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Hermann Cohen's *Das Princip der Infinitesimal-Methode:* The history of an unsuccessful book

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ABSTRACT

This paper offers an introduction to Hermann Cohen's *Das Princip der Infinitesimal-Methode* (1883), and recounts the history of its controversial reception by Cohen's early sympathizers, who would become the so-called 'Marburg school' of Neo-Kantianism, as well as the reactions it provoked outside this group. By dissecting the ambiguous attitudes of the best-known representatives of the school (Paul Natorp and Ernst Cassirer), as well as those of several minor figures (August Stadler, Kurd Lasswitz, Dimitry Gawronsky, etc.), this paper shows that *Das Princip der Infinitesimal-Methode* is a *unicum* in the history of philosophy: it represents a strange case of an unsuccessful book's enduring influence. The "puzzle of Cohen's *Infinitesimalmethode*," as we will call it, can be solved by looking beyond the scholarly results of the book, and instead focusing on the style of philosophy it exemplified. Moreover, the paper shows that Cohen never supported, but instead explicitly opposed, the doctrine of the centrality of the 'concept of function', with which Marburg Neo-Kantianism is usually associated.

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1. Introduction

Hermann Cohen's *Das Princip der Infinitesimal-Methode* (Cohen, 1883) was undoubtedly an unsuccessful book. Its devastating reviews are customarily mentioned in the literature, but less known and perhaps more significant, is the lukewarm, and sometimes even hostile, reception the book received from Cohen's early sympathizers. Some members of the group dissented publicly, while others expressed their discomfort in private correspondence. Nevertheless, *Das Princip der Infinitesimal-Methode* has been enormously influential in the history of Neo-Kantianism. Despite its cumbersome style and shaky conclusions, the book seems to have emanated an almost totemic aura inside the little group of scholars gathered around Cohen, which, at the turn of the century, would become the 'Marburg school'. While the members could not endorse Cohen's results without reservations, caveats, or qualifications, they still had to defend the book from attacks and sarcastic comments coming from outside Marburg, as though the identity of the entire school were being threatened. To use Gregory B. Moynahan's expression, one could call the surprising impact of this fundamentally unsuccessful book "the puzzle of Cohen's *Infinitesimalmethode*" (Moynahan, 2003, 3).

This paper was written with the conviction that the time is now ripe to address, if not solve, this puzzle. In the last several decades, interest in Marburg Neo-Kantianism seems to have spread to English-speaking historiography of philosophy (Makkreel & Luft, 2010). Historians focusing on German post-Kantian philosophy have begun to extend their interests beyond the golden age of classical idealism (Beiser, 2014). Those working on the emergence of twentieth-century philosophy of science have dedicated increasing attention to the role played by the Marburg school (cf. e.g., Friedman, 2000, 2010; Heis, 2011; Ryckman, 2005). Scholars often seem to unilaterally focus on Ernst Cassirer and sometimes Paul Natorp—the other leading Marburg figures—but the translation of Andrea Poma's monograph (Poma, 1997) has reintroduced Cohen's work into the discussion. Recently, even *Das Princip der Infinitesimal-Methode* itself has attracted some interest, from the perspective of the history of ideas and culture (Moynahan, 2003), of the history of philosophy (Edgar, 2014) and also the philosophy of

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mathematics (Mormann & Katz, 2013). However, as far I can see, the literature still needs a detailed reconstruction of the path that led Cohen to write *Das Princip der Infinitesimal-Methode,* and more importantly, a history of its reception among both the 'big shots' of the Marburg community, and the minor but relevant figures gravitating around Cohen at the time (even if Schulthess, 1984 is still an invaluable source). The present paper seeks to fill this gap, and to offer tools to chip away at, if not quite break through, the infamous impenetrability of Cohen's prose.

This paper will hew to the following chronology. After an early attempt to read Kant's principle of the Anticipations of Perception through the lens of nineteenth-century psychophysics, at the turn of the 1880s Cohen (1883) became convinced that he should change course and try to understand Kant's second principle by historically reconstructing the discovery of the infinitesimal calculus (Section 1). Despite the negative reception of *Das Princip der Infinitesimal-Methode* both within the Marburg community and outside of it (Section 2), in the late 1880s Kurd Lasswitz (1890) adopted Cohen's approach in his own historical research, with some success (Section 3). Although Cohen (1896) was less than enthusiastic, at the turn of the century Lasswitz's insight was echoed in the young Cassirer's Leibniz monograph (Cassirer, 1902). In this book, despite using Cohen's language, Cassirer could not hide his dissent (Section 4). After the publication of the first volume of Cohen's system of philosophy (Cohen, 1902), by the 1910s the Marburg community seems to have split into two factions—an orthodox Cohenian front represented by Dimitry Gawronsky (1910), and a critical front containing the most representative members of the school, Natorp (1910) and Cassirer (1910) (Section 5). Cohen himself did not fail to notice the latter fact. By the time of Cohen's retirement in 1912, the school, while celebrating his work, was rife with internal tensions and conflict, a portentous sign of its decline (Section 6).

In retrospect, *Das Princip der Infinitesimal-Methode* seems to have been an unsuccessful book with a small and controversial legacy. Outside Neo-Kantian circles it never rose to the status of a respected monograph on the history and philosophy of the differential calculus, in contrast to other products of the Neo-Kantian historiography of science (Cassirer, 1906a, 1907a; Lasswitz, 1890). In addition, within the Marburg community, Cohen's philosophy of the infinitesimal calculus seems to have been a source of embarrassment. Even the most sympathetic readers were puzzled by Cohen's mystical use of the 'differential' dx as the *origin* of the finite quantitative difference x. As in mainstream presentations of the calculus, they insisted, Cohen should have emphasized the role of the differential quotient dy/dx, in which the *relation* between the finite differences y/x is preserved even when they vanish. After all, as Natorp and Cassirer pointed out, among others, this is the clearest historical example of the fact that in the exact sciences the relations are independent from the *relata*. For those who are used to considering this the core tenet of Marburg Neo-Kantianism, it might come as a surprise that this was not the message Cohen wanted to convey—as some of his other students, like Gawronsky, did not fail to realize. Thus, paradoxically, *Das Princip der Infinitesimal-Methode* played no role—or possibly just the role of a hindrance—in the emergence of the opposition between the 'concept of function' and the 'concept of substance', which Cassirer made a trademark of the Marburg School as a whole.

Nevertheless, *Das Princip der Infinitesimal-Methode* was an undeniably influential book, and its towering presence has loomed over the entire history of Marburg Neo-Kantianