *History of Materialism*, Friedrich Albert Lange responds to Überweg's argument.[68] Lange's goal in the *History* as a whole was to support his claim that "[t]he consistently materialistic view...changes round...into a consistently idealistic view."[69] Lange argues that it is "the last refuge of materialism to maintain the order in space and time belong to the things-in-themselves," and consequently he saw Überweg as an exponent of materialism.[70] Lange argues that Überweg's account is adequate within certain bounds, but that it neglects to explain significant facts. These facts can only be explained by means of an idealist argument, Lange argues—hence, as he argues in general, a consistent materialism leads to idealism.

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68 Lange 1881 [1866], hereafter cited as *History*.

69 *History* 223.

70 *History* 224.

Lange argues that Überweg's inference from the particular, local spatial and temporal organization of our representations to the global properties of space and time is much too quick. Lange gives various counterexamples to Überweg's camera obscura example. Überweg imagines an animated camera obscura, which would be able to move around like a human being and represent its environment. (Überweg's animated camera obscura is deliberately reminiscent of Condillac's animated statue.) Lange points out that the camera obscura is a particularly revealing example, because it presents us with an already constructed image. To use his example, it is much easier to see the pattern on a loom when the completed rug is in front of you. A camera obscura presents us with a *fait accompli*, an image, and it is then easy to draw inferences from it, whether justified or not. Another way to conceive of Lange's example is as follows. Imagine that a rug covers miles of ground, and that it is dark outside. The animated camera is given a lamp and a sketch pad, to map the rug's pattern. The camera may discover that the pattern in its area is that of a giant claw. On the basis of the available information, it may infer that the rug has a pattern of many claws. But the true pattern of the rug is of a cat, whose claw the camera represented. The camera's reliance on local results led it astray—or rather, the eagerness to infer from local properties to global ones on the basis of abstraction leads Überweg astray.[71]

Lange's position is that Überweg has privileged the results of physiology and physics over the equally compelling results of pure mathematics, such as topology. Recall that Lange and Überweg are debating the question whether space and time are properties of things in themselves, or more simply, whether they are "really" objective. Überweg responded to Lange's arguments by arguing that three dimensional space is "transcendentally real" in Kant's sense.[72] In fact, the basis for Überweg's argument is found even in Helmholtz's speech "The Facts of Perception," in which Helmholtz argues that we can only sense a space of three or

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[71] My revision of Lange's example here is supported by further examples in his text. For instance, Lange argues that a being that can see only in two dimensions would find it impossible to independently arrive at an idea of stereometry (*History* 226).

[72] *History* 225. Überweg's response is found in Überweg 1871 [1868], §85.

fewer dimensions.[73] Überweg argues that the local argument, that our sensations are restricted to three dimensions, justifies a global conclusion, that there is an independent objective space that has three dimensions. Lange describes Überweg's argument as follows:

> Überweg's proof…rests entirely on the assertion that a mathematical knowledge of objects would not be possible to the extent that it is possible for us (e.g., in astronomy), unless the number of dimensions of the self-existent world agreed with those of the phenomenal world.[74]

Again, Überweg uses the empirical facts of science to back up his reasoning. Lange points out, though, that Überweg's argument involves a *petitio principii*. Überweg appeals to the implicit claim that astronomy is the only means according to which we can make the world mathematically intelligible to ourselves. Lange points out that Überweg has not proven that astronomy is the absolute standard for knowledge of the world. Indeed, Lange observes, we have no such absolute standard.

Überweg might respond that we cannot *conceive* of a space of more than three dimensions. Following his teacher, Hermann Lotze, Überweg might even claim that the notion of a space with four or five dimensions is merely a kind of "logical prank" that violates ordinary experience and consciousness. Lange responds that such arguments are unjustified:

> Ordinary consciousness has no such right as against science; least of all with the mathematicians, who have long been accustomed to attain their most beautiful results by the most paradoxical generalisations. Compare negative, incommensurable, imaginary, and complex quantities, broken and negative exponents, etc.[75]

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73 "Three dimensions are sufficient, however, for all our experience, since a closed surface completely divides space as we know it….And, just as a continuous line can enclose only a surface and not a space—that is, a spatial form of two and not three dimensions—so a surface can enclose only a space of three and not four dimensions" (Helmholtz 1971 [1878], 377). Here Helmholtz is almost certainly arguing we can only have a particular sensation of a surface corresponding to three dimensions, given a particular definition of a "surface"—since Helmholtz knew that we can conceive of a space of four dimensions.

74 *History* 226-7n. Translation of Ernest Thomas corrected by me, as suggested by Alison Laywine.

75 *History* 227n.

Lange argues that Überweg is too optimistic about what can be proven by means of reasoning about the local space of our "ordinary consciousness," or even about the space of a particular empirical science. Claims about the global character of space require a carefully reasoned set of perfectly general mathematical inferences.

The debate between Überweg and Lange prefigures the issues of the Trendelenburg-Fischer debate for two reasons. First, Überweg's proposal to revise Kantian epistemology is a precursor of Trendelenburg's argument about the Transcendental Aesthetic, as will become clear soon. Second, Cohen's objections to Trendelenburg are along the same lines as Lange's. However, Cohen's evaluation of the Trendelenburg-Fischer debate will go well beyond Lange's objection, that Überweg's inferences from particular to general are unfounded. Cohen will argue that Trendelenburg's entire approach to *Erkenntnistheorie* is flawed, and that the Kantian epistemology is in fact better suited to adapt to progress in the empirical sciences.

In particular, the Trendelenburg-Fischer debate will bring up the issue of Kant's philosophy taken as a system. Überweg was sensitive to the question of the role of the Aesthetic in Kant's philosophical project. He remarked, for instance,

> The *Transcendental Aesthetic* has to do, in particular, with *the possibility of pure mathematics*, the *Analytic* with *the possibility of pure natural science*, the *Dialectic* with *the possibility of metaphysics in general*, and the *Doctrine of Method* with *the possibility of metaphysics as a science.*[76]

However, saying that the Aesthetic has to do with the possibility of pure mathematics does not settle the question of whether space and time are objective or subjective. More critically, it does not settle the problem of whether Kant's philosophy is a secure foundation for an *Erkenntnistheorie.* Cohen will focus on these questions in his review of the debate between Trendelenburg and Fischer.

[76] Überweg 1896, 276, my own translation. Paragraph is set off in original text. Emphasis in the original.

1.3 The Trendelenburg-Fischer Debate

The "new discipline" of *Erkenntnistheorie* had its start in the post-Kantian movement. Philosophers such as Friedrich Schleiermacher, Karl Reinhold, and later Friedrich Beneke, saw the Kantian philosophy as the foundation for posing the questions of *Erkenntnistheorie.* In the mid-19th century, a small but historically significant number of philosophers and scientists pursued *Erkenntnistheorie* along Kantian lines. Helmholtz himself professed to be a Kantian, of course, partly under the influence of Eduard Zeller; and Lange based his arguments for idealism in the *History of Materialism* on the Kantian doctrine. In 1865 Otto Liebmann closed each chapter of his *Kant und die Epigonen* with the cry, "Also muß auf Kant zurückgegangen warden" ("Thus we must go back to Kant"). However, by 1869, the year the Trendelenburg-Fischer debate began to gather steam, it had become clear that *Erkenntnistheorie* and the late Kantianism out of which it had grown were not necessarily compatible. Debates such as the dispute between Überweg and Lange revealed deep disagreements about the relationship between Kantian doctrine and empirical science.

Cohen was among the first Kantian philosophers to attack these fundamental questions. Cohen's personal history of philosophical study left him well prepared for battle. In Berlin he had studied with Adolf Trendelenburg, as well as with Emil du Bois-Reymond and Heymann Steinthal.[77] All of these professors were concerned with *Erkenntnistheorie*, though from very different perspectives. Du Bois-Reymond was a materialist who argued, in the so-called "Ignorabimus" lecture, that materialism is the only possible grounds for an *Erkenntnistheorie*, since we have no solid empirical evidence for idealism. In contrast, Steinthal was engaged in a broadly Kantian epistemology of history, as I will discuss in the next chapter. But it was Trendelenburg whose influence on Cohen's early career was to prove decisive.

During the years 1869 and 1870, a heated debate took place between Trendelenburg and the neo-Hegelian professor Kuno Fischer about Kant's arguments in support of transcendental idealism. This debate was the main stimulus for Cohen to consider and state his position on Kantian epistemology and

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[77] Kinkel 1924, 39.

philosophy of science. In 1871, Cohen published an essay, "On the Trendelenburg-Fischer Debate," in Steinthal's journal *Zeitschrift für Völkerpsychologie und Sprachwissenschaft.*[78] It is telling that Cohen begins his essay on the debate by saying that he will not discuss the natural history, so to speak, of the debate itself. For instance, he does not discuss in detail how the debate came about, or the reactions of other philosophers. Instead, Cohen focuses on the debate's impact on Kantian epistemology and philosophy of science.[79] As I will argue, Trendelenburg himself saw his argument as a commentary on the Kantian foundation for *Erkenntnistheorie.* That is why Cohen's essay was "decisive" in the debate, I think. He was able to focus on the central problems in the context of the renewal of the Kantian philosophical system as an *Erkenntnistheorie*, and did not confine himself to discussion of the interpretation of any one article of doctrine.[80]

In what follows I will present Cohen's assessment of the impact of Trendelenburg's objections on Kant's philosophy as an *Erkenntnistheorie.* I will show that consideration of Trendelenburg's objections lead Cohen to consider the more general question of the suitability of Kant's system as an *Erkenntnistheorie* in general. Finally, I will present the result of these considerations, Cohen's characteristic blend of Kantianism and historical philosophy.

The catalyst for the debate was Trendelenburg's argument that Kant had left a gap in his proof that space and time are only subjective "forms of intuition." Trendelenburg argues that we can prove that the spatial properties of our representations are caused by external motions, and thus are objective. Here Trendelenburg's argument very much resembles Überweg's. Trendelenburg and Überweg argue that physiological research into the process of perception yields a scientific justification for the claim that the spatial and temporal properties of our

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[78] Cohen 1928 [1871b]. All translations from this essay are my own.

[79] "Den Ausgangspunkt für die intensiven und epochemachenden Kant-Studien Cohens bildete die Kontroverse zwischen dem Aristoteliker *Adolf Trendelenburg* (1802-1872) und dem Hegelianer *Kuno Fischer* (1824-1907), die sich in den Jahren 1869 und 1870 heftig zugespitzt hatte und in die Cohen selbst—man darf wohl sagen abschließend—eingriff" (Holzhey 1972, 48).

[80] "Der *Sache* nach ging es in der jahrzehntelang geführten Diskussion nicht mehr weiter, wie das Apriori der Erfahrung...bei Kant zu verstehen seien. Die Trendelenburg-Fischer Kontroverse war nur die jüngste Erscheinungsform dieser Stellungsschlacht. Cohen nahm sich nun wohl deshalb der zwischen hoffnungslos erstarrten Fronten eingekämpften Problematik ein, weil er selbst Anlaß dazu hatte, der erkenntnistheoretischen Grundfrage nachzugehen" (Holzhey 1972, 48).

representations are properties of the objects themselves. They conclude that space and time themselves are "objective." However, as Cohen points out, this is a "different concept of objectivity" from the Kantian concept.[81] Trendelenburg appeals to the concept of "exclusive objectivity," a characteristic feature of his argument. In what follows I will present that concept, and then will introduce Cohen's objections to Trendelenburg's account.

The first element of Trendelenburg's revision to the Kantian theory is negative. Trendelenburg argues that Kant has left a "gap" in his argument that space and time are "merely," or "exclusively," subjective forms of intuition. Cohen presents Trendelenburg's argument as follows. First Trendelenburg concedes that Kant proves the "pure" [*rein*] subjectivity of space and time:

> According to Trendelenburg Kant did prove space and time to be *a priori* and therefore *purely* subjective intuitions. The expressions "*a priori*" and "purely subjective" coincide with each other in Trendelenburg. *Both mean that they presuppose no empirical perceptions, no experience.* We should keep this in mind, for it is the crux of the argument [*nervus argumentationis*].[82]

By proving that space and time are *a priori*, Trendelenburg claims, Kant has shown only that they are "purely" subjective, that is, that they do not presuppose experience. Trendelenburg continues, though, to say that Kant was not content with a proof that space and time are "purely" subjective, that is, *a priori*. Instead, Kant wants to show that they are "exclusively" [*bloß*] subjective: that space and time must be determined by the subject alone, or in other words, that space and time are only independent configurations of our faculty of sensibility, and have nothing to do with the objects whatsoever.[83]

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81 Cohen 1928 [1871b], 235.

82 Cohen 1928 [1871b], 233.

83 As Cohen puts it, if space and time are to be exclusively subjective, "Space and time should be not only pure, a priori intuitions because they precede all experience, but are meant generally to be representations grounded only and exclusively in the forms of our sensibility; thus not only do they precede all experience, but they are also only prior modifications of our sensibility" (Cohen 1928 [1871b], 234.

Trendelenburg claims that Kant argues that there is a boundary between space and time, as the subject's forms of intuition, and the objects of experience.[84] For Kant, Trendelenburg continues, external objects do not cause the construction of the spatio-temporal concepts that subjects apply to objects. Rather, for Trendelenburg's Kant, spatial and temporal concepts are constructed in an independent process in the mind of the subject.[85] According to Trendelenburg's interpretation, for Kant the concepts of succession and of simultaneity are only "modifications of our sensibility" *prior* to any experience of objects. This is the fortune-telling machine model of space and time, presented above in my discussion of Überweg.

Similarly, Cohen continues, Trendelenburg says that Kant has ruled out the possibility that space and time would be "purely objective," that is, that they would be derived directly from objects of experience:

> The concept of the *objective* is determined from this conception of the subjective in terms of the *a priori*. The purely objective is that which is grounded only in objects and is acquired through *experience* of objects. When Kant proved that space and time are *a priori* intuitions, he proved their *pure* subjectivity, and thus ruled out a *pure* objectivity according to which they would be acquired from objects through *experience*.[86]

Trendelenburg's opposition between "pure objectivity" and "pure subjectivity" hinges on the difference between *a priori* and *a posteriori* demonstration.

Trendelenburg claims that Kant's argument for *pure* subjectivity leaves open the possibility that the intuition of space and time could be caused by the objects themselves, because external objects would be the catalysts for our representations, and would be the true causes of the events we represent. In that case, Trendelenburg urges, space and time could be considered as both subjective and objective, since

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[84] Since space and time are mere subjective forms that precede representations of objects, Trendelenburg argues, "According to this space and time are something subjective and indeed according to Kant something *only* subjective" (Trendelenburg 1862, 158, my translation).

[85] Since space is necessary to our intuitions but arises independently of our experience, Trendelenburg says, it is (for Kant) "nothing but this subjective form and not a property of the things"(Trendelenburg 1862, 158, my translation). The same goes for time: it is only the form of "the intuition of ourselves and of our inner state and thus the form of all appearance in general" (*Ibid.*).

[86] Cohen 1928 [1871b], 233-34.

objects would be the *source* of representations in space and time. Trendelenburg calls this type of objectivity "exclusive objectivity:"

> This objectivity is not pure, for it is the reason why *a priori* intuition is possible. But it is *exclusive*, that is, it rules out the claim that subjectivity is its *conditio sine qua non*, for it exists even though our sensibility does not encounter it.[87]

Trendelenburg observes that spatial and temporal constructs need not *resemble* their objects directly. It is enough to argue that they refer to external objects, objects that exist independently of our ability to represent them.

Trendelenburg's argument is another extension of Helmholtz's theory of perception. Unlike Überweg, Trendelenburg focuses on the consequences of Helmholtz's sign theory, in which Helmholtz argues that our sensations do not directly resemble their objects. The sign theory alone does not prove that space and time are not objective, Trendelenburg argues. The sign theory proves only that they are not "purely" objective. That is to say, the sign theory demonstrates that the real spatial and temporal properties of objects are not communicated to us directly, via our nerve endings. That does not rule out the claim that space and time are exclusively objective, that they are caused by events that take place completely outside the subject. Space and time could still be subjective reactions to objective stimuli. But if our representations have the spatial and temporal properties they do because they are caused by external events, then Kant's account that "the manifold in space and time" is determined before any experience, *exclusively* by the subject, must be incorrect.

Cohen begins his essay with an examination of the debate between Trendelenburg and Fischer. The question of who won the debate is not relevant here, except insofar as it contributes to Cohen's final argument. Cohen concludes his essay with a partial fulfillment of the promise he made at the very beginning: to show how one's position on the Kantian doctrine of space and time can contribute to epistemology. Cohen sees, correctly, that Trendelenburg's objections to the Transcendental Aesthetic are in fact an attempt to undermine the Kantian foundations of

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[87] Cohen 1928 [1871b], 235.

Erkenntnistheorie, by arguing that Kantian philosophy does not allow for knowledge of the real properties of objects. Cohen responds to Trendelenburg's allegations on two fronts. First, he argues that Trendelenburg does not grasp the role that Kant's Transcendental Aesthetic plays in the Kantian system. Second, Cohen argues that Kant's philosophical system, properly understood, is a secure foundation for epistemology.

Cohen argues that Trendelenburg has shown that Kantianism would not fulfill the purposes of Trendelenburg's *own Erkenntnistheorie*, which explains and justifies our knowledge of objects by reference to the empirical phenomena of psychology and physiology. Cohen responds that this is not the purpose of a Kantian epistemology. While Kant is indeed interested in giving a description of empirical phenomena such as the occurrence of representations in us, he also wants to explain how someone like Newton or Euclid can demonstrate facts about our knowledge from synthetic relations of ideas, by proving geometrical theorems, for instance. Further, Cohen argues that a progressive, *neo*-Kantian epistemology responds to a question that is fundamentally distinct from the basic question of Trendelenburg's *Erkenntnistheorie*. There are two possible questions for epistemology, Cohen observes:

> Does the nature of things rest on the pre-conditions of our mind? Or is it the case that natural laws can and must ground our thinking? The search for the meaning and the value of the Kantian doctrine of space and time can be another way of enquiring into the principles of knowledge.[88]

Cohen distinguishes two different kinds of epistemology from each other. One tries to identify the psychological basis for our knowledge of objects, that is, it tries to find a foundation for our knowledge in the psychological or physical experience of objects, and in the *analytic* "pre-conditions of our mind" that make this experience possible. The other takes the order of explanation to be precisely the other way around: we can become aware of natural laws, such as Newtonian mechanics, through a *synthetic* process of reasoning—and can then enquire into the foundation of that reasoning, and into the grounds for claiming that the argument applies to the

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[88] Cohen 1928 [1871b], 229.

objects of experience. According to Cohen, the first conception of *Erkenntnistheorie* is Trendelenburg's, while the second is Kant's.

Trendelenburg's *Erkenntnistheorie* and Kantian philosophy differ in the phenomena they aim to describe. For Trendelenburg, logic and mathematics are tools to account for the inferences we can draw from our experience. Trendelenburg would not try to explain a fact by appeal to elements of a theory. In contrast, Cohen argues, Kant wants to show how the act of constructing theories *itself* can contribute to knowledge. That is the distinctive element of Kantianism, he observes. Kantian epistemology can, and indeed should, begin its investigation into our knowledge with an analysis of the theories by means of which one demonstrates scientific facts. Unlike Trendelenburg and Überweg, Kant isolates the contribution of reasoning *per se* to our knowledge.

According to Cohen, here is where Trendelenburg's reading of the Transcendental Aesthetic goes badly wrong. Trendelenburg presents his objections to the doctrine of space and time as if the arguments in the Aesthetic were the only support Kant gives to his doctrine of their exclusive subjectivity. But the Transcendental Aesthetic is not the only place where Kant discusses space and time. As Cohen points out in a rejoinder to Kuno Fischer, the Aesthetic is already "heading towards" the Transcendental Logic.[89] Cohen argues that Kant's theory can only be understood properly as a system, which includes the Aesthetic, the Logic, the Doctrine of Method, and the Analytic of Principles. Kant uses the Aesthetic as a prelude to the Analytic and to the System of Principles, in which Kant discusses the possibility of a mathematical natural science. Cohen's take on Kant is that in order to represent a scientific theory, we need not only the principles of construction of *empirical* concepts, but also an explanation of how mathematical proofs are constructed. The point of Kant's analysis of mathematical concepts, for Cohen, is to analyze how they are used to construct a theory. According to Cohen's interpretation, part of the purpose of the Transcendental Aesthetic is to show that some of the scientific facts that need a foundation are facts revealed by geometrical reasoning. These facts need a different sort of foundation than the psychological and

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[89] Cohen 1928 [1871b], 268.

physiological phenomenon of representation. In particular, some of the facts revealed by Newtonian mechanics seem to actively contradict our experience. A significant task of Kantian epistemology is to show, step by step, how one can become aware of and justify these puzzling facts.

Cohen argues that *Erkenntnistheorie* cannot progress if we take it to express or explain only analytic judgments about our physiological representations. The discipline also requires an account of demonstrations, like some geometrical proofs, that may not appeal to the physical event of representation at all. Cohen observes that Trendelenburg's type of *Erkenntnistheorie* uncovers important facts within its own purview, but that the overall project lacks an essential element. The analytic judgments of Trendelenburg's *Erkenntnistheorie* are accurate when applied to the objects of our ordinary experience, but not necessarily as a description of the global (or, for Kant, "transcendental") properties of space and time. The spatial and temporal aspects of our representations may indeed be objective, and yet this judgment may not apply to space in general and time in general.[90]

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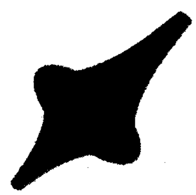
[90] The famous Beltrami pseudo-sphere provides a good example of a case where Cohen would find Trendelenburg's theory lacking. To show that Euclidean space, with no curvature, is not necessarily the space of everyday experience, Beltrami asks that we imagine a pseudo-sphere that looks something like two the sound chambers of two trumpets, placed with their wide ends together. A pseudosphere is generated by a tractrix rotating about its asymptote. A pseudosphere would look something like this (picture from The Geometry Center, Center for the Computation and Visualization of Geometric Structures, University of Minnesota, http:// www.geom.uiuc.edu/):



Beltrami asks us to imagine that we inhabit this sphere, and have no other frame of reference. In such a universe, the space of everyday experience would still appear flat to human beings, because human eyes function like a camera obscura, flattening perspective onto a "table" or image of no curvature. Beltrami points out, further, that within the sphere, there would be no way to test whether space is curved by observation of local phenomena. For example, even if it were possible to hold up a straightedge to some observed segment of space, perhaps the path of a light ray, the test would prove nothing. By hypothesis the straightedge *itself* would be really curved, but our image of it would be straight, and the same would apply to the path of the light ray. As a result, if someone were to compare their representation of a (really) curved straightedge with their observation of a (really) curved ray of light, both would *appear* straight—the light ray would not curve away from the straightedge. No amount of physical investigation of the local properties of space would yield an accurate determination of the global property of hyperbolic curvature. Cohen's position implies that if Trendelenburg were to live on a Beltrami sphere, Trendelenburg would infer that, since all observations and experience of objects depict objects existing in a space of no curvature, the space of the sphere must not be curved. Since Trendelenburg's project is to show how space and time originate

Cohen argues that Kantian epistemology is meant to examine and to explain how we can know about such phenomena as general natural laws and the global, rather than the phenomenological, character of space. This is the second model of epistemology, in which "natural laws can and must ground our thinking." By "natural laws" Cohen does not mean the events themselves as described by the laws, but rather, the scientific way of conceiving the necessary connections between these events. One project of Kant's Transcendental Aesthetic, according to Cohen's interpretation, is to show how we must conceive of space and time if we are to make Newton's laws of nature *comprehensible* to ourselves. That is how a Kantian epistemology can be progressive. A Kantian philosopher can investigate how we are able to demonstrate scientific facts, by observation and by reconstruction of the scientist's reasoning. Cohen observes that "the real criterion of apriority...*in the Critique of Pure Reason* [is that] *the object is drawn from the concepts.*"[91] Cohen thinks that the phenomena in which a Kantian philosopher should be most interested are *theories* such as Newton's laws for mechanics, in which *a priori* proofs are given for objective facts.

Cohen suggests that theories should be the subject matter of epistemology, which indicates that Cohen goes beyond Kant's own doctrine in outlining the plan for a progressive *Erkenntnistheorie.* Cohen argues in closing for a historical, psychological method for the reconstruction of theories. This way of putting it may seem to be at odds with the several pages Cohen has spent arguing *against* psychological accounts. The disconcerting appearance allows for an important clarification of Cohen's view. Cohen does not want to say that psychology *per se* is useless or even dangerous. He argues that we need a "*well founded* psychological method," that is, a method that gives a foundation for the construction of concepts and theories, and not one that limits itself to describing the occurrence of particular sensations.[92] I referred above to the causal account of knowledge, according to which knowledge is founded on psychological or physiological representation, as the "genetic" account. Using Paul

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in our observations, he would have no other way of making or testing conjectures about the space of the sphere overall, but could only fall back on observations of objects on the sphere.

[91] Cohen 1928 [1871b], 245.

[92] Cohen 1928 [1871], 271. Emphasis mine.

Natorp's phrase, one might refer to Cohen's refinement of Kantianism as an "intellectually genetic" epistemology.[93] Finding a foundation for such an intellectually genetic account, which grounds the process of reasoning used in constructing scientific theories, will be a career-long preoccupation for Cohen.

Cohen has reached the conclusion of his rhetorical project in the essay, to defend the progressive use of Kantian epistemology against Trendelenburg's attacks. Cohen's vision for epistemology is not yet fully expressed in the essay, but he makes some remarks suggestive of his future thought:

> What sort of thing is the object to be known? It is a *thought* [*Gedanke*], as such the product of a *rational process*. This process should be represented as an experiment. To this end the thought to be analyzed must be taken apart into its constitutive pieces. First the collected mass of historical facts must be tested to show how, and how extensively, they have influenced the thoughts to be represented or their relation to other thoughts.[94]

Cohen, almost with tongue in cheek, uses the language of empirical research to describe the process of analyzing thoughts. The contrast with Trendelenburg is complete. The "object" of epistemology is not a physical representation, or a physiological process: it is a *thought*, a theory or demonstration, considered as the culmination of a process of reasoning. The "experiments" of this science involve testing the relations between thoughts, namely, the way they fit together to reveal a given scientific fact.

In the publications following the essay on the Trendelenburg-Fischer debate, Cohen will develop a full-blown philosophy of science, in which he elaborates a kind of historical Kantianism. His Kantianism is based on an analysis of how ideas and thoughts come to be manifest in history. Cohen sees one of the main aims of philosophy as to show how science, philosophy and culture are connected, with the ultimate goal of promoting an even more intimate connection between them: "In the final analysis, what does philosophy want to accomplish? It wants to represent the

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93 As Alan Kim has pointed out to me, however, Natorp used the phrase "intellectually genetic" as a derogatory term, and he would not necessarily have used it about Cohen.

94 Cohen 1928 [1871b], 271.

progressive connection of philosophical problems to the whole of human culture."[95] Cohen argues that this ultimate goal for philosophy is best served by means of a philosophical history. Such a history is always conscious that the objects of science, and of knowledge in general, are *thoughts*. Cohen argues that his own conception of "objectivity" is that of the least biased subjectivity, that is, of a subject who can make rigorous and universal judgments. Such a subject can do so only because she is in possession of a set of principles of judgment that are general and, ideally, universal. However, until the problems of philosophy are finally settled, the goal of universality will be elusive. Until that time, perfect objectivity will always be beyond our grasp:

> The path that the historian of philosophy has to take is determined by our conception of relative objectivity. The more he takes a *systematic* view of the texts he represents, the clearer his work will be, in terms of *documentary faithfulness* and in terms of systematic clarity… Precisely because the problems are still in flux, we have usually handled the question by judging what belongs more or less to the philosopher [*who addressed the questions*], what he has thought more or less clearly, which claims from other system inspired him, and what he has added as something new. The more we examine the systematic difficulties, *the more independently we come to grips with the great thinkers*—[*we will find that*] the elements of the analysis will stand out from each other more clearly, the historical development will be more clearly distinguished, and the texts will become less ambiguous.[96]

Cohen distinguishes two approaches to the history of philosophy. The first focuses on what might be called the natural history of the text, in which the philosopher or scientist is considered in his relation to other philosophers or scientists, as an exponent of a particular school, or as representing a particular stage in the development of a way of thinking. The second approach tackles the texts as presentations of a *system*, a system that is constructed on the basis of fundamental principles. Cohen argues that the latter is the proper approach to philosophers such as Kant, and to philosophers of science in general, who present a systematic

[95] Cohen 1928 [1871b], 271.

[96] Cohen 1928 [1871b], 272-3.

conceptual foundation for scientific knowledge. Cohen urges that the history of philosophy, and of philosophy of science, should be considered as a systematic discipline that inquires into thoughts and theories as its subject matter.

Cohen published his essay on the debate between Trendelenburg and Fischer in the *Journal for Völkerpsychologie and Linguistics* (*Zeitschrift für Völkerpsychologie und Sprachwissenschaft*), a journal founded by Moses Lazarus and Heymann Steinthal, who was a mentor of Cohen's in Berlin. Lazarus and Steinthal were influenced less by Helmholtz's work in epistemology than by Johann Friedrich Herbart's attempt to develop a Kantian scientific psychology. Herbart went about this task in a way strikingly analogous to Helmholtz's techniques in epistemology. He argued that individual representations could be evaluated by giving a truly *empirical* psychology, based on such qualities of representations as their frequency and intensity. Lazarus and Steinthal diverged from Herbart's project in a significant way. They aimed to develop a critical method for psychology that went beyond Herbart's analysis of individual representations. As such, Lazarus and Steinthal fit into the tradition of "critical" responses to the genetic methods espoused by Helmholtz and Herbart. In the following chapter I will give a detailed account of Herbart's psychological method and of Lazarus's and Steinthal's responses to it. I will conclude with a sketch of Cohen's version of a "critical" approach to philosophical history.

Chapter Two

Genetic and Critical Methods in History

2.1 *Psychology: Johann Friedrich Herbart*

Hermann Cohen's earliest writings appeared in the *Journal for Völkerpsychologie and Linguistics* (*Zeitschrift für Völkerpsychologie und Sprachwissenschaft*), a journal founded by his mentor, Heymann Steinthal, and Moses Lazarus. Cohen's articles had such names as "A Psychological Interpretation of the Platonic Doctrine of Ideas," "A Psychological Interpretation of Mythological Representations of God and the Soul," and "Poetic Imagination and the Mechanism of Consciousness."[97] Cohen's interest in a psychological approach to philosophy is evident. In what follows I will examine first the theory that inspired Lazarus and Steinthal, that of Johann Friedrich Herbart.

Herbart was one of the founders of modern experimental psychology, along with Gustav Theodor Fechner and Wilhelm Wundt.[98] He was also interested in the relation between theories of psychology and pedagogy, and founded a still-influential pedagogical movement. However his work in philosophy and psychology is what interests us here. Herbart studied with Johann Gottlieb Fichte, and his philosophy and psychology were profoundly influenced from the beginning by Fichte's approach, whether sympathetically or critically.[99]

Herbart set himself the problem of constructing a mathematical rational psychology that would serve as a foundation for experiment. His rational and empirical psychology developed in tandem, as is in evidence in his *Psychology Textbook* (*Lehrbuch der Psychologie*), the first Part of which is on rational and the second Part on empirical psychology.[100] In the *Lehrbuch*, Herbart argues that one can distinguish between the quantitative and the qualitative properties of representations. Quantitative properties include discrete variations in time or space, such as the time at which a representation occurs, or the size of an object represented. It had sometimes been assumed that one can investigate only the quantitative aspects of our

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97 Originally published as: "Die platonische Ideenlehre, psychologisch entwickelt" from 1867; "Mythologische Vorstellungen von Gott und Seele, psychologisch entwickelt" of 1868/9; and "Die dichterische Phantasie und der Mechanismus des Bewußtseins" of 1869.

98 See Boring 1950, 253-4. Also see Ribot 1886 [1879].

99 Maigné 2002, 317.

100 Herbart 1850.

representations using mathematical reasoning. Herbart argued that the variations in "intensity or force [*Kraft*]" of representations are "equivalent to clearness," which is a qualitative attribute.[101] For instance, a sensation of a finger being pressed into the eyelid can vary in intensity, depending on how hard the finger is pressed. It is easy to see that this sensation varies with respect to the intensity of its stimulus. Since these attributes can be represented as a kind of variation, Herbart argued, the qualitative properties of our representations can be evaluated mathematically. For instance, the relationship between the force of pressure on the eyelid and the intensity of sensation could be graphed as a set of values that have a determinate mathematical relationship to each other.

According to Herbart, we can construct the equivalent of a mechanical system for representation that treats psychological phenomena as objective *events*. The mathematical foundations of this will be a "mental statics (the mathematics of qualitatively separate ideas varying in intensity) and a mental dynamics (the mathematics of ideas varying in time and intensity)."[102] Herbart treated representations as objects in the world equivalent to physical forces and thought that they could be given a similar mathematical analysis. Herbart's view is a kind of realism about representations. He argues that representations are as real as physical phenomena:

> It is alleged...that mathematics treat only of quantities, whereas actions and states of greatly different qualities of are the subject of psychology. In order to refute this argument scientifically, I should proceed to show from metaphysics that the real, true and original qualities of simple elements are completely concealed from us, and that they, therefore, cannot be the object of any investigation whatsoever; moreover, that where we believe that we apprehend a difference of qualities in common experience, the cause thereof is very often a difference of quantities merely, as, e.g., when we believe that we hear the qualitative difference of sounds and of their great number of harmonies and disharmonies, while in fact strings of different length are

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[101] See, e.g., Boring 1950, 255.

[102] Wozniak 1999.

> vibrating faster or slower....I do not care to prove in this place, the proposition that no variety of original faculties is co-existing in the soul: the prejudice of a multiplicity of different quantities inhering in the same identical substance may pass unmolested, though it belongs to the first conditions of true knowledge to be free from that. It may be sufficient to assert that however great the number of fictitious qualities which a man may distinguish in his soul, he certainly cannot deny that over and above them there is an infinite variety of quantities determining mental action. Our thoughts are stronger or weaker, more or less clear; their coming and going is faster or slower; their number at every moment greater or smaller, our susceptibility for perceptions, our excitability for emotions and passions is variable to a greater or less extent. These and innumerable other differences of quantity which obviously occur in mental states, have been reckoned among the less essential modifications, but unjustly, and this is the true reason why the lawful consistency of mental phenomena have not been discovered.[103]

This difficult passage contains a concise and compelling argument for Herbart's main contention: that it is possible to determine a set of laws for the variation of mental phenomena.[104]

Herbart argues that the lack of a complete mathematical description of representations should not deter psychology from making mathematical hypotheses. After all, he points out, mathematical reasoning proceeds in other sciences, such as physics, by making and testing more and more general hypotheses. Herbart argues that psychology can follow the lead of those sciences with a mathematical foundation, by using the same method:

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103 Herbart 1877, 255.

104 Like Helmholtz, Herbart was not interested in arguing against one tenet of Kantian transcendental idealism, namely, the distinction between phenomena and noumena. Herbart could accept the claim that we do not have direct representations of things in themselves. As he said in the citation above, "the real, true and original qualities of simple elements are completely concealed from us, and that they, therefore, cannot be the object of any investigation whatsoever." From context it is clear that this claim goes for *any* simple elements. However, Kant had argued that a mathematical psychology is impossible, and Herbart disagreed with Kant about that.

> Any and every law of quantitative ratios, either hypothetically assumed or even known to be false, admits of the substitution of numbers, and we have necessarily to try hypothetical explanations of recondite but important matters, and we have, but careful calculation, to ascertain what consequences would result therefrom, before we can find which of the different hypothetical laws agrees with experience. The older astronomers tried eccentric circles. Kepler tried the ellipsis to rhyme with it the motions of the planets; he then had to compare first the squares of their annual motions with the cubes of their mean distances, before he could find that their relation was identical with regard to all. Newton had, in like manner, to try whether a force of gravitation inversely as the square of the distance would be sufficient to keep the moon in its course around the earth; and if this supposition had not been satisfactory he would have tried another power, say the cube or fourth or fifth power, and computed the results and compared them with the data of observation. This indeed is the very great benefit of a mathematical hypothesis, that we may survey the possibilities within the limits of which the facts must be found, long before we are in possession of sufficiently definite experience, and that we are thus enabled to seize upon very imperfect hints to get rid of, at least, the grosser mistakes.[105]

Herbart sees the mathematical method of proposing quantitative ratios to describe mental phenomena as the way forward for psychology to become a science. He takes the model of a science quite self-consciously to be astronomy and physics. In trying to give a mathematical method he sees his models as, first, Socrates. Herbart imagines what Socrates' observations of the 19th century would be, and of course thinks that Socrates would approve of the great advances in mathematics. However, Herbart says, Socrates would also ask: "Tell me, ye exceedingly wise men, what is better, soul or matter? What is more important for you to know, the inclination of the axis of the earth or the causes of the vibration of your opinions and dispositions?"[106] In the task of giving a mathematical *psychology*, Herbart sees the

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105 Herbart 1877, 253-4.

106 Herbart 1877, 252.

"germ" of his method in the "school of Fichte," whose student he had been. The basic idea of this Fichteanism, which Herbart shared with Helmholtz, was of demonstrating the relation between the perceiving self and the phenomena encountered in experience.

2.2 *Völkerpsychologie, a Herbartian school*

Herbart's influence came to Cohen through his friend and mentor, Heymann Steinthal, who was a founder of a movement called "Völkerpsychologie." The Völkerpsychologie movement was a broadly Herbartian school that nonetheless was critical of Herbart's representational psychology. Its founders, Moritz Lazarus[107] and Heymann Steinthal, rejected Herbart's view that representations are themselves

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[107] There is a more than superficial similarity to the life stories of Moses (Moritz) Lazarus, a founder of Völkerpsychologie, and Hermann Cohen. Both Jews from eastern Germany whose fathers had learned positions in their respective communities (Levin Lazarus was a Talmud scholar, Gerson Cohen a cantor and yeschiva teacher), they became prominent academics who, though this is forgotten sometimes, *both* supported the Prussian state. The following biographical information is all from Belke 1971, Introduction: Lazarus was born 15 September 1824 in Filehne, in Posen, in what was then Prussia. His father, Levin Lazarus, was a Talmud scholar who had studied with the most famous German Talmudist of his time, R. Akiba Eger. (Lazarus, like Cohen, had a fundamental and profound early education in the Talmud, and would write to his family in Hebrew until the end of his life.) Filehne was a diverse religious community: its population of 3,000 was divided evenly, one-third each of Catholics, Protestants and Jews. Lazarus would say later that his early experience of linguistic and religious diversity kindled an interest in comparative anthropology: "Da nun auch die drei Religionsgemeinden des Ortes, die fast zu gleichen Zahlen nebeneinander wohnten, verschiedener Abstammung waren, verschiedene Sprachen redeten, so mag jenes einsame Sinnen und Suchen, das alltäglich durch offenbare Erscheinungen aufgeregt wurde, die tiefste Wurzel der Völkerpsychologie geworden sein, wie die fortgesetzte Beobachtung des verschiedenen Auftretens der drei Bevölkerungsgruppen in fast allen Lebensäußerungen die persönlichen Anfänge einer vergleichenden Psychologie enthalten hat" (Belke 1971, XV; cited from Lazarus' autobiography *Aus meiner Jugend*, p. 33.) The cosmopolitanism of Lazarus' Prussian town was uneasy. Posen was acquired from Poland by Prussia in the partitions (between Prussia, Austria and Russia) of 1792-5. The province became more and more "Germanized" over time, and was so when Lazarus lived there. However, it was annexed back to Poland in the first World War. Most of the Prussians who had lived there emigrated to Germany. What we can note for now is that Lazarus' early interest in history and in comparative anthropology came from his own experience, though it would be systematized later through painstaking and extensive study. At the age of 19, Lazarus was able (only just) to attend a Gymnasium (Martino-Katharineum) in Braunschweig (1844-46). This was his introduction to mainstream culture. Here he learned Latin and Greek, and had his first acquaintance with Herbart's philosophy through Friedrich Konrad Griepenkerl (1782-1849), who had himself been a student of Herbart's. In 1846 he went to Berlin to study at the university (Belke 1987, XIII). Throughout his career as a student Lazarus lived in poverty, freezing and sometimes going hungry. However, he managed to study with some of the best professors in Berlin. In particular, he studied with the Hegelian psychologist Beneke, with Trendelenburg, and less frequently with more Hegelians: Georg Andreas Gabler and Karl Ludwig Michelet. In his own time he read the psychologists Drobisch and Herbart. The empirical signs from Lazarus' early career show a preponderance of psychological study, of course, especially in the systems of Beneke and Drobisch, but also a concern with Hegel and with Herbart.

objective or real, and argued that only communal structures such as languages or theories are real phenomena. Lazarus and Steinthal developed an account of cognition as an historical process, which saw individual psychology and representations as determined by historical and cultural events and facts.

In 1860 the first edition of the *Zeitschrift für Völkerpsychologie und Sprachwissenschaft* was published. The first article was a manifesto written by Lazarus and Steinthal, bearing the title "Introductory Thoughts on Völkerpsychologie, as an Invitation to a Journal for Völkerpsychologie and Linguistic Science."[108] The text sets out the program of the school and lays out a common set of assumptions and goals for its disparate researchers. The common assumption was that each cultural artifact or historical event is the result of prior thought, not of material causes alone. The aim of each article in the journal should be to show the relation between the phenomenon under investigation and the psychological or linguistic process of construction that lay behind it. The goal is as follows: "to investigate the historical life of peoples, in all its manifold aspects, in such a way as to account for the discovered facts from the innermost part of the soul [*Geist*], and thus to try to trace the facts back to their psychological roots."[109] The *Zeitschrift* went on to publish essays from a range of disciplines. For instance, the first edition contains an article on Italian folk poetry by the novelist Paul Heyse, and an essay by Steinthal on idealism in linguistics. Insofar as the *Zeitschrift* has a common subject matter, it is in the careful attention to poetry, myth, peoples, languages, and institutions. The journal is not confined to "comparative" studies, either. Most of the essays are investigations of "empirical" phenomena (Italian poetry, coal workers in England) or of very general topics (a "Characteristic" of languages, "Über Nationalität").

Lazarus' and Steinthal's revision to Herbartian theory begins with a critique of individual representational psychology. They did acknowledge that Herbart himself realized that, after all, "Psychology will always be one-sided if it continues to regard

[108] "Einleitende Gedanken über Völkerpsychologie, als Einladung zu einer Zeitschrift für Völkerpsychologie und Sprachwissenschaft," in Lazarus and Steinthal 1860, 1-73.

[109] Lazarus and Steinthal 1860, 1

people as standing alone."[110] Herbart had limited his criticism of individual representational psychology to proposing a general rational psychology, which accounts for individual psychological phenomena as manifestations of the human capacity for representation. Herbart's revisions still considered individual representations only in their relation to the individual mind, however. Lazarus and Steinthal pursue this line of argument even further. They propose the following thesis of radical reciprocity between individual mind and communal structures, that is to say, historical and cultural facts and events: "The entire spiritual evolution [*of the individual*] is predetermined from his birth onward… not only his knowledge [*Wissen*], but also his conscience [*Gewissen*], his sensation and his willing, his acting and his enjoyment, his perception and thus also his accomplishment."[111] Völkerpsychologie can incorporate all manner of "historical events" [*geschichtliche Erscheinungen*], which are the source material for analysis. Analysis of these events should treat individual and cultural psychology as reciprocally determining. For example, individuals create poetry, but poetry is also an expression of the collective imagination insofar as it expresses the borders of the collective intellectual potential of a people. It is at the same time a material expression of the individual's synthesis of these elements of intellectual life.

Lazarus and Steinthal thought that ideas, taken as independent of history, and history reciprocally determine each other. One should study what is given in history (*real* facts and events) to have access to real content, but that content is partly a result of reasoning. Lazarus's and Steinthal's critique of Herbart is on two fronts. First that mathematical reasoning, or reasoning in general, can be productive: ideas can and do influence the course of events.[112] Second, the source material for psychology is to be found in history and not in individual psychology. This latter commitment was actually Herbart's own idea, but Lazarus and Steinthal take it much further.

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110 Herbart 1834, §240. As cited in the bibliography, Herbart's *Lehrbuch* went through several editions. Lazarus and Steinthal cite the 1834 edition here (in their text as " 2. Aufl."). Otherwise in the paper I cite from a reprint of the third edition, edited by Hartenstein, published in 1850. This is my translation.

111 Lazarus and Steinthal 1860, 4.

112 Here we should note that an "idea" is not taken here in the sense of a clear image in the mind as opposed to a sense-impression. As with Herbart, the mathematical method is the model here.

Herbartianism was not as it stood a system that could provide a foundation for Völkerpsychologie. Lazarus and Steinthal wanted it to be possible for an idea or concept to be created in the mind of an individual that would nonetheless have a broad and unforeseen impact on social reality:

> Wherever the power of great, general ideas spreads over many peoples, where *a single* thought seizes and dominates the genius of many nations, there psychological research will be directed not only to the behavior of the spirit of the people [*Volksgeist*], but to the nature and the law of those societies, which goes beyond them.[113]

Lazarus and Steinthal wanted to account for the fact that certain ideas, paradigm cases of which are mathematical or poetic ideas, have a broader application and effect than the determination of a representation. They wanted psychology to account not only for the phenomenon of individual representation, but also for the fact that the individual's ideas are a synthesis of facts about the world. Hence Völkerpsychologie could not be content with a narrowly Herbartian view, as Lazarus observed:

> On various occasions, even I have called myself a *Herbartian* by preference. I have done so substantially because I regard psychology as the core and center of all philosophical work, and because Herbart was the founder of truly scientific psychology. Nevertheless, I think that if an edifice of psychology is to be built properly, none of its fundamental pillars can remain as Herbart has established them.[114]

Lazarus and Steinthal turned Herbart's analysis of representational psychology into an immanent critique of psychological *action* in history. Individual representational psychology is no longer the foundation of our description of knowledge. Instead, Lazarus and Steinthal develop a model of cognition according to which the principles of knowledge are built from the reciprocal interaction between

113 Lazarus and Steinthal 1860, 6.

114 From Lazarus' *Vorlesungen über Psychologie* 1875/6; cited in Belke 1971.

particular and general, an example of which is the relation between a person and a "people" (*Volk*).

In Lazarus' 1863 Rektoratsrede, *On Ideas in History*, he explains how we can give an account of ideas as productive (*schöpferisch*) and effective (*wirksam*) in history without succumbing to Hegelian dialectic or to Humboldtian[115] empiricism.[116] Lazarus' idea is that philosophical reconstruction of history can reveal the ideal content of events, institutions and artifacts, and that these can be put into an ideal form, an outline or sketch. We do so by first isolating the relevant data and then by analyzing these data in terms of how they manifest fundamental psychological relations. One of Lazarus' first moves is to demonstrate how he has applied the empiricist criticisms of Herbart to the Hegelian and Humboldtian philosophies of history. He does so in a footnote worth citing:

> It should arrest the critic's attention compellingly that the great force of ideas is equally strongly emphasized in two such fundamentally different points of view as Hegel's and Humboldt's. Certainly one of the most important ways that ideas are determined is in relationship to acting and productive people, to the individuals that appear to have them. However, whereas in Hegel conscious or unconscious generality comes into the foreground, with Humboldt [*it is*] personal individuality. For the former [*Hegel*], the individual is only a medium... for the latter [*Humboldt*] the individual is the higher expression, the true life of the idea; for the former the expression: "we do not have ideas, but they have us" is common; for the latter the doctrine is that only in the productive personality do ideas attain a productive existence.[117]

Lazarus wants to identify two "violent" misuses of ideas: the first is Hegel's theory that the individual person is only a vessel for the perfect, absolute Idea. The second

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[115] Wilhelm von Humboldt (1767-1835), a humanist scholar known for, among other subjects, his contributions to linguistics and to aesthetic theory. He founded the Humboldt University in Berlin (Uecker 1990). The foundation of Steinthal's linguistic study was in Humboldt's linguistic theory (Lassahn 1995).

[116] *Uber die Ideen in der Geschichte*, cited as Lazarus 1865.

[117] Lazarus 1865, 41n.

is Humboldt's view that the activity of the person is a necessary and sufficient condition to achieve the being (*Dasein*) of the Ideas. Lazarus gives a long list of characteristics of each *type* of theory.

Lazarus conceived of the effectiveness of reasoning as coming neither directly from the process of thought in the individual, nor from the analysis of concepts. Rather, the impact of reasoning (of "the Ideas") is found in their influence on the relation between individual and general structures, where "general structures" can mean culture and ethnicities, but also theories and poetry. If we want to analyze reason itself, then, we should define the relation between individual and thought in a way that captures the *creative* power of the individual's synthesis of the general. The paradigm case of this sort of reasoning is the use of mathematics to evaluate natural science.

Lazarus proposes to revise Herbart's theory in his account of the relation between individual representational psychology and community structures. Lazarus locates the contribution of reason to history in the influence of ideas on individual thought and action:

> Ideas in history are the ideas that are *effective* in the lives and activities of men, that is, of individuals and peoples, and thus in the life of humanity. They are not transcendental powers found outside the human soul, which somehow affect it from outside, but are actual [*wirkliche*] ideas, that is, ideas that appear within people as acts of their mental agency. They are produced, shaped, and developed within the human soul, and are partly realized in action and productivity.[118]

The only materials available to the psychologist for analysis are the empirical data of experience and of recorded history. Though he insists on the productive (*schöpferisch*) aspect of reason, Lazarus continued to argue that this aspect could be analyzed only in its empirical manifestation. In particular, ideas in *history* are the ideal forms[119] of the phenomena:

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118 Lazarus 1865, 73.

119 Here meaning just *Gestaltungen*, not the Eternal Forms!

> The content of these ideas consists in all the norms of the will, in the criteria for action that keep the natural impulses of human life within certain bounds, describe goals and ends for it, and give form to individual and common human life… Thus structuring ideas [*Ideen der Gestaltung*] are the true ideas in history.[120]

How will we find a way to collect and to analyze that data that will reveal its ideal content? Lazarus argues in *On Ideas in History* that recorded data and physical and cultural artifacts already possess ideal content as artifacts of human effort. That does not mean that the data is already organized in such a way as to reveal ideal patterns or to answer, say, ethical questions. Our goal in evaluating the assembled data, then, will be to treat it at first as a bare assemblage of raw material – Lazarus' example is of a mosaic – and then to arrange it into a bridge, to the past and to the thought or idea we are trying to reconstruct.

Lazarus and Steinthal were able to parlay a re-interpretation of Herbart's insight, that *individual* psychology alone was not a good basis of epistemology or even of psychology itself, into a theory that evaluates the psychological basis of history and theory according to a mathematical method.[121] In so doing, they relocated the search for the principles of knowledge from a description of the subject's drives or dispositions to an immanent critique of reasoning in general. I am interested in Cohen's reception of Lazarus' and Steinthal's work insofar as it presents a certain

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[120] Lazarus 1865, 73-5.

[121] The clearest examples are from Lazarus' and Steinthal's analysis of the *poetic* impulse. Given his fundamental interests in psychology and history, it is surprising that Lazarus' dissertation was called *De educatione aesthetica*, clearly a reference to Schiller's *Letters on the Aesthetic Education of Man* (turned in to the philosophy faculty at Halle, 30 November 1849). His question was: "Was haben die einzelnen Völker originell zur Entwicklung des Schönen in der Menschheit beigetragen?" (Belke 1987, XXII).[121] While we are still not going to draw any hasty conclusions, I should point out a connection here between Lazarus and the later Marburg school. Schiller's *Letters* and his philosophical poetry were fundamental to the work of that school, which connection is itself worth study. Friedrich Überweg, a neo-Aristotelian follower of Trendelenburg who will become very important later, wrote a book called *Schiller als Historiker und Philosoph*. Rudolf Lotze wrote *Über den Begriff der Schönheit*. Friedrich Albert Lange wrote an *Einleitung und Kommentar zu Schillers Philosophischen Gedichten*. Cohen wrote a lengthy essay *Die dichterische Phantasie und der Mechanismus des Bewußtseins* for the *Zeitschrift für Völkerpsychologie und Sprachwissenschaft*. Of course these connections do not show any causal links between Lazarus and any of these philosophers; the German national pride in Schiller was (and is) considerable and intellectual work on him abounds. However, the idea that poetry could be analyzed as a fundamental *Triebveder* of humanity, that poetry itself reveals important *facts* about the psychology but also about the imagination and *Schöpfungstrieb* of the *people*, as well as of the individual, that created it: these ideas are at least a link between Lazarus and the early Cohen, if not a causal link.

model of cognition and of its effectiveness in history. [122] Two interlinked concepts are the backbone of the Völkerpsychologie model of cognition. Ideas, in their manifestation in individual psychology, are productive: of history, of theories, and of human culture in general. The proper method in epistemology is to give a model of cognition according to which individual synthesis is productive of cultural and historical facts (the most significant of which is the division of humanity into peoples). These facts should then analyzed as products of a rational process, as well as a material one.

Cohen's position on the Trendelenburg-Fischer debate is based on Lazarus's and Steinthal's method for historical research. Lazarus and Trendelenburg use the example of a mosaic to describe the method of assembling facts relevant to the investigation of a problem. But Lazarus and Steinthal argue that the carefully assembled mosaic must be arranged into a pattern that reflects the thought process of the person or persons who conceived the theory or system. In the following section, I will introduce Cohen's own historical method for philosophy, which is influenced by Lazarus's and Steinthal's methods.

2.3 *Cohen's way forward: Kant and historical method*

Cohen's approach to the history of science is to identify those mathematical relations embedded in a theory that determine a domain of objects or facts, and to analyze how the theory fits into the evolving structure of scientific explanation. A philosophical history of science will evaluate a single theory by identifying the mathematical relations embedded within it. To evaluate more than one theory, especially in the case of a conflict, Cohen argues that we should adopt an analysis by

[122] Lazarus' theory of history was unsatisfying to Cohen ultimately, although for reasons not found in my presentation. Lazarus had an almost teleological view of history, manifest in his essay "Verdichtung des Denkens in der Geschichte" (Lazarus und Steinthal 1862, 54-62). This view assumed not only that the employment of the ideas *could be* progressive but that history itself could be described as making continual progress in realizing ideals. Cohen did not adopt this way of looking at things. However, Cohen did seem to approve of Lazarus' and Steinthal' *general* analysis of the employment of the Ideas, and in particular of the analysis of cognition as a process immanent in historical facts. For a closer discussion of Cohen's reception of Lazarus' theory of history and of this essay in particular, see Köhnke 2001.

means of philosophical principles that can assess how the theory contributes to our progressive promotion of the methodological and explanatory goals of the sciences.

Cohen considered the history of science and of philosophy to be crucial to any intellectual endeavor, as only by doing sound history can we evaluate the systematic worth of a theory and develop reliable criteria for successive refinements to it. Cohen's views on this subject were forged by his early alliance with *Völkerpsychologie*. Lazarus and Steinthal argued that cognitive content is revealed in cultural structures including institutions (such as universities and governments) and artifacts (such as science, languages, literatures and music). They developed a theory of intellectual history according to which cultural structures can be evaluated using rational criteria, since they are produced by reasoning. However, Lazarus and Steinthal were concerned as well with the question of how to divorce historical analysis from the study of the psychology of the individual subject. Individual psychology constrains conceptual analysis to a description of psychological processes, which Cohen derisively called the "Vorgang und Apparat" or "process and faculty" picture of cognition. Locating intellectual history in an analysis of collective cultural structures such as language allowed for evaluating the impact of ideas in a broader context than individual psychological processes. Adopting a similar approach to the history and philosophy of science allowed Cohen to distance himself from Herbart's psychology and Trendelenburg's *Erkenntnistheorie.*

Cohen picks up on Lazarus' and Steinthal's approach to history as it is manifest in ideal structures. Steinthal taught Cohen that the Kantian *a priori* could be interpreted as a set of principles implicit in actual cultural structures. Our specification of these principles depends, then, on the form of a manifest structure: a language, a building, or even a theory. The clearest example of such cognition is a physical artifact such as a building. To show how Cohen's early historical philosophy works, I will give a concrete example of a cultural artifact and evaluate it using Cohen's methods. The Duomo in Florence is capped off by Filippo Brunelleschi's huge cupola. The cupola is 45.5 meters in diameter and is constructed entirely without reinforcement. While Brunelleschi constructed a model of the cupola, and there are plans and notes executed in his hand, there is still an element of intellectual surprise that he was able to build the cupola at all given the engineering challenge it presents. The cupola presents us with a puzzle for intellectual history – in fact, a puzzle that remains to some degree unsolved, though there are several hypotheses.[123]

How would Cohen solve the puzzle? Cohen's historical reconstruction is based on taking an object (a scientific fact) as the result of a cognitive process. To review his account,

> What sort of thing is the object to be known? It is a *thought* [*Gedanke*], as such the product of a *rational process*. We should represent the process as an experiment. To this end the thought to be analyzed must be taken apart into its constitutive pieces. Thus the collected mass of historical facts must be tested in this way first, to evaluate how they have influenced the thought to be represented or its relation to other thoughts.[124]

We know that Brunelleschi solved the puzzle, because we are presented with an artifact: the cupola itself. The cupola is a fact, a necessarily *single* solution to the engineering problem. In order to solve the puzzle of how Brunelleschi designed the dome, we would need to amass all the necessary historical and formal data: so, for instance, the drawings he left behind and our knowledge of Renaissance science. We will need the records and information about the materials and the site as well. However, to solve the problem all the information must be organized according to a single principle: our reconstruction of Brunelleschi's reasoning must lead to a single solution, the solution that would explain how the cupola in Florence was constructed. It is certainly possible for an historical researcher to construct a second solution that would solve the engineering problem in another way. That solution would not resolve the problem in giving an intellectual history, though, which for Cohen is the problem of showing how ideas such as Brunelleschi's are made manifest in history.

Cohen's history and philosophy of science is an extension of his early method of historical analysis to scientific theories. Here, though, there are obstacles. The first is that two conflicting theories can explain the same fact. As we will see in Part Two,

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123 For instance, Ross King argues in that it was possible partly because the dome uses a herringbone pattern of tiles (King 2001). This account has not settled the question, though.

124 Cohen 1928 [1871b], 271.

Cohen solves this problem by arguing that theories differ according to the *hypotheses* implicit in their structure. For Cohen, the fact that a physicist chose to turn certain propositions into *axioms* of a theory is a fact that must be taken into account when evaluating that theory.

The second obstacle to extending Cohen's approach to conflicting physical theories is that he considers himself to be giving a neo-Kantian theory of science, but he does not restrict himself to a Kantian foundation for science. Cohen's philosophy of science is irreducibly historical in character. In particular, Cohen felt that a truly philosophical history of science would contribute to the conceptual foundations for the sciences. In Part Two, I will introduce Cohen's arguments for this claim in the context of his work on the foundations of the calculus and the foundations of mechanics.

Cohen was almost unique among philosophers in that he took an historical approach to the question of the foundations of a Kantian physical theory, investigating the historical antecedents of Kant's own approach to the problems of physics. Here Cohen finds the seeds of a new approach, one that corrects an historical error of Kant's and allows us to construct a viable systematic philosophy of physics.[125] Kant's Transcendental Aesthetic limits that which can be presented to us in a possible experience to that which can be *given* in a possible intuition. Thus for Kant that which can be given is limited by our "pure intuitions" of the *a priori* forms of sensibility, space and time.[126] Cohen distances himself from this interpretation of the Transcendental Aesthetic. In particular Cohen argues that Kant's distinction between that which can be intuited and that which can be understood, which is founded on Kant's distinction between sensibility and the understanding as faculties, is false. For Cohen, Kant's requirement that any concept of the understanding be demonstrable in "intuition" to yield knowledge of objects is no longer necessary. Rather, Cohen argues that the pure relations between ideas of the understanding that

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125 It is beyond the scope of this section to go into detail about Cohen's Kant interpretation. The core of Cohen's argument is that Kant's embrace of Cartesian physical geometry, which assumes that extension is substantial, rather than the Leibnizian method of determining pure mathematical relations in advance, held Kant back from developing a truly dynamic foundation for physics.

126 Here I am following Cohen's own early interpretation of space and time as forms of *sensibility*, which is arguably not fair to Kant.

Kant characterizes as analytic are in fact *synthetic* relations that determine that which can be "given."

Thus we arrive at Cohen's claim that we cannot apprehend physical scientific facts without some foregoing rational structure, or as he calls it a "productive synthesis" *a priori*. Like Herbart, Cohen takes Johannes Kepler's theory of elliptical planetary orbits as an example of a scientific theory that contains an *a priori* productive synthesis. Kepler was able to give a single, internally consistent, unified cosmology using elliptical orbits. Without that cosmology, we would never be able to see an "elliptical orbit" as an *object*, as a fact about the world. We could make exactly the same observations of the night sky that would confirm our "elliptical orbit" hypothesis. But if we didn't have the hypothesis in the first place, which posits the right kind of law-like relation between the theory and the phenomena, then we would never be able to identify elliptical orbits as scientific *facts*.

Cohen sees each theory as a set of solutions to intellectual problems. A solution will result in cognition of a fact. Again, Kepler's orbits are a good example. Kepler's theory specifies an answer to the problem of describing the planetary orbits, and it specifies at the same time the conditions under which we can observe or confirm the fact that the orbits are elliptical. Cohen argues that when we reconstruct Kepler's theory as we did Brunelleschi's cupola plans, we will see that Kepler's reasoning was synthetic. Of course, Kepler did not call elliptical orbits into being. But what *is* an ellipse? It is an ideal geometrical form – ultimately, an idea. Kepler's achievement was to construct a set of relations between that idea and the mechanical estimates of planetary motions so he could prove that the idea of an ellipse gives a law-like estimate of the motion of the planets around the sun. After all, what else would an "elliptical orbit" be? We surely don't want to insist that the concept of an ellipse is substantial and is made manifest many times in the orbits of the planets. Rather, we can say that the constructible geometrical shape "ellipse" serves as a law or rule according to which we may interpret or measure our celestial observations. As a result, the ellipse gives us a principle of order *a priori* according to which we may measure or estimate the mechanical forces of the planetary orbits.

Cohen argues that all physical laws are backed up by our grasp of mathematical relations. At a most basic level, the real numbers are constructed from the continuum by the application of the Peano axioms. All such reasoning must be synthetic for Cohen: the real numbers are constructed, we give the law of orbits. We construct the relations that allow us to unify the phenomena because nothing is given already structured. The planets do not appear to us as they appear on maps of the solar system, with colored lines describing their orbits. Rather we measure continuous forces and motion by first specifying the conditions to bring them into a unified picture.

At the level of evaluating a particular theory, Cohen argues that we can simply investigate how the mathematical constructs embedded in the theory correspond with physical reality. But Cohen is also trying to develop a history of science, and he wants to be able to achieve a meta-theoretical perspective. For Cohen analysis of theories in terms of philosophical principles determines the conditions under which we can develop an inter-theoretical standard of truth. Cohen defends an historical and systematic analysis on the basis of philosophical principles, or as he puts it principles of the critique of knowledge (*Erkenntniskritik*), rather than the purely logical methods used by the founders of *Erkenntnistheorie.* Cohen is arguing that any critique of cognition must go beyond such logicism in order to arrive at a meta-theoretical perspective. As we will see in the next Part, Cohen argues that we cannot give a foundation for the calculus through logic alone: "the concept of an infinitesimal quantity [*i.e. of the differential*] can count as a penetrating example of the necessity *to complement logic* through a related, but distinct, area of research."[127] This area is the historical and philosophical critique of cognition according to philosophical principles.

Cohen's basic argument for the necessity of a meta-theoretical critique based on principles of cognition has two parts. First he argues that any physical theory is based on the prior specification of certain mathematical relations. Further, though, those mathematical relations must be put into a structure of explanation. If two theories or constructs are meant to explain the same phenomena, we can compare them based on the mathematical relations they specify or we can compare them based on how they fit into a meta-theoretical structure of explanation. Identifying and evaluating

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127 Cohen 1928 [1883b], §1. All the following citations from this work are my translation.

the philosophical principles of cognition at work in a theory's structure of explanation can tell us how well the theory contributes to our attempt to solve basic problems.

The analysis based on principles thus distinguishes Cohen's philosophy from other forms of "idealism," as he emphasizes:

> Idealism in general dissolves things into *appearances* and *ideas*. In contrast, the critique of cognition dissolves science into those *presuppositions* and *foundations* immanent in its propositions and taken for themselves. Critical idealism[128] takes as its objects not so much things and events, or even consciousness as such, but *scientific facts* [*Tatsachen*].[129]

One can distinguish the "presuppositions and foundations immanent in scientific propositions" from the foundations of science in general. Cohen's critique of cognition applies only to the *results* of cognition, scientific facts. A foundation need only support the particular fact for which it forms part of the structure of explanation. Any metatheoretical analysis will then depend on analysis by philosophical principles.

Analysis by principles depends on separating the mathematical content of a theory from its presuppositions or hypotheses. Those presuppositions can then be evaluated in themselves, according to the method we used above to evaluate Brunelleschi's cupola and Kepler's planetary orbits. That is, one can look at how the presuppositions and hypotheses made by the theory led to its specifying a single class of objects, that is, led to a result: a scientific fact. Cohen argues that it does not matter which principle is ultimately at issue. The analysis requires only that *some* unifying principle be found:

> Thus the critique of cognition consists in the justification of those conditions on which the mathematical science of nature rests. Surely we dare not succumb to capricious choice [*Willkür*] in enumerating and reconstructing the conditions… Rather we will refer back to the single structure and object

[128] Following Andrea Poma, I refer to Cohen's doctrine as "critical idealism." The term in the original German is "Erkenntniskritisch Idealismus."

[129] Cohen 1928 [1883b], §9.

> that the foundation supports. All basic principles of the critique of cognition are of equal value as such. Thus a *reconstruction of scientific experience* can begin with any one of these, and similarly may be completed with one [principle].[130]

Analysis by means of principles is akin to our attempted reconstruction of Brunelleschi's architectural plan. All such analysis must refer to the scientific fact or object it is trying to explain. Cohen wants to separate the reconstruction of the mathematical axioms or principles of a theory from the description of the basic concepts of the theory.

Cohen's students and the later members of the Marburg School of neo-Kantianism that he founded, Paul Natorp and Ernst Cassirer, both emphasize this formalist aspect of Cohen's philosophy of science in essays on the Marburg School. Natorp remarks:

> We distinguish, according to Kant's different definitions of the term "transcendental," between the psychological and metaphysical and the merely logical sense; the latter is the old Aristotelian and Wolffian conception and barely distinct from modern logicism, although there have been significant improvements in the individual positions. What is similar is that they begin from fundamental and irreducible concepts and indemonstrable propositions and aim at judgments of identity ("analytic" in Kant's sense).[131]

Trendelenburg, as an exponent of *Erkenntnistheorie*, takes this logicist approach. He argues as well that epistemology is based on the physiological and psychological process or of representation in the individual. Ernst Cassirer observes that Cohen abandoned that conception of *Erkenntnistheorie*:

> [T]hough it is idealist, the critique of knowledge [*Erkenntniskritik*] has a rigorous *objective* inclination: it does not deal with representations and processes in the thinking individual, but with the valid relations between principles and laws, which, as such, must be established independently of any consideration of the psychological manifestation of thought in the subject.

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130 Cohen 1928 [1883b], §13, my translation.

131 Natorp 1918, 196. My revised translation of a draft translation by R. Pates.

> This idea, fundamental to the "transcendental" methodology, proved to be especially effective and fruitful in the development of 19th century philosophy.[132]

The influence of Lazarus and Steinthal on Cohen's work goes hand in hand with Cohen's critique of Trendelenburg's *Erkenntnistheorie.* Cohen accepts the idea, familiar from Völkerpsychologie, that methods for the critique of knowledge, as a rational process, must be distinguished from a description of the individual's thought processes and physical means of perception. This basic point of orientation leads Cohen to reject Trendelenburg's idea that the subject matter of epistemology should be individual representations. Cohen argues that philosophical reasoning should be focused on the analysis of *objectivity*, and that the objective should be distinguished as sharply as possible from the representations and mechanisms employed by the individual. The foundation of these representations and mechanisms, the laws of thought, should be supplemented by a peculiarly philosophical (transcendental) analysis that deals with the validity of principles and laws.[133] The latter, philosophical analysis will be most effective within a well-founded historical analysis, which reveals how the progressive use of reason can promote the goals of science and philosophy.

My final subject will be Cohen's method for the history of philosophy and of science.[134] Cohen's theory belongs in the class of axiomatic formulations of physical

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132 Cassirer 1918, 257, my translation.

133 In Cohen's book *Das Prinzip der Infinitesimalmethode und seine Geschichte* he puts the need for such a philosophical analysis as follows: "Insofar as we seek to ground the concept of the infinitesimal solely in logic, we will perceive the lack of such a grounding—despite uncounted efforts that have been undertaken since the discovery of the calculus, always giving a logical justification on new grounds. Hence, the concept of an infinitesimal quantity counts as a penetrating example of the necessity to *complete* logic through another related, but distinct, area of research" (Cohen 1928 [1883], §1).

134 Hertz [1864] 1956, 1: Here Robert Cohen has given a clear and interesting picture, in his appreciation of Hertz's Principles of Mechanics: "Clearly it is more important to analyze theories than to analyze terms, for even the most denotative of terms enters into scientific usage by its role in theoretical and experimental analysis, i.e., through its role in a systematic theory or in a system of apparatus. In whatever way it may be undertaken, analysis of theories generally comes to be a rational reconstruction of an existing body of thought, formulating, in logical sequence, the natural laws and their consequences for the field in question....Thus axiomatic formulation of a body of scientific knowledge enables us to know more exactly what we are talking about; perhaps it is best to put this negatively, by saying that axiomatic formulation reveals what we do not know but about which we are in danger of self-deception. The great strength of this deductive procedure is that the primitive terms and fundamental axioms of the system form a model-system to which the natural processes are akin. Hertz wrote of this: "We form for ourselves (internal) images or symbols of external objects; and the

theories. One account of the historical evaluation of scientific and philosophical theories reconstruction has it that we need to reconstruct the process of reasoning each scientist followed; ideally, this will to lay bare the conceptual relations between elements of the theory. So we might say that Mach was committed to the claim that what he called "connections," which roughly means concepts or relations, must be inferred from the phenomena, from sense-data. Mach also accepted the claim that there was at the time no direct verification in experience for the atomic hypothesis. Thus, we could say, the previous two claims are the necessary and sufficient reasons why Mach discarded the atomic hypothesis.

Cohen rejects this sort of history of philosophy and of science. He does so because it is, in his view, a representation of a psychological process, not an argument justified by good evidence. For Cohen, what we are trying to capture with an historical reconstruction of a theory is a relationship between scientific facts for which there is, or could be, evidence. Reconstructing what such-and-such a scientist must have thought can be very dubious, especially when the scientist is not our contemporary. Cohen argues that any reconstruction of conscious reasoning must fail as science, simply because conscious reasoning is historically constituted. He does not mean that material conditions like average rainfall alone alter one's reasoning, but rather that the background intellectual conditions, such as the facts in evidence, may be very different. Cohen argues that in reconstructing science the facts, and not a meta-representation of a conscious process, should guide the researcher. Cohen rejects psychological reconstructions *because* he has an historical account of consciousness and reasoning.

In direct contrast to methods for the reconstruction of scientific theories based on conceptual clarification alone, Cohen developed early on a type of historical reconstruction of scientific theories. Cohen treats the axioms of a theory as synthetic rather than analytic propositions: they are principles that govern the construction of a theory, rather than first principles of a deduction. Here Cohen goes counter to the type of logicist epistemology favored by Trendelenburg, for instance. Cohen's approach is to use this historical method in conjunction with a hierarchy of the

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form which we give them is such that the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the things pictured."

sciences. The idea is to start with the objective, proven facts of science, or of those sciences that operate at the "ground level," like mechanics, chemistry, or experimental physics. Cohen takes the fact of our basic grasp of the results of scientific enquiry as the basis for analysis. The project is then to take that which gives scientific theories and propositions their unity and coherence, the conceptual functions and relations that make it possible for us to classify the phenomena and to unify them under general causal laws, as the subject of comparative evaluation.

Cohen is able to argue that the functional, axiomatic reconstruction of a theory can allow the philosophical historian of science to evaluate the formal properties of a theory, without assuming that the theory is merely a system of inferences, for instance. This is part of the gentle methodological recommendation that I'm making, with Cohen: that we consider philosophy's goals to be continuous with those of the sciences, and that we work to make sure that this is in fact the case. I will conclude with Cohen's own observation on the subject:

> And now, with this factual grounding of philosophy as logic of science, we grasp the relationship between philosophy and its history: This history, to which philosophy has a natural, innate relation, is *eo ipso* the history of science, which latter we habitually and justifiably categorize as mathematical science of nature. There are not two basic streams of the history of philosophy, the history of philosophy of logic and of the philosophy of mathematical natural science: rather both sources flow together, both roots stretch themselves in both directions, and grow in both.... This is the relation that exists between philosophy and its history: the relationship to their common factual sources, to the roots of their method and to their history.[135]

135 Cohen 1928 [1914], 189.

PART TWO

HERMANN COHEN'S HISTORY AND PHILOSOPHY OF SCIENCE

CHAPTER THREE

The Infinitesimal Method

"Ah, Edward," cried the disembodied voice of the head of the family from forty miles away at Gattenden. "Such a really remarkable discovery. I wanted your opinion on it. About God. You know the formula: *m* over nought equals infinity, *m* being any positive number? Well, why not reduce the equation to a simpler form by multiplying both sides by nought? In which case you have *m* equals infinity times nought. That is to say that a positive number is the product of zero and infinity. Doesn't it?" The diaphragm of the telephone receiver was infected by Lord Gattenden's excitement forty miles away. It talked with breathless speed; its questions were earnest and insistent. "Doesn't it, Edward?" All his life the fifth marquess had been looking for the absolute... Could it be that he had now caught it, so easily, and in such an unlikely place as an elementary schoolbook on the theory of limits? It was something that justified excitement. "What's your opinion, Edward?"

"Well," began Lord Edward, and at the other end of the electrified wire, forty miles away, his brother knew, from the tone in which that single word was spoken, that it was no good. The Absolute's tail was still unsalted.

Point Counter Point, Aldous Huxley[1]

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[1] This epigraph comes from *Point Counter Point* by Aldous Huxley, which is, among other things, a satire of the 19th and early 20th century intelligentsia. (It was originally published by Harper and Row in 1928. My citation is from the 1965 Perennial Classic paperback edition published by Harper & Row, pages 139-140.) Lord Edward Tantamount, the Lord Edward of the quotation, is generally supposed to be an avatar of Aldous Huxley's relation T.H. Huxley, a 19th century scientist. Lord Edward's wife, Lady Tantamount, is thought to be a literary representation of Lady Ottoline Morell, famously associated with Bertrand Russell. I haven't yet been able to place Lord Gattenden, Edward Tantamount's elder brother in the book.

3.1 *The Infinitesimal Method*

In 1883 Hermann Cohen published *The Principle of the Infinitesimal Method and its History*.[2] The system of critical idealism that Cohen worked out in the book was a foundation stone of Cohen's philosophy of science. There are three major parts to the work: a lengthy Introduction, a History, and a section on further progress (*Ausführungen*). In the Introduction, Cohen explains why his examination of the "infinitesimal method" is epistemologically significant. For Cohen, the historical debates over the foundations of the infinitesimal method show why epistemology should not be limited to logic or psychology. Instead, he urges, the *Erkenntnistheorie* of Trendelenburg, Herbart and Helmholtz should be re-named *Erkenntniskritik*, or knowledge critique. The main job of the Introduction is to give the requirements for a critical method. In the brief outline that Cohen gives in the *PIM*, he explains how the Kantian method can solve the legendary disputes over the foundations of the "infinitesimal method" since its discovery by Newton and Leibniz.

Cohen observes that the calculus was invented in the context of finding a solution to three closely related problems: the quadrature of a curve, or what is now called finding the definite integral; the "problem of tangents," which encompasses several related problems of differentiation; and the question of finding the sum and limit of an infinite numeric series.[3] A curve can be taken as a graphic representation of a continuous increase in velocity, for instance. All these methods are strategies to find one of two things: finding the tangent to a point on a curve, or differentiation, and finding the area under a curve, a rough definition of integration. Historically, quadrature was a method of integration, and finding the tangent was a method of differentiation. Methods for differentiation and integration date back to Hippocrates and Archimedes.[4] In the sketch that follows, I will show how Newton and Leibniz

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[2] Hereafter cited as *PIM*. All citations from this work are my translation from the Cassirer and Görland edition.

[3] As Marc de Launay remarks, this way of presenting the history is reflected in Léon Brunscvicg's 1922 work, *Les étapes de la philosophie mathématique* (de Launay 1999, 12n).

[4] "The first quadrature of a figure with curvilinear boundary was achieved by Hippocrates in the fifth century B.C. Hippocrates showed that the area of the lunule … (that is, the figure bounded by one-half of a circle of radius 1 and one-quarter of a circle of radius $\sqrt{2}$) is equal to the area of the unit square B [*That is, the unit square made up of two radii of the half-circle, at right angles to each other, and the two*

solved the problems of the calculus, i.e. the quadrature of a given curve and the problem of tangents. In so doing I will explain the differences between the limit method and the infinitesimal method in the context of their use by Newton, Leibniz and others such as Jean d'Alembert and Lazare Carnot. Finally, I will introduce Cohen's own account, according to which the grounding of the calculus is in fact a question of giving a foundation for what he calls "intensive reality," or calculations using variable quantities instead of the so-called "limit method." I will conclude this section with an explanation of what Cohen considers to be the problem of giving a foundation for "intensive reality." This foundation has two components: first, an epistemological argument based on Kant's critical philosophy; and second, an argument that the foundation for the calculus should be in terms of function theory, based on Augustin Cournot's account.

The first historical question Cohen considers is the quadrature of the curve. The quadrature of a curve is the problem, generally speaking, of measuring the area under that curve. The technique that came to be called "quadrature" originated in the techniques developed to find a circle equal in area to any given plane figure. Since curvilinear figures were the most difficult, mathematicians began to concentrate more and more on finding methods to "square the circle," or to find a square of the same area as any given circle or curve.

For a long while techniques of quadrature rested on what Cohen calls the "limit method." According to this method, "the ratio between a plane surface delimited by a curve and the polygon inscribed in it could be...determined on the basis, not of intuition, but of the theorem according to which 'the sum of all the remaining sections would be less than any possible surface'."[5] Cohen traces the use of this method back to Archimedes' *Quadrature of the Parabola.* He points out that the notion of the infinitely small employed by thinkers from the Middle Ages to the 16th century (such as Giordano Bruno and Nicholas of Cusa) was inspired by this Archimedean

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tangent points to the intersection of these radii and the half-circle.]...In the third century B.C. Archimedes effected the quadrature of a parabolic segment. He showed that its area is $4/3\Delta$, where Δ is the triangle of maximal area inscribed in the parabolic segment" (Shenitzer and Steprans 1994, 66).

[5] *PIM* §35.

definition.[6] In order to find the area of a curvilinear figure, Archimedes' method of exhaustion inscribes a polygon of infinitely many sides within the figure, so that the area remaining between the inscribed figure and the boundaries (limits) of the polygon is less than any *possible* surface.[7]

The problem with this method, Cohen argues, is that it depends on the purely *negative* concept of a limit and of the infinitely small. "This deficient sense of the infinitely small, in the *exclusively negative concept of a limit*, also determines the majority of the mighty attempts to attack this problem that stimulated the 17th century."[8] The limit method was applied to the problem of differentiation, or drawing a tangent to a given point on a curve. A tangent point is on the boundary, or limit, of a curve. One way to find a tangent is to find the limit beyond which a point cannot go and still be on the curve. Then a line might be deemed a tangent if one can prove, by some means, that it approaches the boundary of the curve so closely that the length between the point on the line that approaches the curve and the point on the curve that approaches the line is smaller than any possible surface or quantity. The reasoning behind this "negative concept of a limit," as Cohen calls it, is that if the gap between a line and the curve is so small at a given point that there is no determinate quantity equal to it, then the point on the line and the point on the curve coincide, and we can call the line a tangent to the curve at that point.

The positive concept of a limit implies that the point on the curve that coincides with the line, and the point on a line tangent to a curve, can be proven to be the *same* point: an infinitesimal. Cohen calls this the "infinitesimal method." Cohen points out that the differences between the limit method and the infinitesimal method hinge upon two concepts: the concepts of equality and of identity. The limit method

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[6] While it is beyond the scope of my treatment here to make a detailed investigation of Archimedes' significance for Cohen's historical account, it should be noted that Cohen treats Archimedes as solely an exponent of the method of exhaustion and the limit concept based on that method. Cohen could not have been aware, in 1883, of Archimedes' text *The Method*, rediscovered in 1906, in which Archimedes makes use of something very like the infinitesimal method. (See, e.g., Boyer 1970.)

[7] For a translation of Archimedes' text, including diagrams, of "The Quadrature of the Parabola," see Heath 1897, 233-252.

[8] *PIM* §36. Among these attempts he includes those of Cavalieri, Roberval, and Mersenne. It is beyond the scope of my treatment here to give a detailed investigation of their methods. For a detailed account of the contributions of Cavalieri, Roberval and Mersenne, see *PIM* §§36-40; see also Brunschvicg 1922, 150-170.

depends on being able to call a point on a curve "equal" to a point on a line that approaches the curve within an infinitely small limit. The infinitesimal method depends on being able to call a point on a curve "identical" to a point on a line tangent to that curve. Cohen's project in *PIM* is in part to clarify the two concepts, of equality and of identity, and to give the principles that underlie their use.

Having presented an initial sketch of Cohen's account of the infinitesimal method in mathematics, it remains to show why the foundation of the method poses a special problem for epistemology. In Chapter One, I presented the *Erkenntnistheorie* of Cohen's time as a discipline with two strategies: the logicist project of finding analytic expressions for the facts given in representation, and the causal or genetic account of thought and perception. These strategies were aimed at a common goal: to be able to give a single system of principles for philosophy and for empirical science.[9] Cohen attacks both these foundations for epistemology, and argues that only a critical, neo-Kantian method can meet his own requirements.

Cohen argues that psychology cannot give a proper foundation for the theory of knowledge, because the foundations of knowledge must be *prior* to psychological processes. Here he opposes Leibniz, "a thinker who embraces the vast requirements of knowledge at the fundamental level," to Locke, who "analyzed the sense-mechanism of cognition – and, as far as we trust his method, uncovered real and important methods and powers."[10] While Cohen acknowledges that Leibniz's reliance on logic is itself problematic, he argues that psychology does not give a secure enough foundation for epistemology either. He gives two reasons for this. First, psychology is concerned with the causes and effects of thought, and not with the conceptual foundations of our knowledge. Indeed, he argues, "psychology itself presupposes the concepts of the theory of knowledge."[11] Further, psychology cannot identify the presuppositions and foundations inherent in scientific propositions, because psychology is concerned with "hypothetical elements," that is, with the tools of thought instead of the reasons behind our thoughts. Cohen objects that whereas

[9] See the Introduction to Chapter One for textual support for these claims.

[10] *PIM* §6.

[11] *Ibid.*

psychology is (merely) descriptive, the new discipline we're looking for needs to give a secure grounding for our knowledge:

> Psychology develops a *description of consciousness* from its elements. These elements must therefore be—and remain—hypothetical, because nothing operating with consciousness, and arising in it, can dig up and establish that with which consciousness in fact begins and from which it develops.[12]

This is the radical break between Cohen's method and Herbart's, for instance. Cohen argues that while the project of describing consciousness gives us a description of the results of thought or perception (for instance, in terms of how frequent or how intense our thoughts or perceptions are), such a project cannot reveal the source of, and the justification for, the knowledge thus gained. Consciousness is not yet knowledge, Cohen claims, but rather is a "subjective fact" that tells us about only the subject, and not the objects, of knowledge. So on Cohen's terms, a descriptive or "empirical psychology" along Herbartian lines fails as an epistemology.

Cohen argues that even to use the name *Erkenntnistheorie* to refer to epistemology is objectionable, because it conjures up the view according to which cognition is a psychological process. Trendelenburg's and Überweg's foundations for *Erkenntnistheorie* are ruled out by this reasoning as well. Cohen argues that geometry, including the calculus, can produce synthetic proofs *a priori* that cannot be justified by means of a psychological account. Cohen argues that Trendelenburg's method is wrong, not because Trendelenburg uses inductive methods, but because Trendelenburg does not allow for mathematical reasoning to ground judgments about reality. Cohen is perfectly willing to allow inductive methods in his epistemology, but he argues that epistemology must be able to explain and to justify mathematical conjectures, such as the Beltrami sphere discussed in Chapter One.

Cohen's project for epistemology is to demonstrate the foundation for our representations in synthetic reasoning *a priori*. As such, Cohen argues that we should

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[12] *PIM* §7. I am grateful to Alison Laywine for translating most of this difficult paragraph. The original German reads: „Psychologie entwirft die *Beschreibung des Bewußtseins* aus seinen Elementen. Diese Elemente müssen daher hypothetische sein—und bleiben, dieweil dasjenige, womit in Wahrheit das Bewußtsein beginnt und worin es entspring, kein mit Bewußtsein Operierender auszugraben und festzustellen vermag."

use a critical, idealist method of epistemology that looks for the principled foundation of scientific facts:

> In contrast, if I take knowledge not as a type or method of consciousness, but as a *fact*, which came about in *science* and continues to take place *from a given grounding*,[13] then the investigation no longer refers to a subjective fact, but to a state of affairs given objectively and *founded on principles*, not on the process and apparatus of cognition, but on the result of these, science.[14]

This is one of Cohen's arguments against the genetic or causal justifications of knowledge given by the physiology of perception, for instance. For Cohen, knowledge may be explained by means of a physiological or psychological mechanism, such as taking the eye as a camera obscura, but it cannot be justified by those means. This is not to say that causal explanations are irrelevant. In fact, causal explanations are valuable, in that they yield the *facts* of science that form the material for critical inquiry. However, such explanations need further justification. Cohen's position does not necessarily entail a radical break with Helmholtz, for instance. In Chapter One, I described how Helmholtz accepted an idealist explanation of perception, according to which even phenomena such as separation in space are due to the peculiarities of our perception. Helmholtz was content to stop there, according to his Fichtean principle that the limits of the self determine the limits of knowledge. Cohen, on the other hand, argues that the foundation for knowledge *should be* given by means of an analysis of the foundation for synthetic reasoning *a priori*, such as Newton's laws of physics and Euclid's geometrical demonstrations.

The justification for such reasoning must go beyond the basis provided by what Cohen calls "traditional logic." He observes,

> [t]he foundation of the infinitesimal concept is a concern of philosophy in *two* respects. First the conscience of traditional *logic* cannot be set at ease before we have described and clarified this basic concept of mathematical

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13 Here I use the word "grounding" and not "foundation" advisedly. Cohen spoke in various places (including his Introduction to the *History of Materialism*, Cohen 1928 [1914]) of the distinction between a "Grundlage" and a "Grundlegung." Only the latter, he says, gives the correct picture of the basis of knowledge.

14 *PIM* §7.

science as far as is possible, given its methods and according to its own rules. Further, though, a lacuna remains in the enumeration of the foundations and basic propositions of knowledge as long as this fundamental tool is not recognized and defined as a *presupposition* of mathematics and consequently of the cognition of nature. These considerations support each other. Insofar as we seek to ground the concept of the infinitesimal solely in logic, the lack of such a foundation will be felt—despite the innumerable efforts that have been undertaken since the discovery of the calculus to give a logical justification on new grounds. Hence the concept of an infinitesimal quantity counts as a penetrating example of the necessity *to complement logic* through a related, but distinct, area of research.[15]

The problem with a purely logical inquiry into the foundation of the infinitesimal method is that, as I remarked above, for Cohen the infinitesimal method requires a revision to the concepts of equality and of identity. A differential, as described above, is not *equal to* any number on the real line, nor is it a ratio of identity between any two such numbers.

According to Cohen, the foundation for the infinitesimal method requires a proof of the equality of two points by means of thought alone. In other words, it requires a demonstration that we are constrained by the laws of thought to think of the point on a line tangent to a curve, and the point on the curve that the line touches, as equal to each other: as an infinitesimal, or what is now sometimes called an increment. Cohen argues that giving such a proof is similar to proving that two numbers are equal to each other. We know that 2 + 2 is equal to 1 + 3, because the laws of arithmetic tell us so. Cohen points out that what is needed to give a foundation to the infinitesimal method is to show that the notion of equality allows us to prove, without appeal to sense psychology and *a priori*, that the laws of thought can prove the equality of the tangent point and the point on a curve.

Cohen argues that this proof is a matter of giving a conceptual foundation for the infinitesimal method. To show how the concept of equality can be given such a foundation, Cohen examines its use in the historical systems for the calculus. Since

15 *PIM* §1.

Cohen is looking for a foundation for a fact about *thought* itself, he critically examines a series of attempts to ground the calculus. In what follows I will present his investigation. First, I present Leibniz's and Newton's own foundations for the calculus. Then I present Cohen's series of critical reflections on the progress of the foundations for the infinitesimal and the limit method. He examines D'Alembert's attempt at a foundation for the limit method, and Carnot's "metaphysics" of the infinitesimal method. Cohen concludes by endorsing the account of Augustin Cournot, who argues that Newton's and Leibniz's methods can be placed on the same conceptual footing. Cournot argues that function theory, combined the method of using higher-level differentials to determine first-order differentials, can give us a demonstration a priori that grounds the infinitesimal method in the laws of thought. Finally, I will present Cohen's account of how Kantian epistemology can show that the infinitesimal method, while it is determined by means of pure thought, nevertheless refers to real phenomena.

3.2 History and System

In the section called "History," Cohen analyzes the difference between the limit method and the infinitesimal method in the context of the use of these methods by Newton and Leibniz, respectively. Cohen concludes that the difference between Newton's and Leibniz's foundations for the calculus have been made obsolete by Augustin Cournot's work, which gives a foundation for the calculus based on function theory.

To Cohen, the priority debate between Newton and Leibniz, over who discovered the calculus first, is less interesting than the differences of method and philosophical system between Newton and Leibniz. Cohen concludes from his investigation that Leibniz's goal was the precision and refinement of his logic with the ultimate purpose of constructing a *characteristica generalis*, whereas Newton's aim was to construct a system of nature based on universal natural laws.[16] Hence, Cohen believes that, while Leibniz was interested in establishing the logical foundation for the infinitesimal method, Newton was concerned with proving that his fluxion calculus applied to real, physical processes:

> We know, at least, that Leibniz rejected the basic concepts of Newtonian mechanics and hence could not systematize the mathematical science of nature. We see this failure emanating from his larger desire: he wanted to restrain mathematics and all cognition of nature within the limits [*Paragraphos*]

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[16] As Cohen's student Ernst Cassirer observes, "Whereas Newton started out with the study of certain natural *phenomena* – with an investigation of optical phenomena and with a theory of the motion of the moon, Leibniz, on the other hand, began with a *logical analysis of truth*" (Cassirer 1943, 374). Cohen dismisses what he takes to be a wrongheaded account of the difference between Newton and Leibniz, according to which Leibniz's "monadological metaphysics" kept him from constructing a solid foundation for the calculus. In contrast, according to the account Cohen discards, only Newton had "elaborated and perfected this instrument, the utility of which he had certainly recognized from a logical point of view" (*PIM* §16). Cohen observes that this account is widely accepted, even by Carl Gerhardt, the editor of an extensive series of Leibniz's philosophical and mathematical works and the author of a mid-19th century history of "higher" analysis. The error in the account is subtle, Cohen acknowledges: it is in the assumption that Newton's "fluxion" concept rests, as Gerhardt put it, "on a perfectly assured basis," while Leibniz's algorithm for the calculus was discovered "almost by chance." Cohen argues that the opposite is true, that is, that Leibniz's infinitesimal concept was an integral part of his philosophical and logical system.

> of logic; the latter, however, grew from its own foundations, from principles that were not logical.[17]

Cohen concludes that Leibniz's attempts to give a secure logical foundation for the calculus must ultimately fail. Accordingly, Cohen follows what he takes to be the Newtonian path. That is, Cohen wants to show that the calculus is a part of the "mathematical science of nature," and in particular that the calculus can demonstrate results *a priori* that nonetheless hold true of real natural processes.

Finally, however, Cohen's historical analysis of the role of the calculus in Newton's and Leibniz's philosophical systems is meant to show that the difference between the two is less important than what they have in common: a search for a way to calculate using quantities that vary uniformly with time. Cohen investigates the subsequent attempts to give a foundation for such quantities, by critically evaluating attempts by Jean le Rond d'Alembert and Lazare Carnot to clarify the concepts of equality and identity basic to the calculus. Finally, he argues that a truly secure epistemological foundation is found in two places: the progressive, neo-Kantian approach he sketched in his essay on the Trendelenburg-Fischer debate, and the integration of function theory into the foundations of the calculus, championed by Cournot.

Cohen begins his historical presentation with a précis of Newton's fluxion calculus. Newton's method is presented in Chapter One of the *Principia.* Cohen points out that Newton avoids presupposing anything about the character of *time*, any more than he makes hypotheses about the structure of space. However, Newton conceives of quantities, in his own words, as "generated by a continuous process, like the space that a body describes or some moving thing describes." Newton considers these quantities to vary with time, since they are "generated by a continuous process" of motion. He conceives of a new type of variable quantity, distinct from ordinary variables, that is assumed to vary continuously and uniformly with time:

> Since the usual spatial quantity is considered...as increasing by degrees and infinitely in flux, it will be called *fluent* and symbolized by x, y, z. These indefinite increases by stages describe the *speeds* "according to which each

[17] *PIM* §10.

particular fluent is increased *by the movement that engenders it*;" these are therefore called *fluxions* and are described by x°, y°, z°.[18]

Cohen points out that Newton then needs some way to represent the "continuous process" that engenders quantity, other than by assuming some fixed idea of time, which would violate Newton's own strictures against making hypotheses. Newton bridges this gap by using the concept of *flux*, which he takes from Napier:

> Newton expresses the unfolding of that which takes place by means of the expression *flux*, a term successfully introduced by Napier to designate the production of quantities. Thus the rigor and exactitude of the concepts of space, of time and of quantity need to be supported by conformity to a law, which, it is possible to show, flux obeys: "I suppose that one among the posited quantities, similar to the others, increases in a uniform flux, and that the others are related to this quantity as to time: hence it would not be inappropriate to call this quantity time, by analogy." In consequence, the types of quantities that need to be distinguished will be deduced and defined as *modifications of flux*. [19]

Cohen argues that Newton has deduced the properties of fluxions from a variation of the law of continuity. Newton argues that flux, or the "continuous process" of motion, increases uniformly with time. Hence Newton does not need to appeal to a hypothesis in this case, but only to the foundation of a general law, the law of the uniform continuity of motion.

Newton uses fluxions to solve the problems of the calculus by combining them with the method of taking "first and last ratios." This method takes a given ratio between two quantities to go to zero as the quantities increase to infinity:

> One will understand this more clearly with infinitely large quantities. If two quantities, the difference between which is given, increase to infinity, their last ratio will be given and will certainly be a ratio of equality; similarly the

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[18] *PIM* §64. The citations are from Newton's *Opuscula mathematica, philosophica et philologica*, ed. J. Castillion. "Methodus fluxionum et serriarrum infinitarum cum ejusdem applicatione ad curvarum geometriam." 1744. Volume 1. Lausanne and Geneva.

[19] *Ibid.*

last or the largest quantities which correspond to this ratio will not be given quantities.[20]

This is a variation of the limit method described above. Newton's explanation in the above quotation applies well to a technique of integration for a circle. One such technique inscribes a polygon inside a circle, and circumscribes another outside it. Newton's strategy above is to argue that if the number of sides of both polygons increases to infinity, but we know the limit beyond which they cannot go (the boundary of the circle), then ultimately the two polygons will meet each other—or, at least, they will be so close to each other that no possible surface can stretch between them.

Newton's method depends on the claim that the ratios between the polygons simply vanish. He calls these "evanescent quantities." Leibniz's presentation of the infinitesimal method, on the other hand, insists on the fact that the quantities are not zero, but are less than any conceivable positive quantity. Hence, Leibniz insists that these quantities demand a revision to what Leibniz takes to be the logical concept of equality. In what follows, I will present Leibniz's method as he puts it forward in an addendum to a letter of 1702, called "Justification of the Infinitesimal Calculus by that of Ordinary Algebra."[21]

In the "Justification," Leibniz presents the following diagram:

[20] Cited in *PIM* §65. Cohen is citing here from the following edition: *Opera quae entant omnia*, vol. 2, ed. S. Horsley. 1779-1785. London, as well as the German translation by J. Ph. Wolfers, Berlin 1872. The citation is from the Scholium to Lemma 11.

[21] Leibniz 1976 [1702], 545-6; addendum to the letter to Varignon of 1702.

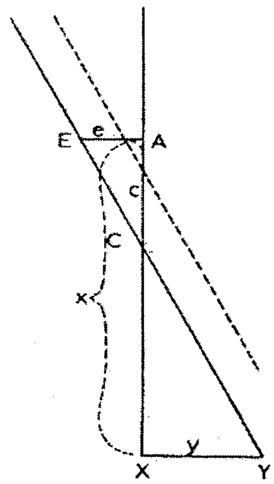

Figure 1 (Leibniz 1976 [1702], 545)

Leibniz sets up his reasoning by establishing the ratios between the quantities x, y, c and e, given the rules of the construction. First, Leibniz assumes that the angles XCY and CYX are *not* 45-degree angles, and that AX is perpendicular to XY. By the usual geometrical rules, the triangle ACE is similar to the triangle CXY. Leibniz uses this fact to construct the following proportion:

$$\frac{(x-c)}{y} = \frac{c}{e}$$

Now, Leibniz asks, what will become of this equation when we take the line EY to pass through the point A? (That is, on the above diagram, if we move the line EY up and to the right.) The quantities c and e will be smaller than any quantity we can draw, because the lines AE and AC now coincide at point A. But the above proportion still holds.[22] Leibniz concludes that the original equation can be rewritten, for this new case, as x/y = c/e. Then, Leibniz points out, the ordinary rules of algebra allow us to infer that x-c = x.[23] According to the usual definition of equality, the only quantity c can be is zero. But, Leibniz shows, c is not zero, because it preserves the ratio of CX to XY. If c and e were zero, then the ratio of CX to CY

[22] To be precise, it holds under two assumptions: that the angle is not 45 degrees, and thus that the ratio between c and e is not 1:1.

[23] Because (x-c)/y = c/e, and x/y = c/e, so (x-c)/y = x/y, therefore, multiplying both sides by y, x-c = x.

would be one to one. That cannot be true, because by hypothesis the angles CXY and CYX are not 45-degree angles. Since AX is perpendicular to XY, then CXY is a right angle. Hence, either CXY and CYX are 45-degree angles, or they are not equal to each other. Thus they are not equal, and therefore c and e must not be zero.

While Leibniz's construction does not draw a tangent to a curve, it is an ingenious approximation of a construction in the calculus. If EAX represented a smooth curve, taking line EY to the point A as limit would make EY a tangent to (curve) EAX at point A.[24] Leibniz concludes that since his construction appeals to only the ordinary rules of algebra, the calculus rests on the same secure foundation.

Nonetheless, Leibniz observes that his reasoning in the above demonstration challenges the ordinary notion of equality. His remarks about the foundation of his resolution to the challenge are far from persuasive:

> Although it is not at all rigorously true that rest is a kind of motion or that equality is a kind of inequality, any more than it is true that a circle is a kind of regular polygon, it can be said, nevertheless, that rest, equality, and the circle terminate the motions, the inequalities, and the regular polygons which arrive at them by a continuous change and vanish in them.[25]

Newton and Leibniz are struggling with the same basic problem: finding a foundation for the calculus in the law of continuity. However, as Cohen remarks above, Leibniz goes about the task by making revisions to logic alone. As Leon Brunschvicg observes, Leibniz's version of the law of continuity "is the principle of a new logic, which Leibniz will bring to its highest degree of clarity when, in his 'Justification of the Infinitesimal Calculus by that of Ordinary Algebra,' he will make the fundamental mathematical relation of equality 'a particular case of inequality. (Infinitely small) inequality,' he writes to Arnauld, 'becomes equality.'"[26]

In contrast, Newton's limit method does not require revisions to the concept of equality. Indeed, Newton's adherents, such as d'Alembert, argued that giving a

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[24] For a construction that uses Leibniz's method to differentiate a curve, see the Appendix to Chapter Three.

[25] Cited in Loemker 1976, 546.

[26] Brunschvicg 1922, 182, my translation.

foundation for the calculus does not require any changes to number theory or logic. Their position was refined in a series of debates beginning in the mid-18th century, which Cohen tracks closely. In *The Analyst* of 1742, George Berkeley, an implacable enemy of the calculus, saves his most harsh contumely for Newton's method of taking the "first and last ratios." Berkeley charges that the "evanescent" quantities, the ratios of which Newton employs to get his results, are nothing but the "ghosts of departed quantities."[27] Berkeley's accusations galvanized debate around the issue. In 1742, Charles MacLaurin wrote *A Treatise of Fluxions* in defense of the calculus, but challenging the traditional methods of geometry. That was the catalyst for a character to appear on the scene who plays an unexpectedly central role in Cohen's book. In 1743, Jean le Rond d'Alembert published his *Traité de dynamique*, in which he presented a geometrical limit method for the calculus. D'Alembert says quite clearly that one of his reasons for using the limit method is the fact that the "lack of rigor" of the infinitesimal method has spawned doubts of the "certitude of geometry" – and here he cites MacLaurin's book of 1742. In what follows I will present d'Alembert's argument for the so-called "limit method," which is based consciously on Newton's method of "first and last ratios." In so doing I will explain why giving an alternative method to d'Alembert's was a central and pressing concern for Cohen, and why he thought that doing so was an *epistemological* requirement.

In his article on the differential ("Différentiel") for the *Encyclopédie*, d'Alembert bases his account of the differential calculus on Newton's method, because Leibniz's infinitesimal method, he says, "would damage the geometrical precision of the calculations."[28] D'Alembert characterizes Newton's method of finding first and last ratios as the way to find the "limits of ratios." In his article d'Alembert makes the following construction:

[27] Berkeley 1996 [1734], §35.

[28] D'Alembert 1966 [1754], my translation.

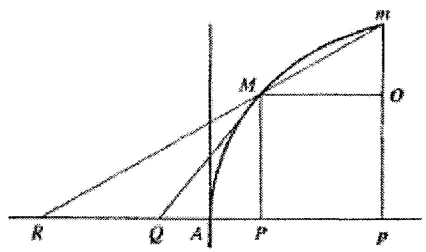

Figure 2

D'Alembert uses the above construction to solve the problem of tangents, or the problem of drawing a tangent to the curve AMm at a given point (M on the diagram). He argues that this can be done purely geometrically, without positing evanescent or infinitesimal quantities:

> Let AMm be an ordinary parabola, the equation for which, taking AP as x and PM as y...is yy=ax. One proposes to draw a tangent MQ to the parabola at the point M. Let us assume that the problem is solved, and imagine an ordinate pm at some finite distance from PM; and draw a line mMR from the points m and M. It is clear:
>
> 1. That the ratio MP/PQ of the ordinate to the subtangent is larger than the ratio MP/PR or mO/MO (which is equal to [MP/PR] because the triangles MOm and MPR are similar;
>
> 2. That the closer the point m draws to the point M, the more the point R will approach the point Q, and the more, consequently, the ratio MP/PR or mO/MO will approach the ratio MP/PQ; and that the first of these ratios could approach the second as closely as one desires, for PR can differ as little as one desires from PQ.
>
> Thus the ratio MP/PQ is the limit of the ratio mO/OM. For if one can find the limit of the ratio of mO to OM, expressed algebraically, one will have the

algebraic expression of the ratio between MP and PQ; and thus the algebraic expression of the ratio between the ordinate and the subtangent, which will go to find that subtangent.[29]

In the above reasoning D'Alembert takes a parabola with the equation $ax = y^2$, draws a tangent to that parabola at a point, and tries to find the length of the subtangent. He argues that finding the limit of the *ratio* mO/OM will give us "the algebraic expression of the ratio of the ordinate to the subtangent, which will enable us to find the subtangent." In the *Traité de dynamique* of 1743, d'Alembert adds the claim that the *limit* of the ratio mO/OM could even be taken to be equivalent to the ratio 0/0. Here, d'Alembert argues that "this limit is the quantity to which the ratio approaches more and more closely if we suppose [the sides of the triangle MOm] to be real and decreasing."

In the *Traité*, d'Alembert claims that the revision to the "metaphysics" of the differential calculus that he gives in the article cited above allows him to show that "[t]he method of the infinitely small is nothing but the method of first and last reasons [*raisons*], that is to say the ratios between the limits of finite quantities."[30] D'Alembert's article is meant to demonstrate that no revision to the concept of *number* is necessary for the foundation of the infinitesimal calculus. One of d'Alembert's purposes was to deal with the uneasiness of the attackers of the calculus, who had objected to the apparent certainty of algebra being undermined by Newton's and Leibniz's methods. D'Alembert says reassuringly that no such fictitious quantities as Leibniz's "c" above, or Newton's "evanescent" quantities, need to be introduced. Only the concept of the *limit* of a ratio between two real numbers, which could be equivalent to the ratio of zero to zero or between any two finite quantities, is necessary for the foundation to the calculus.

Cohen's interest in d'Alembert is manifest from the second section of his book, in which he remarks:

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[29] D'Alembert 1966 [1754], 986. The translation is my own. The drawing in Figure 2 is taken from Ewald 1996, 126.

[30] D'Alembert 1758, 49.

> Since D'Alembert, mathematicians have attempted to ground infinitesimal calculation in the method of limits. However, this method consists in the idea that the elementary concept of equality must be completed through the exact concept of a limit. Consequently the concept of equality was presupposed first. However, equality is no longer a part of logic. Equality corresponds to the logical concept of identity. Equality describes a relation between quantities. Carnot had already referred to this distinction, insofar as he distinguished between "egalité" as a "rapport" from "identité" as a "relation." Consequently the limit method presupposes, secondly, the concept of quantity. And this concept lies outside the bounds of logic.[31]

Cohen argues that the apparent simplicity of d'Alembert's presentation hides an important distinction, to which Lazare Carnot drew attention in his *Réfléxions sur la métaphysique du calcul infinitésimal*: the difference between equality as a mathematical "ratio," and identity as a logical "relation." In the *Réfléxions*, Carnot constructs the following diagram:

[31] *PIM* §2. Cohen leaves the words in quotations in the original French; they are from Carnot 1857 [1797], §42. "Rapport" here means "ratio."

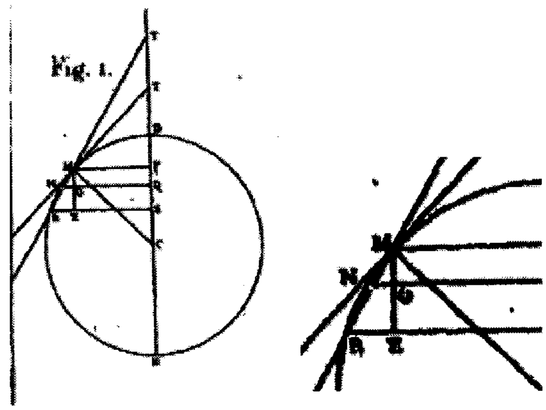

Figure 3, Carnot 1797

The diagram on the right is a detail of the diagram on the left. Carnot draws a tangent to the circle at the point M, and tries to find its slope, MZ, which is a technique of differentiation. One way to find the slope is by the limit method, and thus Carnot uses Newton's and d'Alembert's method of first and last ratios. Carnot's point is that even if one uses d'Alembert's method, it is still necessary to make certain revisions to the concepts of equality and identity. Carnot's argument is not that the infinitesimal method is more rigorous already, but that both versions of the calculus reveal the need for revisions to their foundations.[32]

The limit method takes the known quantities, the ratio of which determines MZ (that is, ZR and MR in the diagram), to go to zero as limit. Newton would treat MZ as a vanishing or "evanescent" quantity, that is, a quantity that vanishes as the values become infinitely large. However, Carnot argues, the quantity MZ does not "vanish"

[32] Carnot argues here, though indirectly, against d'Alembert's assumption that there will always *be* a limit, that is, that curves are everywhere differentiable. D'Alembert admits that he begins his construction by assuming that the problem has been solved, by assuming a tangent has been drawn already. In the course of the 19th and early 20th centuries, mathematicians proved this assumption inadmissible. Karl Weierstraß, for instance, exhibited a curve differentiable nowhere. Paul du Bois-Reymond, whom Cohen cites, was interested in finding functions that could not be differentiated everywhere. Further, d'Alembert's appeal to a series of ratios was replaced later, by mathematicians such as Cauchy and Weierstraß, by the more rigorous notion of the convergence of an infinite series. Analyzing series convergence leaves open the possibility that the series does *not* converge, and therefore the convergence approach does not make the assumption that all curves are differentiable everywhere.

at all, because it will always be equal to itself and never to zero: "Thus, whereas in general one will have 0 = 2x0 [*2 times zero*] = 3x0 = 4x0 = etc., one cannot say of an evanescent quantity such as MZ, that MZ = 2MZ = 3MZ = 4MZ = etc.; for the law of continuity cannot assign between MZ and MZ any other ratio than that of equality, or any other relation than that of identity."[33] Carnot's justification for saying so is that even though ZR and MR tend to zero as limit, they do not actually arrive at zero. Carnot points out that just as dx does not have the usual properties of real numbers, MZ, defined as the last ratio of ZR: MR, does not have the properties of a ratio of 0:0. D'Alembert had argued that a limiting ratio could be "equivalent to" the ratio of zero to zero. But Carnot points out that this notion of "equivalent to" does not correspond to any of the properties that we have to work with, namely: equality, a mathematical ratio between quantities, and identity, a logical relation between quantities. There is no such thing as saying that a number is "equivalent to" another number. D'Alembert's method, Carnot says, reveals at least the need to revise the concepts at the foundation of the calculus.[34]

Carnot argues that the fundamental relations of identity and equality are fixed, so a final ratio is another kind of quantity than a ratio between zero and zero—and not one that escapes being governed by the basic relations. In other words, we cannot say that the ratio dx/dy is somehow governed by a new set of relations. The ratio between two non-zero quantities, however "evanescent," is not zero, and thus dx/dy is not equal to 2·dx/dy or to 3·dx/dy. That means, then, that we must deal with Newton's fluxions and Leibniz's differential quantities as real quantities, and not as a ratio between zero and zero, as d'Alembert urges us to.

Cohen argues that Newton's and Leibniz's methods need to be given a common conceptual foundation. He claims that the relations needed to provide such a foundation are necessarily *a priori* and are in need of an examination from an epistemological standpoint. The first challenge Cohen answers by reference to an important work in the foundations of the infinitesimal calculus: Cournot's *Traité élémentaire de la Théorie des Fonctions et du Calcul infinitésimal*. In the *Traité*, Cournot

33 Carnot 1797, 174.

34 For a full account of Carnot's reasoning, see Carnot 1797, 139-180.

explains how Newton's fluxion method and Leibniz's infinitesimal calculations are in fact governed by the same set of functions, and, thus, are given a common basis by function theory and the law of continuity of motion. The details of Cohen's account will depend on how he accounts for the basic notion of continuity. I will present his account in the context of a 20th century appraisal by Bertrand Russell. This appraisal raises objections to the Kantian foundation of Cohen's work. It is beyond the scope of my discussion to give a definitive answer to these objections, but I will raise a few questions about the accuracy of Russell's description.

Cohen concludes that the concept of continuity of motion is fundamental to the calculus. This method was set out in Cournot's *Traité élémentaire.* As I mentioned above, Newton's and Leibniz's attempts at a foundation for the calculus were based on the attempt to submit the calculus to the law of continuity. Cournot points out that this project was not wrongheaded, but that Newton and Leibniz were not able to put their foundation on a secure basis, because they did not have the framework of function theory.

One purpose of Cournot's book is to give a conceptual foundation for the differential calculus on the basis of function theory. As Cohen observes, the origins of the use of function theory for that purpose can be found in the French tradition, including the work of Lagrange and Lacroix:

> In general, as he says in the concluding paragraphs of his treatise, Lagrange wanted to show that the method of the infinitely small was nothing but an "ingenious tool," founded on function theory. He uses it himself in the second edition of his *Mécanique analytique.* In the same way, at the end of his *Théorie des fonctions,* he refers to "*Lacroix's* new treatise of the differential calculus, wherein the calculus is presented '*from all its perspectives*' ('*sous tous ces points de vue*'). And in the *preface* to his work, which contains an historical sketch, Lacroix cites the following remark from a letter that Lagrange had sent to him: "The reconciliation of methods that you are engaged in contributes to their mutual clarification, and that which they have in

common most often conceals their *true metaphysics*: here is why this metaphysics is *almost always the last thing that one discovers*."[35]

Cohen remarks that the goal of the Lacroix-Lagrange program described above is typical of a number of "French attempts" to "clarify this metaphysics" of the methods of the differential calculus. Cohen includes Carnot, whose analysis of the concepts of identity and of equality he uses above to great effect, in this group. The methods and arguments used by Carnot, Lacroix and Lagrange were used in the context of finding a secure foundation for mechanics. For instance, Lagrange uses the infinitesimal method in his *Mécanique analytique*, and Carnot used mechanical principles in his attempts to ground the infinitesimal calculus.[36] As much as Cohen recognizes this historical fact, he is ultimately much more interested in the use of function theory to give a general *conceptual* foundation for the calculus.[37]

In particular, as I will show in this account, Cohen takes his cue from Cournot in arguing that a chief value of Lagrange's function theory is that it allows us to demonstrate that the fluxion method and the infinitesimal method are practically (or "functionally") equivalent. Here Cohen's argument comes full circle. In my presentation of Cohen's account of Newton and Leibniz, above, I emphasized that the usefulness of studying these theories "from the abstract point of view of the critique of knowledge," for Cohen, is that the conflict between the theories is shown to be chimerical. From the point of view of function theory, Cournot argues, Newton's fluxion method and Leibniz's infinitesimal method are simply different paths to the same destination.

Cournot's argument that Newton's method is equivalent to Leibniz's appears in the *Traité élémentaire de la théorie des fonctions*. Here he defends the following claim:

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[35] *PIM* §72. The first quotation, from Lagrange, is from Lagrange 1797, §4, p. 3. The second quotation, from Lacroix, is from Lacroix 1810, p. XIX. Both translations are mine.

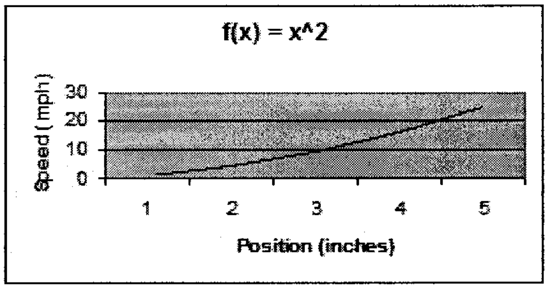
[36] See Gillispie 1971.

[37] This is what Frege missed about Cohen's book, in his very interesting review (Frege 1967 [1885]). Frege picks up on Cohen's historical emphasis on the influence of mechanics, but does not seem to recognize Cohen's ultimate argument, following Cournot, for the conceptual primacy of function theory. This argument will become clear in *PIM* §85, and should be manifest as well in my presentation of Cohen's argument.

Newton's conception [the fluxion concept] extends to any type of quantities, in the sense that one can always define and express, in terms of fluxions, the functions that we have qualified until now as derived, and whose relations with the primitive functions are the essential object of the theory that occupies us.[38]

There are two major components of this argument. The first is that any "derived" function of the calculus (which term I will shortly define) can be expressed in terms of fluxions. Cournot's second major claim is that these "derived" functions can then be expressed in terms of the primitive functions of function theory, which are the basis for the comparison of "any type of quantity," whether these be infinitesimals or fluxions.

A "derived function," in Cournot's terms, is a higher-order derivative. The easiest example is a second-order derivative. Take the function y=f(x), which assigns to each point on the x-axis a point on the y-axis. Take the specific function $f(x) = x^2$. Assume further that the x-values are a series of positions of a given object on a ramp in inches, whereas the y-values of the function are a series of speeds in miles per hour.[39] If we take the x-values to be from 1 through 5, inclusive,[40] the function will look like this:



The value of the function at one inch is one mph (1 squared), at two inches is four mph, and so on. Taking the first derivative of the function yields the value of the

[38] Cournot 1857 [1841], Book I, Chapter III, p. 66, my translation.

[39] It should go without saying that these values are purely fictional and are not meant to be accurate of any given object.

[40] That is, this would be a function restricted to the values [1, 2, ...5] on the real line, and the integral would be a definite integral. In metric units the x-values would be [2.54, 5.08, ... 12.7].

function (the rate of speed) at that point, that is, simply the y-axis value for any x-value. The *second* derivative of the function yields the *rate* of change of the function at any given point. This is the sense in which it is called a "second-order" derivative or function.

The law of continuity requires that variation of motion in time, that is, the second-order derivative, be uniform. This is the law to which both Newton and Leibniz appealed.[41] Cournot observes that if we take the derivative to be the primitive function, and the function itself as the derived function, then we can define the function in terms of its derivative. That means that we can prove that the first derivative, the tangent to the curve, is an increment generated by the function that generates the curve. That is to say, using the law of continuity, which shows that the increase in the second derivative of the function is uniform, we can prove that the point on the tangent that approaches the curve as limit, and the point on the curve that approaches the tangent, are the same point, namely, an infinitesimal increment. Cournot can give a foundation for the concepts of equality and identity used in the infinitesimal and fluxion methods, on the basis of the law of continuity and of the properties of functions.

Cohen takes over this approach from Cournot. Since Cohen's overall goal is to reveal the synthetic *a priori* basis for the calculus, he argues that the determination of higher-order differentials *a priori* is in fact conceptually more fundamental than the determination of the first-order differential.[42] The first-order differential is a finite value on the real line, that is, it is a static or scalar quantity. Cohen and Cournot argue that these quantities are produced by a series of functions on the higher-order derivatives. Cohen argues that Leibniz inaugurated this method by developing a

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[41] Cohen observes that, historically, Leibniz was among the first to use higher-order derivatives. Cohen explains this in §57, "The Orders of the Infinitely Small" (*Die Ordnungen des Unendlichkleinen*), in the context of a discussion of Leibniz's writings on mechanics, as follows: "It is thus not only an illustration, but an extension and a completion of the [differential] concept itself, which, one can perhaps say, is made possible through *its relation to geometry*. For through the geometrical significance of the differential is revealed the *higher orders* which, already in effect with the first differential in the *problem of tangents*, had then effected a clarification of the basic concepts of mechanics that, in the latter instance, were brought to the fore already with the first differential for mechanical reasons (*PIM* §57)."

[42] Here is the sense in which mechanics has a starring role in the development of the calculus, for Cohen.

strategy for taking a series of higher-order derivatives as the basis for a successive series of approximations. As Cohen cites Cournot,

> Thus, the infinitesimal method is not only an ingenious artifice: it is *the natural expression of the mode of generation of physical quantities* which grow from elements smaller than any finite quantity…From this perspective, one can say with justification that infinitely small [*quantities*] *exist in nature*; and it certainly agrees with this manner of thinking to call f′x [*the derivative*] the *generative or primitive function*, and f(x) the *derived* function, instead of applying these functions in the reverse order, as Lagrange did, guided in this by purely algebraic considerations.[43]

The infinitesimal method is a way of generating first-order functions from higher-order functions. To explain this in terms of an earlier example, take d'Alembert's construction above. The equation for the parabola was $ax=y^2$. It is possible to find the derivative of the function, or the value of the function at a given point, by first finding the second-order derivative, then the third, and so on.[44] These higher-order functions are functions on quantities smaller than any finite quantity. They represent rates of change of the function, and describe the behavior of the *function* rather than of any given quantity. By a series of functions on these quantities, we can arrive at an approximation of a finite quantity so close that the error term is smaller than any given quantity. The conceptual foundation for this method follows Cohen's argument above, that unified (finite) quantities are *derived* from the more primitive functions on the infinitely small. The infinitely small quantities are variable or temporal quantities, which as Cohen argues are *intensive* – they have a degree, that is, they vary continuously. Rate of change, as described by the second derivative, is a paradigm of intensive quantity in Cohen's view. (I will discuss Cohen's account of intensive quantity in more detail below.)

Cohen concludes that Cournot is able to determine a synthetic relation between space and time *a priori*, *without* assuming anything regarding the sort of quantity involved. As Cournot points out, higher-order differentials do not discriminate

[43] *PIM* §85.

[44] Cohen explains how Leibniz does so in §57.

according to quantity. Fluxions or infinitesimals alike are subject to the same laws regarding rates of change, the law of continuity. Cournot's and Cohen's arguments regarding the primacy of function theory for the foundation of the calculus has the effect of "relativizing" the quantities used by Newton and Leibniz; these quantities are *derived* from the more fundamental activity of determining the first-order differential on the basis of higher-order derivatives. Cournot observes that Newton's fluxion method (in abstraction from his method of first and last ratios) is in line with this way of conceiving function theory. Recall that "fluents" are quantities that vary over time, and a "fluxion" is the motion itself of that object. Fluxions are therefore variables that can stand in for second-order derivatives, as Cournot puts it:

> Newton gave the name of *fluents* to the quantities x,y,... which are determined to vary with time, and the name of *fluxions* to the derivatives... which vary themselves, in general, with time, and which measure at each instant the rate of change of the fluent quantities....The fluxion x° will be the speed itself of the moving point, at the instant that one considers it: the word *speed* being taken here in its primitive sense, which is also the most common.[45]

Newton's fluxions can be used analogously to the higher-order differentials characteristic of the infinitesimal method. As long as we can show that both methods are based on the same primitive and derivative functions, Cournot remarks, there need be no significant consequences to choosing one over the other. Thus we can finally arrive at a foundation for the use of both methods, which at the same time gives us a clear conception of the kind of quantity that, in the early imprecise formulation, is "smaller than any given quantity." In his role as "philosophical historian," Cohen has been able to settle the debate between the two rival methods, and to show the secure conceptual foundation they have in common. Further, Cohen argues, this conceptual foundation is found in the law of continuity, which is the basis for calculation by means of higher-order derivatives.

Cohen's argument here might appear to overlook a crucial requirement for the foundation of the calculus, namely, the precise arithmetical definition of the notion

[45] Cournot 1857 [1841], 65-66.

of continuity. The notion of continuity at the foundation of the calculus was put on a more secure mathematical footing during the 19th century by, among others, Richard Dedekind and Georg Cantor. Dedekind remarks:

> The statement is frequently made that the differential calculus deals with continuous quantities, yet [*at the time in 1872*] an explanation of this continuity is nowhere given; even the most rigorous expositions of the differential calculus do not base their proofs upon continuity but they either appeal more or less consciously to geometric considerations or to representations suggested by geometry, or they depend upon theorems which are never established in a purely arithmetical manner.[46]

In the *Principles of Mathematics,* Bertrand Russell criticizes *PIM* in this regard. While the historical account in *PIM* is excellent, Russell says, Cohen's account hinges on the spatial intuition of magnitudes and not the arithmetical determination of quantity. Russell distinguishes between (geometrical) magnitudes and (arithmetical) quantities, in the same spirit as Dedekind's remarks above distinguishing geometrical and arithmetical proofs. Russell claims that this distinction is "wholly foreign to Cohen's order of ideas."[47] Rather, Russell claims, Cohen belongs to the class of philosophers who rely on the old, geometrical notions of continuity rather than the newer, rigorous arithmetical proofs. In particular, Russell alleges that Cohen's Kantianism leads him to rely on "pure intuition" rather than on mathematical or logical reasoning:

> Cohen begins by asserting that the problem of the infinitesimal is not purely logical: it belongs rather to Epistemology, which is distinguished, I imagine, by the fact that it depends upon the pure intuitions as well as the categories.[48]

Russell implies that this dependence on "pure intuitions" is the basis of the Kantian nature of Cohen's work.[49] Russell concludes that, for Cohen, the process of

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[46] Dedekind 1996 [1872], 767, trans. W. Beman, trans. rev. W. Ewald. The extent to which Cohen was familiar with the arithmetization of the calculus is a somewhat vexed question in the literature. Dedekind and Weierstraß do not appear in *PIM* at all. Cohen does cite Cantor, as well as du Bois-Reymond, but in a quite different context (*PIM* §91).

[47] Russell 1938 [1903], 341.

[48] *Ibid.* 339.

obtaining the differential from "numbers alone" must involve the "pure intuition" of time.[50]

The initial error of requiring pure intuition, Russell observes, leads Cohen into a more fundamental error, namely, assuming that we must presuppose infinitesimal quantities in order to have any representation of change in time. Russell combines two elements of Cohen's argument to reconstruct the account in *PIM*. First, Russell claims that Cohen relies on Leibniz's (archaic) terminology dx/dy for the differential.[51] Russell argues that when Cohen constructs his philosophical arguments about continuity, for instance, he uses Leibniz's outmoded mathematical theory as a template. Then Cohen compounds this initial error, Russell says, by arguing that any mathematical definition of continuity must be subject to a prior philosophical definition that depends on pure intuition, which is by nature geometrical reasoning.[52] Russell concludes that Cohen's account stands in the way of mathematical progress by requiring an elaborate philosophical apparatus that puts undue restrictions on mathematical theories.

It is well beyond the scope of my account here to determine whether Russell is correct. However, it is possible to raise a few questions about his story. Russell's account of Cohen is painfully compressed, as he himself admits: "For the sake of definiteness, I shall as far as possible extract the opinions to be controverted from Cohen."[53] Russell's extractions from Cohen certainly include many major elements of Cohen's account, but he leaves others out, such as the discussion of Cournot's function theory. One might argue that, as a result, Russell's account is inaccurate, or at least incomplete. Worse, Russell seems wholly insensitive to the difference between Cohen's historical account and his systematic one. For instance, Russell argues that "when we turn to works such as Cohen's, we find the dx and the dy

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[49] I must leave aside the important, but involved, question of whether this is an accurate account of Cohen's argument. There are good grounds to conclude that it is not.

[50] Russell 1938 [1903], 339.

[51] *Ibid.* 338.

[52] Russell 1938 [1903], 339 §317, 344-5.

[53] Russell 1938 [1903], 339.

treated as separate entities, as real infinitesimals."[54] Russell argues that it is better to use the notation f'x for the differential, which "is more similar to Newton's y°, and its similarity is due to the fact that, on this point, modern mathematics is more in harmony with Newton than with Leibniz."[55] The perhaps unnecessarily exhaustive account of Cohen's reading of Newton and Leibniz, above, in which Cohen ends by arguing that Cournot's use of f'x is preferable, should cast at least some doubt on this accusation of Russell's.

Perhaps Russell's most compelling objection has to do with what Russell takes to be Cohen's assertion that the notion of continuity is best defined as it is embedded in theories of motion, or change in time. As Russell observes,

> Change in time is a topic...which has, undoubtedly, greatly influenced the philosophy of the Calculus. People picture a variable to themselves—often unconsciously—as successively assuming a series of values, as might happen in a dynamical problem. Thus they might say: How can x pass from x_1 to x_2, without passing through all intermediate values?...Everything is conceived on the analogy of motion, in which a point is supposed to pass through all intermediate positions in its path.[56]

Russell argues that there is no compelling mathematical reason to suppose that the real line, for instance, must be continuous in the same way that one could imagine motion to be continuous. In fact, Russell considers that he, Russell, has given adequate proofs to the contrary in the *Principles of Mathematics*. As a result, Russell argues, Cohen's argument that the calculus requires Kantian "intensive quantities," which are continuous on analogy with motion, is simply incorrect. Worse, these arguments saddle mathematicians with an unnecessary and unwieldy philosophical apparatus.

However, one might respond on Cohen's behalf that Cohen's reasoning about intensive quantity raises interesting epistemological questions. In particular, Cohen's account can be read easily as an analysis of whether or not reality is continuous, or

54 *Ibid.*

55 Russell 1938 [1903], 338.

56 Russell 1938 [1903], 344.

whether certain objects or phenomena (such as motion) are really continuous, rather than an argument that mathematicians must be compelled to reason with continuous quantities.[57] This is a question about whether or not certain theories of mathematical physics are applicable to empirical reality.[58] In what follows, I will briefly sketch an alternative view to Russell's, one that shows how Cohen's appeal to Kant's Anticipations of Perception can be made somewhat more persuasive.

Cohen's arguments are meant to show that the calculus applies to real natural processes. In this context, Cohen's aim is "[t]o prove by the critique of cognition that *the missing logical foundation of the differential concept on a basic principle of the critique of cognition, and indeed on that corresponding to the category of reality, [...] is contained in the basic principle of intensive quantity in [Kant's] Anticipations [of Perception].*"[59] Cohen's argument is that any conceptual foundation of the differential or limit must take into account the fact that the quantities involved must be "intensive" quantities to be applicable to real objects and processes. From the beginning of his career, in the essay on the Trendelenburg-Fischer debate, Cohen has argued that Kant's philosophy is meant to be the foundation for the *application* of mathematics to real natural processes, such as those described by Newton's laws. As such, Cohen argues that Kant's epistemology gives the basis for applying the pure laws of thought to real phenomena, the paradigm case of which is the conceptual reasoning behind Newton's laws of nature.

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57 Indeed, as Alan Richardson points out in a review of this dissertation, Cohen refrains from making any remarks about concerns within mathematics itself, something that should make one pause before arguing that Cohen is putting undue restrictions on mathematicians.

58 For instance, Peter Smith has raised similar questions about whether or not a theory that only approximates reality can be true. He raises the question in a context that would be directly relevant to Cohen's reasoning, namely, a discussion of classical fluid mechanics. Smith asks, what if the classical theory takes it as axiomatic that fluids are continuous, but we know from the empirical data that they are atomic? What if the theory cannot be revised to reflect this reality? What, then, becomes of the notion of a scientific theory reflecting the truth about reality? Smith argues that we may then coin the phrase "approximate truth" to describe the relation between such theories and the available data: "Thus a theory may still count as approximately true, by any sane standards that respect what we ordinarily say outside the philosophy seminar, even if it stubbornly resists revision. Theories developed within the framework of classical fluid mechanics provide obvious candidates. Such theories cannot be made strictly true by fine-tuning, for the classical framework embodies the essential axiom that fluids are perfect continua—and no piling up of added epicycles is going to cancel that axiom and so deliver a theory which *is* strictly true of real, atomically granular, fluids" (Smith 1998, 254). I cite this example to illustrate that these questions, directly relevant to Cohen's account, are still debated and are still interesting to philosophers of science.

59 *PIM* §18.

In this case, the law behind the calculus is that of continuity. Cohen has shown, through his critical history of the development of the calculus, that the foundation for mathematics requires the revision of the concept of continuity. He argues that Kant's analysis of "intensive quantity" is the necessary foundation for those revisions.

In Book II of the *Critique of Pure Reason,* in Chapter Two, the Analytic of Principles, Kant gives a "Systematic Representation of All Synthetic Principles of Pure Understanding."[60] There are four principles in his Table of Principles: Axioms of Intuition, Anticipations of Perception, Analogies of Experience, and Postulates of Empirical Thought. The first two principles Kant calls "mathematical," and of them he says: "they allow of intuitive certainty, alike as regards their evidential force and as regards their *a priori* application to appearances."[61] The Axioms of Intuition are based on the principle that "all intuitions are extensive magnitudes."[62] An extensive magnitude is a discrete, finite quantity. The Anticipations of Perception are based on the principle that "[i]n appearances, the real, that is, an object of sensation, has intensive magnitude, that is, a degree."[63] Cohen appeals to the notion of "intensive magnitude" when discussing continuity. (I will restrict the following discussion to Cohen's views, as a detailed discussion of Kant's account is beyond the scope of my account here.) As Russell complains, intensive magnitude for *Cohen* is directly related to a notion of continuity analogous to continuous motion. Russell argues that motion need not be considered as continuous, mathematically speaking, but could well be discrete. According to Russell, Cohen reasons, on the basis of "pure intuitions" of space and time, that since space and time as media are continuous, the objects in them must be continuous also. Russell objects that this is to put the cart before the horse. Infinite series need not be continuous, and indeed, the fact that a series has a limit does not at all imply that the series is continuous in Cohen's sense.

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[60] In the Akademie pagination of the *Kritik der reinen Vernunft* (hereafter *KrV*), Book II Chapter II begins on A148 / B187. These translations are from Kemp Smith.

[61] Kant, *KrV*, A161 / B201.

[62] *Ibid.* A162 / B202.

[63] *Ibid.* A166 / B207.

A possible, more sympathetic reading of Cohen might have it that Cohen is raising the philosophical question of whether the mathematical reasoning at the basis of a physical theory need have implications for the truth of that theory, that is, for whether the theory applies to real objects. As I presented it in Chapter Two, for Cohen, epistemology has to do with the relation between theories and the empirical facts, and ultimately, between theories and reality. Cohen argues that behind the distinction between extensive and intensive quantity in the System of Principles is Kant's essential distinction between the totality of independently existing objects and the totality of facts of which we have scientific knowledge. This distinction, Cohen observes, allows Kant to give a critical, idealist definition of the category of reality, according to which reality is a presupposition of thought:

> Herein consists the new thing that Kant has to teach us: Reality is not in the crude [material] of sensible discovery, and also not in what is pure in sensible intuition, but rather must be given validity as a particular presupposition of thought, like substance and causality, as a condition of experience that can only be removed insofar as it lies at the ground of [experience] and is presupposed for its possibility. Hence Kant had to distinguish reality as a special category distinct from actuality.[64]

Cohen argues, then, that reality is a "presupposition of thought," a "condition of experience" that "lies at the ground of [experience] and is presupposed for its possibility." Reality is a *relation* between intuition and thought, Cohen claims. Reality cannot be reduced to the "pure" element of either intuition or thought, since that will deprive it of its irreducibly relational character. Thus neither "pure intuitions" of (perhaps continuous) space and time, nor pure reasoning about (perhaps discrete) number, will determine whether a theory applies to reality or not.

Here is an issue that was present in embryo in the Trendelenburg-Fischer debate: the question of whether the pure intuition of space is wholly independent of the laws of thought, or is linked to them a priori. Here Cohen gives his mature answer to the question: in the *Aesthetic* space is being treated with "the abstraction belonging to the critique of knowledge," and so it can be abstracted away from its connection with

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64 *PIM* §18.

thought. In the Axioms of Intuition and the Anticipations of Perception, space must necessarily have a connection with time, and therefore thought: for the Axioms and the Anticipations deal with *reality*. Reality, for Cohen, necessarily requires a *relation* between intuition and thought. While the separate tools of intuition and thought can be sharpened, each in the absence of the other, the application (or in Kant's terms, "Schematism") of the category of reality always requires a relation between intuition and thought.

Kant's account of this claim is not clearly presented, Cohen admits, since he couches it in terms of "pure intuition," a concept with which Cohen is not comfortable. Cohen rejects any idea that intuition of unity, on any level of abstraction, can be a reliable source of judgments about reality per se. I will cite his remarks in full:

> Whereas Kant does not always present the distinction between *pure intuition* and *empirical intuition* with assurance, or with systematic prudence, the exposition of the first Principle in the *second* edition of the *Critique*, like the remarks of the Transcendental Aesthetic, is distinguished by a keen precision. The determination of a pure sensibility requires the acquisition of a *pure intuition*, preserved by analogy with every form of sensation. The space that is the object of a *pure* intuition is not sensed. This interpretation of the psychological puzzle of space, given from the point of view of the critique of knowledge, settles all the discussion about the *representation* of the space which, certainly, recalls, as such, the element of sensation and is founded on it. Thus pure spatial intuition is an element of abstraction belonging to the critique of knowledge (cf. §21), an element elaborated in connection with thought to become the first mathematical principle. Thus is justified the fact that one *avoids sensation* in evaluating intuition and its apriorization.[65]

Here we can focus on the key claim "Thus pure spatial intuition is… an element elaborated *in connection with thought* to become the first mathematical principle" (*emphasis added*). An essential element of Cohen's argument about the infinitesimal method is that its foundation requires us to subordinate it to the law of continuity.

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[65] *PIM* §78.

Cohen argues that the law of continuity is a law of thought. Cohen points out that the advantage of Kant's system over Leibniz's is that all elements of the *a priori*, for Kant, are made relative to the *unity of consciousness*. Here he says:

> We have always taken into account the *fictive* element that resides solely in intuition, as much as the calculus and measure rest on it. Nonetheless, it is only a *comparison* to which one comes with this hypothetical unity, according to which all inchoate quantity is established and distinguished. But what else should this unity itself be, but the unity that produces and secures absolutely everything? All relations and all objects must necessarily be referred to *the unity of consciousness*. And it [*this unity*] is present in each principle, for it is, as transcendental apperception, the résumé, the generic expression of all the particular types of categories and principles. It is, certainly, equally present in the first principle of quantity [*the Axioms of Intuition*], but the last [*the Anticipations of Perception*] demands to be completed. In effect, quantity is precisely not simply extensive or intuitive quantity.[66]

Cohen has argued that if the concepts of identity and equality are based on the law of continuity, as he has shown they can be, then on Kant's grounds they are necessary for the unity of thought. The employment of the notions of identity and equality in the calculus should not be justified by sensible perception that two points coincide, Cohen argues. Rather, one ought to be able to demonstrate equality or identity by means of a demonstration in thought, that is, geometrical reasoning *a priori*. Cohen argues that the foundation for the claim that a point on a line tangent to a curve and the point on the curve itself are identical is the fact that the laws of thought (in particular, the law of continuity) require us to so conclude. Cohen concludes that representations of real natural processes are subject to the laws of thought.

[66] *PIM* §78.

CHAPTER FOUR

Cohen's History and Philosophy of Science

4.1 Cohen's mature theory of science

In his *Introduction* to Friedrich Albert Lange's *History of Materialism*, Cohen gave the mature, systematic statement of his theory regarding the relation between philosophy, history and science, a topic that would become a primary and characteristic preoccupation of his philosophy (and of the Marburg school).[67] Here Cohen begins to review and to reformulate for himself the position as "philosophical historian" that he had first taken up in commenting on the Trendelenburg-Fischer debate. In the first section of the Introduction Cohen lays out three founding elements of his philosophy, one a statement and the other two questions:

1. Science exists.
2. What is science?
3. What is cognition?

The relation between these three statements is a central problem of Cohen's philosophy. Cohen identifies science with rational inquiry, or more precisely, with that subset of rational inquiry that leads to cognition of *facts*. For Cohen, the question "What is science?" turns out to be identical to the question "What is cognition [in general]?" The problem of Cohen's critical philosophy is then to *explain* the fact that science exists by, first, taking the historical facts of science into account, and second, by providing a philosophical grounding for science, that is, for cognition. Cohen argues that epistemology and science are part of the same endeavor: all science is cognition, all cognition is scientific, hence the act of critique is a scientific act and vice versa.

Lange's project was (in part) to show how we can give a naturalist account of reality and yet reserve a role for ideas, where these are the contributions of *reason* to our conception of material reality. For as Lange argues, human beings are part of material reality and their ability to create new concepts, for example, can form part of

[67] The *Einleitung mit kritischem Nachtrag zu Friedrich Albert Langes Geschichte des Materialismus*. Cohen 1928 [1914] in the bibliography, hereafter cited as *Einleitung*. All translations from this work are my own.

a naturalist explanation. Lange used the term *Erkenntnislogik* to describe his method of evaluating the contribution human cognition makes to material reality.

Cohen retains Lange's view that the job of philosophical critique is to explain (as he says, give a grounding or *Grundlegung* for) the role of cognition with regard to the material facts of culture, of science, and of history. He agrees with Lange that the proper method is to begin with the facts themselves, and then to apply a method of reconstruction that demonstrates the role of cognition in *producing* the facts. Thus a central problem for Cohen is to explain how ideas and concepts are formed, namely how we come up with those ideas that result in cognition (scientific knowledge). This is where the philosopher goes beyond his role as recorder of history.

Lange limits his critique of cognition to a description of the formal *logic* of cognition. Thus he explains the formation of new concepts as psychological exercises, in terms of the brain making new connections in response to certain stimuli, for example.

Cohen wanted to solve the problem of accounting for concept formation in the critique of cognition by distinguishing between the *logic* of cognition and its *method.* Here is where Cohen steps away from Lange, Fischer and Trendelenburg, and argues that the evaluation of history is not simply a matter of logical analysis but also a matter of creating a new discipline in which the act of creating a new theory or concept would itself be the matter under discussion.

For Cohen, the philosophical historian should tackle the problem of how to reconstruct the process of concept *creation.* In that way, the historian can account for the progress of science, for those facts that are in fact innovations, by showing how and to what extent the ideas used by the scientist in pursuing a conclusion were "realizing ideas."

The role of the *Erkenntniskritisch* historian is ultimately to search for realizing ideas. For Cohen the *a priori* (the conditions of cognition) is not universal and necessary. Rather, such concepts, intuitions, principles and laws are relative to the search for productive ideas or principles, the uncovering of which is the true goal of analysis.

Cohen's mature theory of science is a final, decisive step away from the *Erkenntnistheorie* of his youth. The attempt to reduce logic to a set of analytic propositions that describe empirical science is replaced by a thoroughgoing scientific idealism. In what follows, I will outline the basic framework of this idealism, which is the late form of the *Erkenntniskritik* that Cohen introduced in *PIM*.

Cohen objects to the reduction of epistemology to logic, where logic is conceived as a set of analytic propositions describing the results of the empirical sciences. He argues that this form of epistemology obscures the deeper answer to the question, "Was ist Wissenschaft?" and hence to the question of the justification of scientific knowledge. The concepts of empirical science are not justified *per se*, Cohen observes. The concepts of empirical science are not fundamental to that science, but are derived from experience, experiment and deduction – and, Cohen remarks, from basic principles of theory and concept construction. Further, in many cases particular demonstrations or deductions are backed up by a set of axioms. In that case, Cohen asks, can we make a principled distinction between scientific concepts and axioms?

> Do...axioms have the same value that concepts do? The question leads us to another question: Do *ideas* have the same denotation as *concepts*, or do they designate, perhaps, something more like axioms? *In general, what distinguishes axiom from concept?* What lawfulness of thought [*Recht des Denkens*] does an axiom possess? This question is the deeper significance of the general question: What is science? [68]

Cohen isolates axioms as having a "Recht des Denkens."[69] For Cohen axioms, or ideas, are the elements of a scientific theory constructed by pure thought. Thus the justification for their use must come from the "Recht des Denkens" that lies within them. In this sense axioms are strictly distinct from the concepts of empirical science, which are justified by their relation to experience.

By contrast, for Cohen axioms are justified by the fact that they are hypothetical. They are pure products of thought that do not require empirical proof. For Cohen, an axiom is a proposition, specifying certain formal relations, that lies at the

[68] *Einleitung* 186-7.

[69] This way of speaking is redolent of Kantianism: the Transcendental Deduction of the Categories, for instance, seeks to give a *deductio iuris*, or justification of our quasi-legal *right* to apply concepts that are pure products of thought.

foundation of a rational process.[70] Cohen urges that the changing role of the axiom, especially in geometrical theories, has historically contributed to refine and clarify "the relation between basic principle and theorem." In particular, the axiom "is shown as the grounding for all the theorems[71] that can be deduced from it."[72] Cohen gives a definition of "axioms" here as the basis in thought for all the conclusions that can be deduced from them. These conclusions, he says, are based on a hypothetical structuring of reality through the assumption of certain ideal relations. Cohen argues that axioms are the grounding [*Grundlegung*] for the conclusions and propositions one can derive on the basis of a given scientific theory.

There are two aspects of Cohen's argument here that are relevant. First, for Cohen axioms are ideas. They are constructed by a pure thought process.[73] Cohen seems to have the view that axioms are in fact a way of *arranging* the content of a theory, without necessarily changing that content substantially. However, different arrangements can bring different configurations of facts to view. Thus, the axioms are also the intellectual prerequisites for the deduction or observation of those facts. This double meaning of scientific axioms distinguishes the logic of science that Cohen identifies from the logic that rests only on the comparison of scientific concepts. He makes this point in to the context of a discussion of Plato's idealism:

> Pure thought, which is consequently scientific thought, produces the fundamental principle by grounding itself. Hence pure thought is the legitimate medium for the development of the Idea. *The most profound basis of the Idea itself is nothing but a grounding [Grundlegung].* That is the sense of "Idea" that Plato strives to develop, from his first mature dialogues... *This sense of the Idea as Hypothesis distinguishes Idea from concept...* Idealism is based on the logic of the Idea.[74]

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[70] To take an example that has particular importance for Cohen, the Peano axioms are the basis for the construction of the natural numbers.

[71] I have translated "Lehrsatz" here as "theorem," it could also be translated "proposition."

[72] *Einleitung* 187.

[73] Cohen's account of concept construction is found in his *Logik der reinen Erkenntnis*.

[74] *Einleitung* 188-9.

Cohen traces the use of ideas as hypotheses to the theories of Galileo and Kepler, in fact, he says: "To understand the idea as hypothesis thus means to understand it in its *historical* rejuvenation through Galileo and Kepler."[75] However, he sees the culmination of this method in Kant's synthetic *a priori*. Cohen claims that Kant's account of the a priori is continuous with (what he takes to be) Plato's method of taking ideas as hypotheses. Both techniques mean only that human beings create certain ideas or determine certain relations *sui generis*, and that only these self-created relations can be known *a priori*. On the basis of a "philological" argument (which it is beyond my competence to evaluate), Cohen claims that both Plato and Kant take the *a priori* to be the *foundation* for our knowledge, and to be hypothetical:

> Thus it is in no way a leap from Plato to Kant, when Kant explains his *a priori* by saying that we only know that in the things *a priori* which we ourselves put into them. If he says elsewhere: that which is *a priori* lies at the foundation [*Grund*], here he says: We ourselves put it at the foundation. And in Greek that means: we make the hypothesis. That is linguistically correct. And it is also historical; for this ground-laying thought is the fundamental concept [*Gedanke*] of scientific world history.[76]

Cohen concludes that the proper basis for an epistemology is not the logic that evaluates the concepts of empirical science, but rather the method that uncovers the purely formal and self-constructed hypotheses behind our scientific propositions. Once one has isolated these hypotheses, Cohen will argue, it is possible to clarify the use of basic concepts in their construction, for instance. Thus once one has identified the hypotheses behind a theory, one can prove results about their use: for instance, that they are logically valid or invalid, or that they are in conflict with the axioms of another theory. These hypotheses and their evaluation are, for Cohen, the true subject matter of epistemology.

Cohen's epistemology depends, then, on isolating the hypotheses hidden behind scientific propositions and conclusions. His method for doing so depends on a distinction that was becoming more familiar in the philosophy of science of the

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75 *Einleitung* 197.

76 *Einleitung* 198.

period: the difference between *description* and *explanation.* There are many ways of interpreting this distinction. Cohen parses it in several different ways within his own theory, depending on the subject matter under discussion. In what follows I will outline these distinctions briefly and show how Cohen makes use of them.

At a general level, Cohen takes the difference between description and explanation to be the difference between the contributions of pure mathematical thought to natural science and related, but distinct, disciplines such as morality, religion and (non-philosophical) history:

> The triumphs that mathematical thought [*das Denken in der Mathematik*] won for natural science were all too eagerly celebrated in other arenas, the contribution of thought to which has only the name [*of "thought"*] in common with pure scientific thought, which develops effective methods and, thereby, science itself. Behind this ambiguity rest two types of philosophy, which converge in each philosophizing person. For the use of unscientific or insufficiently methodical reasoning is not the only reason the concept of thought is expanded, is blunted, and finally, becomes ambiguous. Other problems of the intellect [*Geist and Gemüt*], which provoke people's intelligence and command a solution to their puzzles in the same way, are also present next to those of natural science. Descartes and Leibniz would and could not restrict their efforts to the mathematics of nature—they also cared about the history of human existence in particular. Thought had to remain, or become, a methodological tool for the questions of *morality* and *religion* as well.[77]

Cohen sees the historical source of the resolution of this problem in *Newton.* Newton resolved the above problem of the duality of philosophical thought by becoming the great *systematizer* of philosophy. If philosophy itself could be considered a system that explains and does not simply describe the phenomena, then we no longer need metaphysics or dialectic – hypotheses of various sorts. In other words, Newton was able to resolve the description/explanation dilemma by making philosophy (of science) *itself* a closed explanatory, and not just descriptive, system.

[77] *Einleitung* 235.

Newton thought he had no need, Cohen observes, to go outside the system of philosophy, to appeal to morality, religion or history to find a more general explanatory framework for the scientific facts he uncovered.

However, Cohen observes, Newton found himself facing a difficulty with regard to the principles of philosophy itself. Here Cohen identifies a second set of more specific distinctions between description and explanation: the difference between mathematical principles and the principles of natural science, and the difference between natural science, mathematics, and philosophy itself:

> Newton determined the mathematical principles of natural science: but are these the only ones? Newton borrowed the principles of natural science from mathematics, and called the former natural philosophy. In this baptism, which remains the standard English usage, the problem is struck dead. Insofar as philosophy is taken to be identical to science, it is cut off instead. Philosophy is stifled when it is not recognized as a method, but only as the most general result of a method. *It belongs next to mathematics, as the method* that should complete the method of mathematics, if natural science is to result from their collective efforts. Newton's positive and negative importance consists in this twofold position on the basic questions... He achieved and completed the highest aim of philosophical efforts, in the narrow realm of science: the system. But the foundation of principles, on which he erected it, is incompletely defined—moreover, the error reaches to the basic laws.[78]

Cohen argues that Newton's confidence in his own philosophico-scientific cosmology led to two errors on his part. First, he made mistakes about the basic concepts of science, such as heavy matter. Second, he missed the true significance of such "hypothetical" theories as the *Undulationstheorie.* Cohen observes that the Leibnizians, inspired by Huygens and Descartes, were able to correct these mistakes by identifying "a new relationship between philosophy and science," based on mathematics and logic as the foundation for philosophical claims.[79] However, as he

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[78] *Einleitung* 237.

[79] *Einleitung* 237.

remarks in the discussion of Leibniz in *PIM*, Cohen does not think this new relationship is sufficient as a foundation for epistemology either.

In this context, Cohen sees Kant as more of a Newtonian than a Leibnizian, and thus as someone who was able to systematize natural science. Moreover, he argues, Kant was able to show that the "apriority" of mathematics is different in *kind* from the "apriority" of natural science, and to achieve the "separation of basic principles" that Newton had not:

> *Kant*, who in fact was trained more as a Newtonian than as a Leibnizian, started from Newton's double relationship to the philosophical criteria for science. This part of Kant's background remained decisive for the construction of his system, although he succeeded in [constructing it] only in later years. Above all, [Newton's influence was felt] in the selection of basic questions with which the *Critique* begins, [and] in the *separation* of the question of the apriority of mathematics from that of the apriority of natural science: as if mathematics meant anything to him outside its methodological relationship to natural science. The dependence on Newton had an effect on this opposition. Even mathematics should not be recognized exclusively as a method for natural science. So it happened that mathematics was not evaluated as a method, but as an independent science, as a self-sufficient synthesis *a priori*.[80]

Cohen does not think that this methodological separation means that Kant sees mathematics as wholly separate from its use as a method for natural science. However, Kant saw mathematics as well as a self-sufficient [*selbstgenügsam*] science, which consists of a synthesis *a priori* based on pure intuition. Cohen objects that this separation of mathematics from its status as *method* for natural science, while it is useful for epistemological purposes, leads Kant away from the true significance of geometry. Cohen sees the justification for geometry as based in pure *thought* rather than in pure *intuition*. In this sense, he claims, the "new geometers" such as Helmholz

[80] *Einleitung* 239-240.

are truer Platonists and Leibnizians than Kant was, because they take "the results of geometry in their relation to pure thought," rather than pure intuition.[81]

Cohen does not believe, however, that it need divorce him from Kant-scholarship in general. As in *PIM*, Cohen argues that Kant's most persuasive account of the relation between intuition and thought comes, not in the Transcendental Aesthetic, but in the System of Principles, and in particular in the Axioms of Intuition and the Anticipations of Perception.[82] Here Cohen explains his own fundamental conception of the relation between description and explanation: the basic relation of epistemology is that between intuition and thought. The goal of epistemology should be to isolate the relations of pure thought implicit in scientific propositions, that is, logical relations (such as axioms) and "pure" geometrical relations (function theory, for instance). In that way, Cohen argues, the contributions of pure thought can be isolated from the data of observation, for example. However, as we saw above, for Cohen pure formal relations are the grounding [*Grundlegung*] or foundation for any observation or conclusion that can be drawn from them. So if one does identify an axiom or geometrical function at the foundation of a given theory, that formal relation will be part of an *explanation* of the facts revealed by that theory.

The novel aspect of Cohen's account of explanation is that, while he thinks with Newton that all explanations are based on fundamental principles, Cohen also thinks that these principles are *hypothetical* and *relative*, that is, that they are methodologically equivalent to each other.[83] An explanation of a given phenomenon can in theory be given on the basis of *any* philosophical basic principle.

Cohen's method for the history and philosophy of science depends, in the first instance, on isolating the formal hypotheses at the foundation of a given theory. However, once isolated these hypothetical elements become the subject matter for conceptual analysis. Cohen uses the analysis of the fundamental concepts of a

81 *Einleitung* 238. This rejection of pure intuition is among the most fundamental of Cohen's moves away from what was known as "scholastic" Kantianism.

82 *Einleitung* 238-9.

83 This is a doctrine Cohen held in *PIM* as well: *PIM* §13 is entitled "Methodische Gleichwertigkeit der erkenntnis-kritischen Grundsätze."

scientific theory to establish the basic principles on which it was constructed, and thus, to logically evaluate how the theory was constructed:

> Now insofar as we appeal, finally, to *logic*, insofar as we come back to logic as the grounding of all those elements of a philosophical system that call into question a particular type of law, we recognize immediately the significance of the principle: that the philosophy of all disciplines must be developed in concert with the *history* of philosophy…[84]

Where the *Erkenntistheorie* of his time sought to make logic into a science of the conceptual relations abstracted from representations, Cohen preferred to use the history of science as his source material. This step emphasized the *constructed* nature of scientific theories: for Cohen, scientific facts are structured beforehand and *founded by* the formal hypotheses made by scientific theories.

Given this orientation, Cohen found conflicts of scientific theories to be manifestations of conflicts between principles of reason. For Cohen, the debate between Newton and Leibniz could be settled only by identifying the true logical issue at its heart: the problem of giving a conceptual foundation for the differential and for the logical concepts of identity and equality. Until this philosophical and mathematical difficulty is resolved, Cohen argues, the conflict between theories cannot be resolved either.

In the *Einleitung*, Cohen seeks to resolve another dialectic of reason: "the ancient opposition between the *atomist* and the *dynamic* accounts of nature."[85] His further goal, in resolving this conflict at the level of ideas, is to show "that all true science always and for ever was and is nothing but idealism."[86] In the account of Cohen's philosophy of science that follows, I will explain how Cohen constructed a philosophical resolution of the "ancient opposition" between atomism and dynamism, based on his reading of Heinrich Hertz's *Prinzipien der Mechanik* and on his own conceptual foundation for the "infinitesimal method."

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[84] *Einleitung* 183.

[85] *Einleitung* 242.

[86] *Ibid.*

4.2. *Atomism and dynamism*

In *PIM*, Cohen addressed the conflict between Newton's and Leibniz's methods of the calculus by attempting a foundation for the concept of variable quantity, or the differential concept. However, a tension remains in Cohen's foundation for epistemology. In the *PIM* Cohen takes his cue from Leibniz's "mathematical" method, that of specifying a set of formal relations *a priori*. On the one hand, he argues that Leibniz wanted to constrain the mathematical science of nature within the boundaries of logic.[87] On the other hand, Leibniz also relied on the notion of force in philosophy, and there is a good deal of evidence to suggest that Leibniz thought the concept of force was necessary to give an explanation of the facts of natural science as well.[88] While the logical foundation for the new concept of variable quantity may have been given a secure basis by Cournot and others, that foundation, as Cohen presents it, does not yet extend to an analysis of the fundamental notions and principles of mechanics. Since Cohen intended his *Erkenntniskritik* to be an epistemology, he needs an account of how a mathematical technique (the infinitesimal method) can be the logical foundation for mechanics. Cohen tries to show that the determination of quantity through the differential concept is fundamental to mechanics. To do so, Cohen tries to demonstrate that accounts of the fundamental principles of mechanics based on fundamental notions such as force and mass do not resolve the logical problem of giving a principled foundation for mechanics.

Cohen identifies the conceptual confusion in the "ancient opposition" between accounts of natural processes based on atomism and those based on dynamism. Whereas dynamism attempts to achieve an "exchange of matter for force" as the fundamental principle of mechanics, atomism tries to make the concept of matter fundamental and force derivative.[89]

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[87] *PIM* §10.

[88] See, for instance, the discussion of Leibniz's early theory of force in Garber 1995, 279-80 and following.

[89] *Einleitung* 245.

> Now if we want to formulate a brief overview of the influence of idealism on the new physics, we must consider the ancient opposition [*Gegensatz*] between *atomism* and the *dynamical* conception of nature. Originally, and indeed also with the first conception of the atoms in *Democritus*, it was the pure thought of the *Eleatics* that produced this concept. Whoever does not immediately realize this for the concept of atom itself must see it with its correlative concept, that of *emptiness*. In both, a pure, rigorous thought-element is made the foundation of existence, the basis of reality, instead of sensible perception [*Anschauung*].... [T]his opposition to sensibility, and in its positive sense, this sovereign constitution of *thought* was never satisfactorily brought to a comprehensive expression even in ancient times, in the disputes over various positions.... And as in modern times atomism was unearthed again, it was its admiration of the materialist view... that allowed for its restoration. In the meantime, however, there was another concept at the center of theoretical speculation and work: the concept of *force* threatened that of the atom.[90]

This passage refers to a *conceptual* opposition between atomism and dynamism. The reason Cohen emphasizes the Eleatics' making a "rigorous thought-element" "the foundation of existence, the basis of reality," is that he wants to argue that this dispute has never really had to do with sensible perception. Rather, it turns on the clarification of concepts: the concept of atom, the correlated concept of emptiness or empty space (which, he implies, cannot be sensibly perceived), and the concept of force.

Before attacking this problem, Cohen engages in a brief historical excursion to explain the development of the conflict between dynamics and atomism. Cohen argues that the development of the concept of force determined the fate of the atomistic conception. Cohen claims that Epicurus saw atomism as the basis for materialism, and Aristotle saw atoms as the basis for the form of bodies that could

[90] *Einleitung* 242.

be perceived by the senses.[91] It was Galileo's account of acceleration, Cohen argues, that swung the pendulum toward dynamism:

> At the same time [*in Galileo's theory*] *nature* was no longer conceived as a being [*Seiendes*], that is, not as something that underlies the concept of motion in scientific investigation, but rather as an *embodiment of motion.* And the *origin* [*Ursprung*] of motion is always force [*Kraft*]. As there is no foundation for physics on that basis, any theoretical account of nature must be *dynamic*, and be, then, through mechanics. Insofar as natural science becomes physics, *atomism* must give way to *dynamism.*[92]

This passage suggests that, for Cohen, one cannot construct a foundation for physics on an account of what *underlies* motion (atoms, for instance), nor can one found physics on the *origin* of motion in force. According to Cohen, Galileo realizes that motion is fundamental to physics, and that, therefore, physics cannot be founded on force. Cohen observes that Galileo's arguments inaugurated a long-standing scientific orientation toward "dynamism," where dynamism for Galileo is the analysis of motion as fundamental to physics.

Cohen remarks that, at the beginning of the 19th century, there was renewed interest in the atomistic theory. This renewed interest springs partly from the advances of the "new chemistry." Cohen sees the "new chemistry" as the "*signature of the time*," insofar as it is the prototype for an explanatory scientific theory that reduces natural phenomena to the laws of physics. Cohen argues that the most "mature" theory that tries to unify chemistry and physics eschews atomism in favor of a new hypothesis championed by Michael Faraday:

> This application of chemistry to the principal problems of natural science, and the solution of the special problems of chemistry through the general principles of physics, this great step in the direction of which modern science tries to orient itself, had its mature source in *Faraday.*[93]

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91 *Einleitung* 242.

92 *Einleitung* 244.

93 *Einleitung* 244-5.

Cohen observes that Faraday tried to unify the research of chemistry, which attempted to describe natural forms, and that of the physics of electrodynamics, which tried to exchange matter for force. Cohen calls Faraday's theory a "doctrine of electricity" [*Elektrizitätslehre*], a reference to Faraday's fundamental experimental and theoretical work in electromagnetism. He argues that Faraday, in his attempt to reconcile chemistry and physics, has expressed the fundamental tension between atomism and dynamism in the conceptual foundations of natural science. Cohen sees the "Energielehre" as the inheritor of Galileo's dynamic foundation for science, through the agency of Faraday:

> Just as Galileo, who was acknowledged as well-schooled and creatively talented in philosophy as well as in mathematics, anticipated the decisive concept of mathematical natural science, so is Faraday the true pathfinder of the new period of natural science—despite his association of chemistry with physics in the doctrine of electricity, and despite his (inevitable, in this context) overcoming of the problem of sensible matter through the problem of force.[94]

Cohen sees the "overcoming of the problem of sensible matter through the problem of force," i.e., replacing matter with force as a fundamental notion, to be a dialectical move that changes the *terms* of the debate without resolving the real problem. Cohen thinks the conflict can be resolved only by a thoroughgoing reconciliation of the conceptual opposition between atomism and dynamism.

One 19th century reaction to Faraday's theory, and to the discovery of the principle of the conservation of energy, was to claim that energy is a fundamental concept of mechanics, instead of force. The proponents of this view were known as the energeticists, and the main figures were James Clerk Maxwell, Wilhelm Ostwald, William Rankine and Ernst Mach. Ludwig Boltzmann and the energeticists, famously Ostwald, will clash at a conference at Lübeck in 1895.[95] The ostensible reason for the debate is a conflict over whether light is a wave or a particle. However, as was

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94 *Einleitung* 245.

95 In my account of the debate I have profited a great deal from Robert Deltete's excellent paper (Deltete 1999).

observed at the time, the true point of contention was the larger theoretical framework, in particular, the conflict between atomism and energetics. In the following paragraphs I will introduce the basis for the conflict and show why it is relevant for Cohen's theory.

The disagreement at the level of theory, over the adequacy of the particular explanations given by the energetic theory or by statistical mechanics, is less important to us here than this question of the fundamental concepts of mechanics. Exceptions and counterexamples to previously unassailable "universal principles," such as the famous "Prinzip der kleinsten Wirkung" of rational mechanics and the principle of universal gravitation began to be uncovered. These seeming "principles" came to be seen as general propositions, or even hypotheses, that could come into conflict with each other. The speaker who inaugurated the Lübeck conference with a lecture on the state of energeticist theory, Georg Helm, argued that the debate at Lübeck was in fact another manifestation of such a conflict of hypotheses, more specifically between hypotheses about the fundamental notions of mechanics. The energeticists argued that all physical or mechanical reasoning should be in terms of mapping transformations of energy, whereas the key opponent, Boltzmann, argued that atoms and material substance were still fundamental notions.

Ludwig Boltzmann's most enduring work was in statistical mechanics, where he gave a probabilistic account of the relation between the behaviour of atoms and the properties of matter. In 1877, he was able to "decisively associate" the Second Law of Thermodynamics with probability.[96] Further, as Ernst Nagel has pointed out, "Perhaps the greatest triumph of probability theory within the framework of nineteenth-century physics was Boltzmann's interpretation of the irreversibility of thermal processes."[97] Both of these achievements were part of Boltzmann's overall aim, to reduce the description of the behaviour of matter to a set of equations describing the probabilistic motion of atoms.

Boltzmann did not object to the postulation of energy as a fundamental concept per se, but to the claim that energetics was inherently more clear, appropriate and

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[96] Daub 1969, 318.

[97] Nagel 1939, 355.

logically sound as an explanation than classical mechanics. In fact, Boltzmann argued, the energeticists' work was often confused and unclear. As Robert Deltete, a modern commentator, remarks,

> Boltzmann thought that the energeticists had often violated in their own writings the methodological posture they professed. In particular, they had imported special hypotheses and unwarranted assumptions into their discussions of the various forms of energy, the result being a conceptual structure much less satisfactory than the precise and clearly stated propositions of classical mechanics and thermodynamics.... Boltzmann regarded these as *ad hoc* maneuvers for which there was no empirical justification, designed solely to permit the development of energetic theory or to rescue that theory from conflict with experiment.[98]

Boltzmann encouraged Ostwald to give a paper at the Lübeck conference, hoping that the presence of major figures in atomism and energetics would stimulate debate. Ostwald presented a combative paper called "Overcoming Scientific Materialism," in which he asserted that "The actual irreversibility of natural phenomena proves the existence of processes that cannot be described by mechanical equations, and with this the verdict on scientific materialism is settled."[99] Ostwald's arguments, like Boltzmann's, had a double purpose: first, Ostwald wanted to show that energetics gives an adequate description of natural processes; second, Ostwald tried on that basis to argue for the claim that energy should replace force as a fundamental notion of mechanics.

Cohen's position on disputes such as that between Ostwald and Boltzmann is that the disagreement often results from a lack of clarity in basic distinctions, most importantly, between disputes at the level of principle (explanation) and conflict at the level of description. In the particular case of Ostwald and Boltzmann, Ostwald thinks that his stance on the fundamental notions of mechanics is the only foundation for a unified theory of physics. In the quotation from his speech cited

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[98] Deltete 1999, 58.

[99] "Die Überwindung des wissenschaftlichen Materialismus," Ostwald's talk, is reproduced in the *Verhandlungen* of 1895-6.

above, Ostwald argues that an experimental result, the irreversibility of certain natural processes, could put a nail in the coffin of "scientific materialism."[100]

Cohen sees Boltzmann's position as more nuanced than Ostwald's, because Boltzmann's commitment to atomistic materialism was not dogmatic. Rather, Boltzmann saw atoms (or material particles) as components of a logically compelling "model (*Bild*)" of natural processes. Cohen evaluates Boltzmann's theory in the context of his materialist psychology, which was Boltzmann's form of *Erkenntnistheorie*. Boltzmann did not argue that atoms or matter are sensed directly, but rather that a *Bild* of the laws of sensation based on material processes in the brain is, possibly, the most accurate:

> Above all we should take care to remember that Boltzmann espoused materialism only within certain bounds, as methodologically useful for research; that on the other hand matter could not be discussed *per se*, without this reference to the *Bild* under which it assists research. "When one says that matter or even atoms are sensed, then, naturally, one has expressed himself quite inaccurately. Rather, one should say that one does not take it to be impossible that the laws of variation of sensations [*Empfindungen*] should be represented most accurately through the *Bild* of material (physical, chemical, electric) processes in the brain."[101]

As a matter of care in drawing conclusions from experiments, Boltzmann says, it is important to distinguish between experiments that indicate that atoms and matter are elements of a plausible *Bild*, and experiments as evidence for the existence of atoms and matter *per se*. Here Cohen finds Boltzmann's view entirely accurate and even praiseworthy.

From Cohen's point of view, the confusion in Boltzmann's account arises when Boltzmann goes on to say which phenomena the materialist *Bild* is meant to explain.

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100 Historically, Cohen seems correct to see this as a flaw: it was Boltzmann, after all, who came up with the most enduring mathematical description of irreversible processes. Boltzmann won the day at the conference as well, or at least, he won the hearts and minds of the "young mathematicians," as Sommerfeld reports: "Boltzmann was seconded by Felix Klein. The battle between Boltzmann and Ostwald resembled the battle of the bull with the supple fighter. However, this time the bull was victorious.... The arguments of Boltzmann carried the day. We, the young mathematicians of that time, were all on the side of Boltzmann" (see, e.g., Flamm 1952, 351).

Cohen's objection to Boltzmann is that his argument for materialism escapes the boundaries of his sound methodological constraints. At first, as befits a founder of statistical mechanics, Boltzmann says that the atomist picture is an almost necessary condition only for constructing mathematical equations to describe the variation of sensations. One can concede easily, Cohen observes, that Boltzmann's *Bild* of the *variation* of sensations is almost inescapable. By the variation of sensations, Cohen means the phenomena that E. W. Weber and Herbart had analyzed mathematically in the early 19th century, namely, the qualitative properties of sensation such as intensity and frequency. These phenomena can be given a purely mathematical description: a degree on a scale of intensity, for example.[102] Boltzmann's solution was to argue that the variation of sensations could be modelled accurately only by giving a mathematical description of the material process in the brain that interprets the sensations. In the above quotation, Boltzmann used atomism, and therefore materialism, as a hypothetical presupposition of a given theory. Later, though, Boltzmann extends "the materialist *Bild*" to explain not just the variations between sensations, but the occurrence of sensations themselves. This is a subtle distinction. Cohen explains it as follows:

> Well, one certainly does not take it to be impossible that the variation of sensations be representable through the *Bild* of material processes. This is entirely to be granted; and one can even go further, and declare it to be impossible to represent the variation of sensations, and to investigate the laws of this variation, through another *Bild*. Nonetheless, Boltzmann goes even further in the other direction…His explanation relates the *Bild* of matter to the variation of sensations, yet he does not restrict the *Bild* to variation. In its place suddenly comes "the occurrence [*Zustandekommen*] of sensations," as if these were the same; whereas the laws of variation omit the question of occurrence in principle. The laws of variation of sensations are the task of

101 *Einleitung* 218. The citation from Boltzmann in the quote is from Boltzmann 1897, 102.

102 Weber had proven that sensed intensity of pressure did not vary in direct proportion to the units of pressure exerted. So if a machine exerted one pound of pressure on the skin, then two pounds, the second sensation did not have twice the intensity of the first, but some larger proportion.

> psychology; the first occurrence of sensations, on the other hand, is an old question of so-called metaphysics.[103]

What is the difference between variation and manifestation? Boltzmann's mathematical account of the laws of variation of sensation describes the general relations that hold between sensation and stimulus. The laws of variation might describe, for instance, the probabilistic mathematical relationship between units of light that a machine shines on one's retina and the motion of atoms in the part of the brain linked to visual sensation of light. However, Cohen observes, giving an account of the manifestation of sensation requires a much more extensive account, which cannot be given by means of the laws of variation. Cohen charges that Boltzmann attempts to extend his mathematical description to a universal materialist explanation of *why* the atomic processes in the brain manifest themselves thus. Cohen objects that this is an attempt to explain the connection between mind and body by means of the materialist hypothesis. Here Cohen picks up on Lange's argument in the *History of Materialism* to which Cohen's text is an introduction. Lange argues that, while materialist explanations are compelling, a "bare" materialism cannot account fully for the phenomena. Cohen argues, similarly, that while Boltzmann's materialist *Bild* is useful in a certain context, it cannot give an ultimate explanation of the mind-body connection.

Cohen's objection here can be extended to Boltzmann's mechanics in general. The atomic, materialist *Bild* is useful, Cohen says, as long as it is restricted to a mathematical description of events. But Boltzmann tries to elevate this description to an ultimate explanation in materialist terms. Cohen argues that Boltzmann has gone too far in his interpretation of the atomic *Bild*, just as Ostwald exaggerates the consequences of the energeticist *Bild*.

The debate between atomism and energetics has to do, then, with a conflict akin to that between description and explanation. As Ernst Cassirer argued, the question comes down to that of "whether perfectly general numerical description or (spatial) mechanical models are the goal of science, taken as the description of the

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[103] *Einleitung* 218.

constituents of reality."[104] For William Rankine, an energeticist and observer of the debate, the issue was:

> Instead of attributing the various kinds of physical phenomena to motions and forces no instances of which are ever given but which are merely inferred, it would suffice to stop at simple comparison and eventually reach principles that hold equally for all cases and so represent the ultimate discoverable relationship between facts.[105]

Rankine's argument would hold equally well of the difference between the "general, numerical" principles of *variation* of sensations and the *Bild* of the *manifestation* of our sensations in Boltzmann's theory. Cohen would agree with Rankine, then, that we should search for "the most general principles that...represent the ultimate discoverable relationship between facts." The question that remained for the controversy between atomism and energetics was how to find such a set of principles. Cohen argues that these ultimate principles must be mathematical rather than mechanical, in Cassirer's sense above.

4.3. Hertz's theory

In his initial presentation of the debate between atomism and energeticism, Cohen argues that both views are properly described as resting on pure thought: "In both, a pure, rigorous thought-element is made the foundation of existence, the basis of reality, instead of sensible perception [*Anschauung*]."[106] As Cohen sees it, atomism is based on the concept of atoms, and dynamism on the concept of force (energetics is a subspecies of dynamism). Both views are also supported by mathematical principles that are set at the foundation of theories of mechanics. Cohen thinks that the opposition between atomism and dynamism should be traced to a conflict of concepts and principles, and not to differences of opinion about what is proven by a given experiment. Cohen observes that Heinrich Hertz, in *The Principles of Mechanics*,

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104 Cassirer 1950, 98-99.

105 Cassirer 1950, 99.

106 *Einleitung* 242.

has isolated the fundamental concepts and principles of the then-contemporary atomist and dynamist theories of mechanics. Hertz proposes a third account that, in Cohen's view, goes some way to reconcile the opposition between atomism and dynamism. However, Cohen argues that further philosophical work is needed before Hertz's account can truly resolve the conflict.

In what follows, I will present Cohen's discussion of the *Principles*, and will present his critical revisions to Hertz's final view. My object in this is to give a final chronicle of Cohen's "historical" project in the philosophy of science in action. As I presented that historical method in the discussions of the Trendelenburg-Fischer debate and the *PIM*, Cohen argues that the empirical facts of a given theory should be set firmly in the context of the principles that have been laid at their foundation. As I presented above in this chapter, in his *Einleitung* Cohen refines this account, to give a more detailed picture of the relation between the principles that ground mechanical theories, the fundamental concepts of those theories, and the theorems that can be proven thereby. Hertz's *Principles of Mechanics* gives a precise account of that relation for the case of the atomist and dynamist (or energeticist) accounts of mechanics.[107] Cohen uses Hertz's results as source material, but argues that Hertz's alternative model for mechanics needs further refinement before it will resolve the conceptual opposition between atomism and dynamism.

Given Cohen's project of resolving that opposition, what are his criteria for success? Here Cohen's ultimate goal for science comes into play. The reason that Cohen emphasizes the distinction between intuition and thought, and the reason he argues for the primacy of thought in theory construction, is that pure thought is intersubjective and intertheoretical. That is to say, first, that thought *per se* must be divorced from the psychological process that engenders it. Cohen suggests we do so by means of his "critical" method, which isolates the axioms at the ground level of a process of reasoning. Second, for pure thought to be a means of becoming aware of objects, it must specify a way to relate spatial and temporal concepts to one another *without* making assumptions that restrict one's perspective. In other words, the

[107] I should note here, at the outset, that it is inaccurate to identify dynamism with energeticism. I take energeticism, as Hertz presents it in his second *Bild*, to be a 19th century token of the dynamist type. Cohen does so as well, but it is worth noting that his account leaves room for a type of dynamism other than the *Energielehre*.

mathematical principles that are presupposed to construct a theory must be universally applicable, and not rely on prior definitions established in experience, for instance. Cohen disapproves of the practice of incorporating a commitment to atomism and dynamism into the interpretation or even the constitution of the fundamental mathematical equations or principles of a mechanical theory. Cohen thinks that the application of mathematical principles will always be slightly subjective, since *some* perspective on experience is necessary. As in the discussion of the infinitesimal method in Chapter Three, however, he thinks we can successively refine the relation between mathematical principle and reality. The mathematical method of arriving at proofs through pure thought and reflection on experience is the model for such a refinement.

Cohen cites Hertz as the source of the way out of the confusion regarding the basic concepts and principles of mechanics. While Cohen thinks that Hertz's philosophical argument is in part undeveloped, he argues that Hertz achieves the necessary separation of perfectly general mathematical principles from *Bilder* such as Ostwald's and Boltzmann's, and thus is able to evaluate the relative merits of different *Bilder* from a logical perspective. In what follows I will present Hertz's theory, and then will show how Cohen thought it could be given an even more effective basis by means of the infinitesimal method.

The difficulty with finding a set of principles for mechanics at the end of the 19th century was compounded by the fragmented state of the discipline. In his Preface to Heinrich Hertz's *Principles of Mechanics*, Helmholtz himself represents the situation in Germany as follows:

> In Germany at that time the laws of electromagnetism were deduced by most physicists from the hypothesis of W[ilhelm] Weber, who sought to trace back electric and magnetic phenomena to a modification of Newton's assumption of direct forces acting at a distance and in a straight line.... This plentiful crop of hypotheses had become very unmanageable, and in dealing with them it was necessary to go through complicated calculations, resolutions of forces into their components in various directions, and so on. So at that time the domain of electromagnetism had become a pathless wilderness. Observed facts and deductions from exceedingly doubtful theories were

> inextricably mixed up together. With the object of clearing up this confusion I had set myself the task of surveying the region of electromagnetism, and of working out the distinctive consequences of the various theories, in order, wherever that was possible, to decide between them by suitable experiments.[108]

Arnold Sommerfeld reports frustration similar to Helmholtz's dating from his student days, which were at the same time as Hertz's experiments:

> My time of study coincided with the period of Hertz's experiments. At first, however, electrodynamics was still presented to us in the old manner in addition to Coulomb and Biot-Savart, Ampère's law of the mutual action of two elements of current and its competitors, the laws of Grassman, Gauss, Riemann, and Clausius, and as a culmination the law of Wilhelm Weber, all of which were based on action at a distance. The total picture of electrodynamics thus presented to us was awkward, incoherent, and by no means self-contained.[109]

Helmholtz duly assigned Hertz, who was his student, the task of establishing certain electromagnetic relationships by experiment. Hertz's experiments, and his conclusions on their basis, would become the standard in the field. Sommerfeld reports that the fog of confusion over basic relations was lifted:

> Teachers and students made a great effort to familiarize themselves with Hertz's experiments step by step as they became known and to explain them with the aid of the difficult original presentation in Maxwell's Treatise. [...] It was as though scales fell from my eyes when I read Hertz's great paper "Über die Grundgleichungen der Elektrodynamik für ruhende Körper."[110]

Hertz used his early, groundbreaking experiments, collected in the "Inquiries into the Propagation of Electrical Force" of 1894, to clarify the fundamental concept of "action at a distance" (*Fernkraft*), a concept central to the Faraday-Maxwell account

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108 Helmholtz [1894] 1956.

109 Sommerfeld 1952, 1-2.

110 *Ibid.* Hertz's paper appeared in the *Göttingen Nachrichten*, March 1890.

of electrodynamics. Here, Cohen observes, Hertz makes the careful distinctions that Boltzmann and Ostwald did not. According to Cohen, Hertz recognizes that his own experiments do not solve the problem of giving an ultimate conceptual foundation for mechanics, but only go to point out the need for an analysis of fundamental concepts:

> This [philosophical] attitude was the source of the methodological approach of this great work; and it is of the greatest value that Hertz did not disdain to profess this wholeheartedly. In the introductory "Overview" that he appended to the complete edition of "Inquiries into the Propagation of Electrical Force," he himself deemed the character of his investigations philosophical. "All of these well-supported experiments deliver a proof for the temporal propagation of a so-called *Fernkraft* [*action at a distance*]. This fact is the philosophical, and at the same time the most important result of the experiments, in a certain sense." This judgment is simply an expression of the precise and clear insight that the fundamental hypothesis of the "Faraday-Maxwell theory" rests on those philosophical grounds. According to this fundamental philosophical insight the measure of electrical forces is associated with that of weighable matter, and further, the measure of forces is connected to space, to the nature of space itself, and concurrently to time – thus, to the most significant basic concepts of mechanics.[111]

In this passage, Cohen argues that Hertz himself makes a clear link between Hertz's experiments and his philosophical insights. For instance, Hertz thinks that his experimental results proving "the temporal propagation of a so-called *Fernkraft*" contribute to the clarification of the concept of action at a distance. He demonstrated in the "Inquiries" that action at a distance was a *hypothesis* in Cohen's sense, that is, a proposition to which all the remaining elements of the theory stand in a relation of dependence.

For Hertz himself, the relation between the postulate of action at a distance and the foundations of mechanics in general was not settled by the account in the

[111] *Einleitung* 246. The citation from Hertz is from "Inquiries into the Propagation of Electrical Force," in Hertz *Gesammelte Werke* Bd. II, 20.

"Inquiries." The experiments in the "Inquiries" showed, as Cohen observes, that the "fundamental hypothesis" of action at a distance "is connected…to the most significant basic concepts of mechanics." But this clarification of the relation between *one* fundamental hypothesis and a prominent mechanical theory throws into relief the fact that rival theories exist, and that no reliable means had been given for distinguishing between them on logical grounds.

In 1894, Hertz published *The Principles of Mechanics*.[112] In it Hertz attempts to clear up the confusion over the basic principles of mechanics, not only on the basis of his groundbreaking experimental results, but also by giving a philosophical analysis of the fundamental concepts and principles of mechanics. Hertz describes the precise character of his conceptual analysis in the Introduction to the *Principles*, in which he observes,

> Strictly speaking, what was originally termed in mechanics a principle was such a statement as could not be traced back to other propositions in mechanics, but was regarded as a direct result obtained from other sources of knowledge…Since Lagrange's time it has frequently been remarked that the principles of the center of gravity and of areas are in reality only propositions of a general nature…. Thus the idea of a mechanical principle has not been kept sharply defined. We shall therefore retain for such propositions, when mentioning them separately, their separate names. But these separate concrete propositions are not what we shall have in mind when we speak simply and generally of the principles of mechanics: by this will be meant any selection from amongst such and similar propositions, which satisfies the requirement that the whole of mechanics can be developed from it by purely deductive reasoning without any further appeal to experience.[113]

At the most general level, Hertz's attention to the relationship of principle to theory is what Cohen takes notice of in his discussion of the "Inquiries," that "According to this fundamental philosophical insight…the measure of forces is connected to space, to the nature of space itself, and concurrently to time—thus, to the most significant

[112] Hertz 1956 [1894], translated D.T. Jones and J.T. Walley. Cited hereafter as *Principles*.

[113] *Principles* 4-5.

basic concepts of mechanics." It is clear from the structure of the *Principles* that when Hertz says that every *element* of the theory should be deducible from the mathematical principle, that does not mean every *theorem* but rather the relations described in Book One:

> The subject matter of the first book is completely independent of experience. All the assertions made are a priori judgments in Kant's sense. They are based upon the laws of the internal intuition of, and upon the logical forms followed by, the person who makes the assertions; with his external experience they have no other connection than these intuitions and forms may have.[114]

Hertz's initial requirement for the construction of a mechanics is that each basic concept, such as space, time, and mass, should be defined by reference to the fundamental principle of the system, without appeal to experience. His criteria for evaluating distinct theories based on distinct principles are based on the logical permissibility, correctness and appropriateness of these systems of principles and fundamental notions.

Here is where Cohen's account of mathematical and logical axioms as hypotheses finds a counterpart in Hertz's text. Recall that Cohen gave two properties of axioms: first, that they are constructed by a pure thought process, and second, that they are the "grounding for all the theorems that can be deduced" from them.[115] The first property will be a significant difference between Cohen's and Hertz's account, as I will show in due time. Hertz's mathematical "principles" of mechanics do satisfy the second property Cohen assigns to axioms. Hertz discusses three *Bilder* of mechanics. In his presentation of the basic principles of mechanics, Hertz relies on what I will call the "principle condition" mentioned above: a proposition can be counted as a principle of mechanics if and only if it "satisfies the requirement that the whole of mechanics can be developed from it by purely deductive reasoning without any further appeal to experience."[116] As Cohen remarks,

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114 Hertz [1894] 1956, 45.

115 *Einleitung* 186-7.

116 *Principles* 4.

> A "selection" of the propositions set at the foundation [of mechanics] could satisfy this condition, and thus different presentations can be given of the principles of mechanics, or different *Bilder* "of the things of the sensible world and the processes that occur in it."[117]

In what follows, I will give a brief summary of Cohen's account of Hertz's *three Bilder*: classical mechanics, the energy-focused *Bild*, and Hertz's own *Bild*. In evaluating Hertz's arguments about the *Bilder* of mechanics, I will first identify and explain the mathematical principle at the foundation of the *Bild* and then will present Hertz's argument for the logical flaws or advantages of the account. Throughout, I will note briefly Cohen's reactions to Hertz's presentation.

Hertz identifies three possible *Bilder* for mechanics, and finds the mathematical principles at the basis of each of the *Bilder*. The classical picture (what Cohen refers to as "atomism") is based on d'Alembert's generalization of Newton's third law of motion. The energy-based picture (Cohen's "dynamism" or energetics) is based on Hamilton's principle, which can be seen as a combination of d'Alembert's principle and the principle of the conservation of energy. Finally, Hertz presents his own *Bild*, based on a mathematical principle combining Galileo's law of inertia [*Trägheit*] and Gauss's principle of least constraint. Hertz believes that this choice at the level of principle gives him the freedom to reduce the number of fundamental concepts included in his *Bild*, and thus to address the logical shortcomings of the *Bilder* of classical mechanics and of energetics.

Cohen cites Hertz's three requirements for the so-called "Eindeutigkeit der Bilder." Hertz's use of the term "Bild" differs somewhat from Boltzmann's use of the term above. Boltzmann thought that the dispute between atomism and energetics was in large part due to differences in the logical clarity of the underlying explanations given by each school. Hertz's "Eindeutigkeit der Bilder" has the connotation of logical clearness to which Boltzmann appeals, but it also includes the important criteria of lack of ambiguity and uniqueness – that the *Bild* should paint a direct and unique picture of the phenomena it describes.[118] Hertz's usage also leaves

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[117] *Einleitung* 249. Citation is from *Principles* 4. All further citations from this edition are translated Jones and Walley. In Cohen's original text, the citations are from Hertz's *Gesammelte Werke*.

[118] For a detailed discussion of this notion of "Eindeutigkeit" see Howard 1996.

open the possibility of alternative *Bilder*, something that will become crucial to his analysis. Cohen reports Hertz's own criteria for the conceptual analysis of a *Bild* as follows:

> The "Eindeutigkeit der Bilder" is determined through *three* criteria: first through that of *permissibility*, "that all our *Bilder* be logically permissible or, briefly, that they shall be permissible." This permissibility rests on the validity of the "laws of our thought" or: "What enters into the *Bilder*, in order that they may be permissible, is given by the nature of our mind." To mention the works in which we ourselves have clarified Kant's terminology [*that is, Cohen's own Kant-scholarship*], one can see that Hertz does not distinguish consciously between the metaphysical and the transcendental *a priori*; but, at any rate, his further criteria are sufficient to construct boundaries against the ill effects of this confusion. The second criterion is that of *correctness*. The requirements for this are "contained in the *facts of experience*, from which the *Bilder* have been built up." Correctness is, then, not within the power of the *a priori*. One the other hand, the *third* criterion leads back to it again. He describes it as that of *appropriateness*. It expresses the number of "*essential* relations" that are "reflected" in the *Bild*. Appropriateness is manifest, then, as "clearness" and as "simplicity," as the latter, insofar as a "smaller number of superfluous or empty relations" are contained in the *Bild*.[119]

These criteria for *Eindeutigkeit* are Hertz's tools for evaluating the distinct *Bilder* of mechanics given by classical mechanics and by some form of energetics. He presents these *Bilder* with the aim of evaluating them according to the criteria of *Eindeutigkeit*.

The first *Bild*, as Cohen observes,

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[119] My translation of the passage from Cohen's *Einleitung* 248. The English translation of Hertz's *Prinzipien* is amended from the 1956 translation as follows: In the original "*Bild*" is translated as "image;" I have left it in the original German, here and in the citations from the Jones and Walley translation that follow. The original for the description of "correctness" reads "enthalten in den *Erfahrungstatsachen*, welche beim Aufbau der Bilder gedient haben." Jones and Walley translate *Erfahrungstatsachen* as "results of experience." I have replaced it with "facts of experience," where "facts" is an equally valid translation of "Tatsachen." Further, in the description of "appropriateness" Jones and Walley do not translate "wiedergespiegelt" directly, whereas I have here, since Cohen cites it directly.

> [i]s that of the usual representation of mechanics in almost all textbooks and treatises. It goes back primarily to *Newton*, in whom all *four* basic concepts first appeared in connection with each other, namely *space*, *time*, *force*, and *mass*. "In it force is introduced as the cause of motion, existing before motion and independently of it." The first two Newtonian laws express this concept of force; but in the third a *new* concept of force is presupposed. Force arises from motion, as a *counterforce*.[120]

The first *Bild*, Hertz argues, rests on D'Alembert's Principle. Hertz claims that Newton's laws alone do not satisfy what I referred to above as Hertz's "principle condition," namely that the whole of mechanics must be deducible from it without further reference to experience. Hertz notes that Newton's laws do not "furnish any general expression for the influence of rigid spacial [*sic*] connections" (*Principles* 5). Newton's three laws of motion from the *Principia* are as follows:

1. Every body continues in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed upon it.

2. The change of motion is proportional to the motive force impressed; and is made in the direction of the right line in which that force acts.

3. To every action there is always opposed an equal reaction.[121]

Hertz argues that Newton's laws alone do not allow us to reduce problems of dynamics to problems of statics, that is, they do not provide a basis for a set of deductive rules for how to extend geometrical reasoning to dynamics. Newton's laws presuppose some notion of force, but in the case of the third law of motion, as Cohen observes, "force arises from motion as a *counterforce*." Newton's laws do not give any formal principle for manipulating the relations of forces to counterforces geometrically. D'Alembert's principle, first presented in the *Traité de dynamique* of 1743, allows us to do so.

[120] *Einleitung* 249-50. Citation is from *Principles* 4.

[121] Laws I, II and III from the "Axioms, or Laws of Motion."

D'Alembert's principle is a generalization of Jean Bernoulli's 1717 principle of virtual work. Bernoulli's principle says the following: "For a rigid body or a system of interconnected rigid bodies [*that is, a system at equilibrium*], internal forces, which always appear in equal and opposite pairs, must do no work."[122] More specifically, Bernoulli's principle can be represented as:

$$\mathbf{F} \cdot \partial\mathbf{r} = 0,$$

where **F** is the force applied to a particle, and $\partial\mathbf{r}$ is a "virtual displacement" or infinitesimal increment of the particle. Bernoulli's principle is referred to as the "principle of virtual work" because the principle implies that the infinitesimal displacements ("increments") of the particle times the force applied must be zero. This has the further implication that Bernoulli's principle applies only to systems at "static" equilibrium.

D'Alembert introduced his own principle in 1742, and gave a proof for it in the *Traité de dynamique* of 1743. D'Alembert's principle extends Bernoulli's to the case of dynamic systems, that is, of systems in motion. The principle allows for problems of dynamics to be solved by static, that is to say geometrical, methods:

> This is accomplished by introducing a fictitious force equal in magnitude to the product of the mass of the body and its acceleration, and directed opposite to the acceleration. The result is a condition of kinetic equilibrium.... The principle shows that Newton's third law of motion applies to bodies free to move as well as stationary bodies.[123]

Hertz's presentation makes it clear that d'Alembert's principle is the principle of the first *Bild* that fulfills what I've called the principle condition, that the elements of mechanics be deducible from the principle without further recourse to experience. As Hertz remarks at the end of his initial presentation of the *Bild*,

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[122] Thomson 1986, 269.

[123] *The Columbia Encyclopedia* 2003, "D'Alembert's principle." D'Alembert's own presentation of the principle in the *Traité* is somewhat less clear for our purposes. A more formal presentation of the principle is: "We will let **p** be the momentum of a particle in the system, and separate the forces acting on it into an applied force **F** and a constraint force **f**. The equation of motion can then be written as $\mathbf{F} + \mathbf{f} - \mathbf{p}^{\circ} = 0$, which states that the forces are in equilibrium with the kinetic reaction $-\mathbf{p}^{\circ}$.... As before, the virtual work of the constraint force is zero since **f** and $\partial\mathbf{r}$ are mutually perpendicular.The virtual work of the forces acting on the particles is, then, $(\mathbf{F} - \mathbf{p}^{\circ}) \cdot \partial\mathbf{r} = 0$" (Thomson 1986, 269).

> Here d'Alembert's principle extends the general results of statics to the case of motion, and closes the series of independent fundamental statements that cannot be deduced from each other. From here on everything is deductive inference.[124]

D'Alembert's principle is used in the first *Bild* as one of Cohen's hypothetical axioms: a rule determining the relations between the fundamental concepts of the system. However, most of Hertz's objections to the first *Bild* do not have to do with d'Alembert's principle, but with the logical status of the fundamental notions of the *Bild*: space, time, force and mass.

Hertz evaluates the logical status of the first *Bild* on the basis of all three criteria for the "Eindeutigkeit der Bilder," and finds problems with the first *Bild* on all three fronts. Cohen sees all three of Hertz's objections as arising from the fact that this first *Bild* does not have a proper appreciation for the "positive significance of the laws of thought."[125] In the evaluation of the correctness of the first *Bild*, Cohen argues, Hertz bases his objections on the fact that the first *Bild* explains the experience that we have had until now, but does not allow for a *Bild* based on the fundamental concepts (of force, mass, space and time) that goes beyond a *description* of prior experience. Hertz remarks, "that which is derived from experience can again be annulled by experience."[126] Appropriateness is the criterion that the *Bild* should represent the essential relations of the objects. Hertz gives two arguments against the appropriateness of the first *Bild*. First, he claims that the natural laws allowed by the first *Bild* are not appropriate to natural motions:

> All the motions of which the fundamental laws admit, and which are treated of in mechanics as mathematical exercises, do not occur in nature. Of natural motions, forces, and fixed connections, we can predicate more than the accepted fundamental laws do.[127]

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124 *Principles* 5.

125 *Einleitung* 251.

126 *Principles* 9.

127 *Principles* 10.

Further, he argues that the relations in nature are in fact simpler than the first *Bild* would have it. As a result, he concludes, we can give several objections to the "Eindeutigkeit" of the first *Bild*.[128]

Cohen interprets all of Hertz's objections to be based on a single claim, "And it happens that all the methodological deficiencies [of the first *Bild*] rest on the *first* mistake in the presentation of the concept of force: that before motion, *force* was taken *as the cause* of motion."[129] We can reconstruct Cohen's argument as follows. If the concept of force is taken as fundamental, and motion taken to be derived from force, then the account risks restricting the concept of force unduly. This restriction leads to the exclusion of certain types of motion from the category of force—for instance, if these motions are not products of mass and acceleration, as defined by Newton's second law. That result will detract from the logical appropriateness of the *Bild*.

The first *Bild* corresponds to the atomistic picture espoused by Boltzmann in the discussion above. The second *Bild* that Hertz presents is based on the "energy doctrine" that Cohen associates with Faraday, Ostwald and Maxwell. As Cohen describes it,

> The *second Bild* that Hertz sketches of mechanics rests on the standpoint of the basic law that has ruled physics since the middle of the century. While the ultimate goal of physics until then was to reduce natural events to *Fernkräfte* [*actions at a distance*] between the atoms of matter, the ultimate aim of modern physics is to reduce appearances to the *laws of the exchange of energy*. The concept of force is replaced by the concept of energy. The basic concepts on which this *Bild* of mechanics is based are *space*, *time*, *mass*, and *energy*.[130]

This *Bild* is the theory of mechanics accepted by the energeticists. All energeticists accepted the principle that energy is a fundamental concept of physics. They differed slightly on how to interpret that claim. Wilhelm Rankine and Robert Mayer (one of the discoverers of the conservation of energy) argued that the analysis of energy is an

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128 See, e.g., *Principles* 13.

129 *Einleitung* 252.

130 *Einleitung* 252-3.

analysis of the numerical relations that govern transformations of energy. Ostwald argued that what were described as transformations of force in the first *Bild* are in fact exchanges of energy, and further, that all dynamic relations (motions) are transfers of energy. Georg Helm had in some ways the most sophisticated position philosophically. As Cassirer puts it, Helm "rejected…[Ostwald's] idea that energy is an indestructible substance shifting from place to place."[131]

Hertz argues that no matter what philosophical commitments various energeticists had about the nature of energy, they all take Hamilton's principle as the fundamental principle for their mechanics. Hertz describes the kinetic energy of a system as the energy determined by the "absolute velocities" of the masses in the system, and potential energy as that determined by the "relative position" of the masses.[132] Hamilton's principle says that if we know the configuration of a system at two times, then the integral sum of the variations of the kinetic and potential energy of the system between those two times is zero. In other words, Hamilton's principle says that for any given time interval, the sum of the absolute velocities of the masses of the system, plus the sum of the energy produced by their relative position, will be zero.[133]

Hamilton's principle describes the transfer of *energy* within a system, and not the attraction between material particles, for instance. Thus the second *Bild* can be credited with having avoided some of the worries with the concept of matter bound up with the first *Bild.* However, as Cohen remarks, Hertz does not think that even the second *Bild* has done away with the logical difficulties with the concepts of force, matter and substance. In particular, Hertz thinks that the second *Bild* comes perilously close to simply replacing the fundamental notions of force and substance with that of energy. Cohen takes this argument to be particularly significant. For instance, he considers the following sequence of passages:

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131 Cassirer 1950, 99.

132 *Principles* 15.

133 Hertz himself presents the principle as a minimizing principle, that is, as if the values for the energy reach a minimum but do not actually reach zero.

"Two of them—space and time—have a mathematical character; the other two—mass and energy—are introduced as physical entities which are present in given quantity, and cannot be destroyed or increased."[134]

> The "real" reason, "why physics at the present time prefers to express itself in terms of energy," is that it is most familiar with a representation of atoms that is in no way suited "to serve as a known and secure foundation for mathematical theories."[135]

Cohen argues that "[i]nsofar as energy does away with the concept of the atom, it appears not only to deal with the concept of force, but at the same time to supplant that of mass. But the disadvantages of the second *Bild* begin with the conflicts about this concept."[136] Cohen focuses on two of Hertz's logical objections to the second *Bild.* The first, as alluded to in the quotation above, is that the concept of energy is only a substitute for substance, and does not resolve the logical inconsistencies with the allied concepts of atoms and substance. The second is an objection to the notions at the foundation of Hamilton's principle.

With respect to the first objection, that energy is used in the second *Bild* as a substitute for substance, Cohen cites Hertz's remarks:

> At the present time many distinguished physicists tend so much to attribute to energy the properties of a substance as to assume that every smallest portion of it is associated at every instant with a given place in space, and that through all the changes of place and all the transfers of energy into new forms it retains its identity.[137]

Hertz points out that if energy is to be treated as that which underlies transformations and motion in space, then there is not much to distinguish it from the old concept of substance besides that it is not atomic. As Cohen observes,

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134 *Principles* 15.

135 *Principles* 18.

136 *Einleitung* 252-3.

137 *Principles* 21.

> For Hertz crediting energy with the character of substance was the objectionable element of the second *Bild.* Perhaps too the description of causality as a category was objectionable to him already, in that it had a tendency to disclaim energy as a basic concept; perhaps he scented behind this the category of substance.[138]

Hertz seems to share Cohen's conviction that the opposition between atomism and dynamics cannot be resolved by positing energy as a fundamental concept instead of (atomic) substance. According to Hertz, the definition of energy in the second *Bild* leads to the same logical difficulties with the concept of substance encountered with the first *Bild.*

In particular, Hertz continues, the use of Hamilton's principle to evaluate transformations of energy mathematically leads the second *Bild* into logical difficulty. Hamilton's principle says that the integral sum of the kinetic and potential energy of a system for any given time interval will be zero. Hertz points out that this way of constructing the principle leads into difficulty:

> Hamilton's principle, when we come to look into it, proves to be an exceedingly complicated statement. Not only does it make the present motion dependent upon consequences which can only exhibit themselves in the future, thereby attributing intentions to inanimate nature, but, what is much worse, it attributes to nature intentions which are void of meaning. For the integral, whose minimum is required by Hamilton's principle, has no simple physical meaning; and for nature it is an unintelligible aim to make a mathematical expression a minimum, or to bring its variation to zero.[139]

Hamilton's principle depends on the concept of *potential* energy, which as Hertz puts it is the energy due to the "relative position" of masses in the system. Hertz objects, not to the claim that potential energy is an unclear concept in itself, but to the way Hamilton's principle is constructed. Hertz argues that Hamilton's principle makes the actual motion of a system dependent on reactions that are imminent, so to speak, given the relative positions of the components of the system. Hertz suspects that this

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[138] *Einleitung* 256.

[139] *Principles* 23.

way of conceiving of the transfers of energy has hidden behind it a teleological or anthropomorphic view, which "attributes intentions to inanimate nature." Further, he argues, these intentions are not easily translated into physical terms. An integral that sums kinetic and potential energy over a time interval is not easily conceived in physical terms; and moreover, he argues, the idea that the sum of such an integral would be zero is not a physical concept but a purely mathematical one. In fact, he says, such a minimization is "for nature…an unintelligible aim."

Hertz takes exception to Hamilton's principle as a foundation for mechanics, not because it uses abstract mathematical reasoning, but because this mathematical abstraction is brought in to support a claim that there is a *dependence* relation between kinetic and potential energy. Hertz argues that the reasoning behind Hamilton's principle really only supports the idea of a mathematical relation between quantities, not of a mechanical relation between types of energy that seems suspiciously akin to causality.

Hertz concludes that the first two *Bilder* have logical disadvantages. The first, atomist *Bild* is logically inadequate for two general reasons: "As far as the form is concerned, we consider that the logical value of the separate statements is not defined with sufficient clearness. As far as the facts are concerned, it appears to us that the motions considered in mechanics do not exactly coincide with the natural motions under consideration."[140] The basic concepts of the first *Bild*, space, time, force and mass, are not defined clearly enough. Further, the laws of motion specified in the first *Bild* are not appropriate to all the motions of which we are aware. The second *Bild*, while it clears up some of the confusion over fundamental concepts, does not fare much better. Hamilton's principle, the mathematical basis of the theory, requires three interlinked concepts: an integral sum of forces over time, kinetic energy, and potential energy. Hertz observes that it is difficult to find a clear physical meaning for these concepts. Insofar as the second *Bild* does specify a physically intelligible concept, it is the idea of energy as mass, which, Hertz finds, comes close to defining energy as a kind of substance.

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140 *Principles* 13.

Having presented the logical difficulties with the first and second *Bilder*, it is left to Hertz to construct a *Bild* for mechanics that he will use himself. Cohen sums up the state of Hertz's argument immediately before Hertz's presentation of the third and final *Bild* as follows:

> Let us remember…that endowing energy with "the character of substance" was the circumstance behind the true logical difficulties with the second *Bild*, while the rejection of the concept of atom was the most important condition for this [second] *Bild*. However, what is the *concept of atom* other than a version of the concept of substance? […] The *third Bild* will give up this fusion of energy and substance. Which path will it then take? If it gives up energy, then there is a danger of falling back into the concept of force in the first *Bild*, which takes force as a cause and thus makes it material. On the other hand, if [the third *Bild*] gives up mass, then it is threatened with the fate of the second *Bild:* to have to illicitly substitute mass-substance for energy.[141]

Cohen argues that the revision of the first and second *Bilder* that is essential for Hertz's purposes is to find some way of avoiding the "category" of substance and the allied concept of causal interaction. While Cohen makes much of the "Kantian" background for Hertz's use of the term "category," it is sufficient for our purposes here to point out that a "category," as Hertz uses the notion here, is a general term for a group of existing things, as opposed to a concept, which could refer simply to a thought-content, for instance. Cohen remarks that even the concept of mass-energy, when interpreted as a substrate of interaction, can bring along with it an appeal to substance (as a category of "being") and causality. Cohen concludes, going beyond Hertz's actual remarks, that Hertz's strategy in the third *Bild* is to posit a "simpler concept" than mass, from which the concept of mass can be derived. This "simple concept" would need to avoid appeal to the notions of force as the cause of motion, and of mass as a substance that persists through interaction:

> Another possible question could be: whether, perhaps, the concept of mass, like that of energy, could be replaced with a simpler concept, to finally

[141] *Einleitung* 254-5.

> resolve the conflicts between force and mass. Hertz does not discuss or mention this possibility. He chooses it, though, and supports it by employing it himself.[142]

The "simpler concept" to which Cohen refers here is the concept of motion, derived from Hertz's basic principle. Cohen argues that Hertz's concept of motion is not only the "simpler concept" to which Hertz appeals to substitute for substance and force, it is also the basis for Hertz's definition of mass.

4.4. *Cohen's final argument*

In what follows, I will examine Cohen's argument for the claim that Hertz's concept of mass is defined by another concept of his theory. First, Cohen presents Hertz's own account of the third *Bild*.

> The *third Bild* of [Hertz's] own mechanics, is based on only "three independent fundamental conceptions:" *space*, *time* and *mass*. These three basic concepts are "objects of experience." "A fourth idea, such as the idea of force or energy…as an independent fundamental conception, is here avoided." It [*the third Bild*] certainly "requires some complement." He attempts to bridge this remaining gap through a *hypothesis*: "the manifold of the actual universe must be greater than the manifold of the universe which is directly revealed to us by our senses. If we wish to obtain a *Bild* of the universe which shall be well-rounded, complete, and conformable to law, we have to presuppose, behind the things which we see, other, invisible things—to imagine confederates concealed beyond the limits of our senses."[143]

Hertz's way of avoiding the problems with the concepts of force and energy is simply to eliminate them from the stock of fundamental notions. There are a number of possible ways to interpret this move. I will focus, of course, on Cohen's own reaction.

Cohen observes that there is textual evidence to support the idea that Hertz avoided adding a fourth fundamental notion (of force or of energy) so that he could

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[142] *Einleitung* 254-5.

[143] *Einleitung* 255. Interspersed citations from *Principles* 24-26.

avoid positing the *existence* of some substantial element, that is, some element that persists through change. Cohen argues that Hertz's "concealed confederates" do not need to be assigned any particular ontological status. Here Cohen's presentation follows Hertz's closely. Cohen cites the following from Hertz's text:

> In the first two *Bilder* the concepts of force and of energy were "entities of a special and peculiar kind....We may admit that there is a *hidden something* at work, and yet deny that this something belongs to a special *category*."[144]

Cohen argues that it is plausible to interpret Hertz's claim, that the "hidden something" does not belong to any "category," in the context of his remark just before that force and energy had been "*entities* of a special kind." That is, Cohen argues, Hertz wants to say here that the "hidden something" that completes his account does not belong to the "category" of being: we do not have to conceive of the hidden something as actually existing for it to be an effective postulate of the theory.

Cohen argues that the notion of a "hidden something" needs more explanation than Hertz gives it. Cohen's reconstruction of what he thinks Hertz's account should be begins with two elements of Hertz's theory, though they may be used differently than Hertz himself employs them. First, Cohen argues that any definition of the notion of a "hidden something" must begin with the fundamental mathematical principle of the third *Bild*: an amalgam of the principle of the straightest path and the law of inertia. Hertz expresses this principle as follows: "Every natural motion of an independent material system consists herein, that the system follows with uniform velocity one of its straightest paths" (*Principles* 27).

Cohen argues that since this principle is at the foundation of the third *Bild*, Hertz can appeal to the argument that *motion* determines the concept of mass on his account. Cohen then goes beyond Hertz's own account, to argue that the use of motion to determine mass needs a purely conceptual foundation. Here Cohen is addressing a concern expressed by Joseph Petzoldt, among others. In his 1895 essay "Das Gesetz der Eindeutigkeit," Petzoldt argues that Hertz's foundation for mechanics was questioned, because of its mix of *a posteriori* and *a priori* elements:

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144 *Ibid.*

> Hertz...includes the law of inertia in his basic principle...However, the law of inertia owes only a small part of its conceptual power of persuasion to its experimental foundation. – The usual expression [of the law] mixes two propositions that should be entirely separate, and moreover, it mixes two sides of these claims, the *a priori* side and the aspect [derived] from experience. As a result, a debate arose over the apriority or non-apriority of the principle.[145]

Petzoldt's point is that, as Hertz admits, the principle requires evidence to be established in the first place.[146] Although Hertz argues that this principle is the first, and last, principle to be derived from experience, nonetheless it is legitimate to ask whether the principle *itself* is *a priori*. If it is not, Petzoldt points out, we might question how much the principle constrains our *thought*, given that we can distinguish the empirical from the formal aspect of the principle. This is a particular problem for Hertz, if, as seems plausible, Hertz wants to argue that the principle does constrain the "laws of internal intuition" of a person constructing a mechanical system.[147]

Cohen's strategy for giving a secure conceptual foundation for the principle of the straightest path is to isolate what, to him, is the part of the principle we can establish by means of pure thought: the concept of "uniform velocity." Once this aspect of the principle has been given a secure *a priori* foundation, Cohen argues, we can derive the fundamental geometrical relations of mechanics from it *a priori*, as Hertz wants to do:

> This purely dynamic sense of his [*Hertz's*] concept of mass outlines the contours of the new *Bild*. It distinguishes the physical content of his *Bild* from its mathematical form. But since it has to do with the physical content of the association between space and mass, then it deals at least with geometrical determinations, which, since mass is a variational concept of movement, must verge upon determinations of infinitesimal geometry. Thus

145 Petzoldt 1895 §15, 188, my translation.

146 *Principles* 28.

147 *Principles* 45.

> the concept of the *straightest path* arises for the motion of material systems, and in it the *basic principle* of the new *Bild.*[148]

First Cohen observes that taking motion to implicitly define mass, as it were, requires Hertz to distinguish "the physical content of his *Bild* from its mathematical form." The "mathematical form," in this case, is the principle of the straightest path, Hertz's fundamental principle. Cohen points out that since this principle is being used to determine the concept of mass, which is a "variational concept of movement," it requires the use of infinitesimal geometry. Cohen finds a secure basis for the foundation of Hertz's basic principle, not surprisingly, in the "infinitesimal method" that he presented in *PIM*. Cohen believes that his account, based on the infinitesimal method, must and can give a conceptual foundation for the claim that Hertz's basic principle specifies a set of relations *sui generis* and *a priori.*

> From this *infinitesimal* account of *new* movement comes the possibility of a new determination for the "mathematical expression" of force, as "the merely thought mediation between two movements." Here a *new concept* comes in with the expression: "merely thought" [*nur gedacht*]. While elsewhere the new thing is only negatively described as "not perceptible," here it is positively described as "thought." What, however, is the positive force of its being thought? This question is not posed.[149]

Cohen sees the weakness in Hertz's account in the fact that Hertz does not give a *positive* account of how mathematical relations determined *a priori* can establish a set of rules for the "merely thought mediation between two movements." Cohen argues that this "merely thought mediation" is the best basis for Hertz's claim that the principle of the straightest path constrains our "internal intuition." Cohen argues that Hertz needs an account of how the concepts of space, time and mass are derived

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148 *Einleitung* 257.

149 *Einleitung* 257. Here I have departed significantly from the 1956 translation. The original citation, as Cohen reproduces it, is: "das nur gedachte Mittelglied zwischen zwei Bewegungen." Jones and Walley translate this as "a middle term conceived only between two motions" (*Principles* 28). Cohen's reading here depends on taking the adjectival phrase "das nur gedachte" in its original place in the sentence: the phrase clearly modifies "Mittelglied," and not "zwischen zwei Bewegungen."

from a prior determination of the functional relations between infinitesimal increments, that is, the determinations of differential geometry.

Cohen appeals to his earlier account in *PIM* for the outlines of his account. I will summarize his argument briefly here. To give a physical definition of "uniform velocity" as used in Hertz's fundamental principle, one must first specify a set of differential equations that will determine the smallest displacement of a particle in the system. Cohen remarks, "*Motion is the determining [element] in the new concept of mass. Concealed mass leads thereby to a refined concept of motion.*"[150] The definition of a particle is then *derived* from the previous analysis of motion by means of differential equations: "The new mass can only refer to the physical motion, in which *differential equations* are conceived along with motion, and not directly to that which we perceive."[151] Here Cohen finds the sought-after foundation for the concept of mass as independent from hypotheses about atoms, force or even energy. Mass is determined mathematically through the evaluation of motion by means of differential equations. This determination depends only on the postulation of a straightest path, which can be interpreted as an infinitesimal increment. The definition of the concept of an infinitesimal increment can be specified uniquely, to satisfy the logical requirements Hertz has set out, by means of "mere thought."

By analyzing Hertz's *Bild* in the guise of a philosophical historian, Cohen finds what he sees as the missing logical foundation for the *Bild*. At the same time, he argues that the "ancient opposition" between atomism and dynamism is reconciled by the specification of a perfectly general, *mathematical* method that all such theories have in common. As Cohen concludes,

> People argue over the basic concepts of mechanics, whether one should choose mass or force or energy, but they forget that with all three the same *mathematical* basic concept is presupposed alongside, without which the new considerations cannot be engaged at all; just as it was itself first brought to a concrete definition as a mechanical basic concept.[152]

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150 *Einleitung* 256, emphasis in the original.

151 *Einleitung* 256.

152 *Einleitung* 262.

CONCLUSION

It remains to show in what sense Cohen's research has contributed to his goals for the philosophy of science. As I remarked above, Cohen believes that philosophy can contribute to science by rigorously pursuing ever more general, and ideally universal, concepts and principles with which to express the scientific facts of which we are in possession. Cohen's investigation of the three *Bilder* of mechanics is meant to show that even Hertz has not capitalized on the force of his own reasoning, that a mathematical principle can be specified for mechanics that relies only on a method for the *a priori*, mathematical analysis of motion. If that account is correct, then Cohen has indeed been able to reconcile the seeming conceptual opposition between atomist and dynamic accounts of mechanics by means of a thoroughgoing investigation of the concepts and principles at the foundations of mechanics.

Cohen's history and philosophy of science began with his formulation of a neo-Kantian epistemology. This epistemology has two features. Cohen takes scientific facts as the subject matter for epistemology. These facts are "given," or available to us, through the medium of scientific theories. According to Cohen, giving a justification for scientific facts depends on reconstructing and analyzing the theories that reveal those facts.

When Cohen first stated his epistemological views in his essay on the Trendelenburg-Fischer debate, he set himself a challenge: to demonstrate how all the facts of natural science can be drawn from a set of fundamental principles. Cohen met the challenge later in his career, with the publication of his *Introduction* to Friedrich Lange's *History of Materialism*. In the *Introduction*, Cohen traces the structure of mechanical theories back to a set of mathematical principles, following Heinrich Hertz's lead. Cohen concludes that the entire structure of fundamental relations in mechanics, Part One of Hertz's book, can be derived from a combination of Hertz's third *Bild* and the infinitesimal method in mathematics that Cohen defended in *The Principle of the Infinitesimal Method and its History.*

Ultimately, Cohen devoted his career in the history and philosophy of science to re-thinking the relationship between mathematics and scientific theories. From the time of the Trendelenburg-Fischer debate, Cohen was dedicated to the idea that mathematics is a science of *sui generis* reasoning that can legislate for itself, according to the laws of thought. The practice of mathematics must not be constrained by any given element of experience or perception, but only by the laws of thought.

Cohen was most preoccupied with the transition from mathematical construction to scientific theories. He was interested in showing how mathematical ideas can be realized. That project will require, at least, an account of how mathematical principles can be identified as one of the steps necessary to construct a scientific theory.

In the course of this project, Cohen was led to rethink the distinction between pure and applied mathematics. From the standpoint of epistemology, Cohen thinks, there is no principled distinction between the two. "Pure" mathematics can be used, as it was by Hertz, as the foundation of a theory of natural science. According to Cohen, there is no reason to argue that pure mathematics thereby becomes "applied." Instead, Cohen thinks of mathematics as a way of constructing fundamental principles, which legislate the law-like relations between elements of a theory. These elements might include the basic concepts and notions of the theory. Cohen argues that, in view of the fundamental role of mathematics in grounding our knowledge of facts, all mathematics should be called "free" rather than either "pure" or "applied." As he remarks in the conclusion to *PIM*:

> That which is conceived by pure mathematics is nonetheless *applicable*, because pure [mathematics] is only that which is capable of being applied under given conditions. But, certainly, pure [mathematics] is not preoccupied with the fact that these conditions are given. That is why it seems relevant [to mention] the modification that qualifies mathematics as *free*.[153]

Thanks to his evaluation of Hertz's *Principles of Mechanics*, Cohen can now back up his claim that the selection of fundamental mathematical principles is free, and yet can be applied to real processes. From the early days of the Trendelenburg-Fischer debate, Cohen has searched for a sound argument that logic and mathematics need not consist of analytic inferences from our representations to be "objective." Finally, in the *Einleitung*, Cohen has presented a comprehensive argument for this basic claim of his epistemology.

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[153] *PIM* §91. Cohen cites Cantor here; the citation can be found in Cantor 1996 [1883], §8.

Finally, Cohen argues that epistemology should gather its source material from the *history* of science. I have just sketched the two reasons for this conclusion. Cohen's epistemological method is to analyze and to reconstruct scientific theories, and to show how they reveal the facts. Since we have a choice of the mathematical principles on which these theories are based, constrained only by the laws of thought, an historical account can identify and then compare the principles at issue. Further, a *philosophical* history can contribute to the common goals of science and of philosophy, by clarifying and analyzing the fundamental concepts and principles of construction of scientific theories.

APPENDIX TO CHAPTER THREE

L'Hôpital and the Method of Tangents

One of the clearest presentations of Leibniz's method does not come to us from Leibniz himself, but from the Marquis de l'Hôpital, who was a defender of Leibniz's use of infinitesimal quantities to calculate differentials and integrals. In his *Analyse des infiniment petits* of 1696, l'Hôpital presents one of the first instances of a so-called "differential triangle." Using a construction with such a triangle, and Leibniz's infinitesimal analysis, l'Hôpital gives a geometrical presentation of differentiation as follows (in what I will call Figure 3, since he has so labeled it):[154]

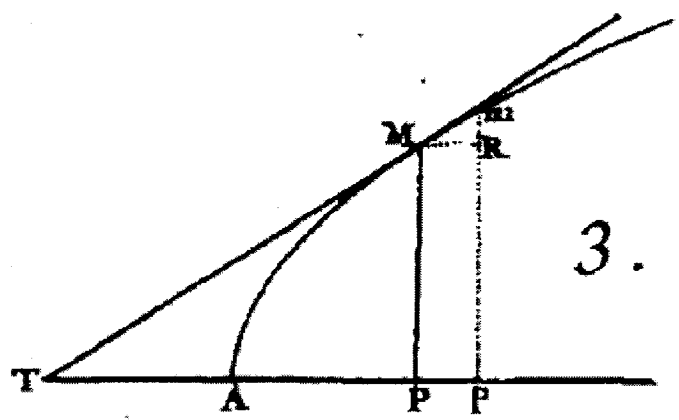

Figure 3. From l'Hôpital 1696.

L'Hôpital gives the equation of the curve AM in Figure 3 as $ax = y^2$ (that is, x times some constant equals y times y), but says his reasoning will work for any equation. The problem of finding the tangent to the curve, l'Hôpital argues, is equivalent to the question of finding the length of the subtangent PT. Take AP to be x, or the distance the curve travels on the x-axis, and PM to be y for the same reason. Then Pp or MR is the increment dx on the graph, and Rm is dy (Rm is the slope). Leibniz

[154] The following presentation owes a great deal to Mancosu 1996. The original presentation is found in l'Hôpital 1696, *Analyse des infiniment petits, pour l'intelligence des lignes courbes.* À Paris, de l'Imprimerie royale : Section II, Proposition One, p. 11.

indicates the infinitesimal increase in velocity of the tangent point to the curve as "dx" and "dy" (along the x and the y axes respectively; these terms refer to what one would now call an increment. Since ΔMTP (the triangle with MT as hypotenuse) is similar to ΔmMR,

$$dy: dx :: MP : PT \qquad (1)$$

since dy is Rm and dx is MR. That means, then, that:

$$dy : dx :: y : PT, \qquad (2)$$

and thus,

$$PT = ydx/dy \qquad (3)$$

Now the equation of the parabola itself is $ax = y^2$. In his presentation of the infinitesimal calculus Leibniz set out a number of axioms for the manipulation of differentials (dx and dy in the equation). One of these axioms, Axiom I, has it that "If a is constant, then da = 0 and d(ax) = adx."[155] By Leibniz's Axiom I, then, as well as Axiom VI, which is equivalent to the Power Rule,

$$adx = 2ydy \qquad (4)$$

and so:

$$dx = 2ydy/a \qquad (5)$$

Using the earlier result PT = ydx/dy, we get:

$$PT = 2y^2/a \qquad (6)$$

And since $y^2 = ax$, finally,

$$PT = 2x \qquad (7)$$

which is the length of the subtangent.

[155] Leibniz, *Nova methodus pro maximus et minimus*, 1684, reprinted in Gerhardt 1855.

Leibniz's "Axioms" for the manipulation of dx and dy as quantities, given in the *Nova methodus* of 1684, are the source of the apparent ease with which l'Hôpital makes the above calculation. The objection to the method used above, though, is that dx and dy are not similar to any other kind of quantity. The axioms are rules for the manipulation of quantities that, Leibniz claims in that same text, are incomparable to any finite quantity. Sometimes he refers to them as "well-grounded fictions." What, then, asked many objectors, is the logical justification for applying these axioms or rules for manipulation to these quantities?[156]

In the above case we do not even need to engage in the heretical practice of "dropping" dx or dy from the equation—it is eliminated in the course of the calculation. I use this example to demonstrate only the use of the so-called "differential triangle," in which the x-axis is divided into an infinite number of asymptotes. Why? The differential triangle in Figure 3 can be constructed only if, for any point on the circle, its increment dx can be mapped on the x-axis. This increment is the smallest possible increase in velocity of the point on the curve (what is now called an increment), and is mapped as an asymptote to the tangent point. Note, in reference to the above point about differentials as abnormal quantities, that the differential triangle does not have a normal existence on the numerical grid. Differentials such as dx and dy are not real numbers, they are aids to calculation. Here, l'Hôpital announces, the differential terms "dx" and "dy" do not have the same properties as the integers or even as real numbers. Various justifications are given for this startling practice: dy is "negligible," it is "smaller than any assignable quantity," or, in l'Hôpital's apt terms, it is a "variable" quantity. Leibniz, as we will see later, will finally argue that differentials are simply incomparably small when put in a ratio with other numbers, as a grain of sand is negligible when compared with the moon. In all these cases, one can sympathize with Berkeley, who in the *Analyst* argues that these quantities (such as dx and dy) are not numbers at all.[157]

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[156] These objectors included Bernard Niewentijt, Samuel Clarke, George Berkeley, and a host of others. For details see Jesseph 1999.

[157] "Now to conceive of a quantity infinitely small, that is, infinitely less than any sensible or imaginable quantity, or than any the least finite magnitude is, I confess, above my capacity. But to conceive a part of such infinitely small quantity that shall be still infinitely less than it, and consequently though multiplied infinitely shall never equal the minutest finite quantity, is, I suspect, an infinite difficulty to any man whatsoever" (Berkeley 1992 [1734], §5).

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APPENDIX

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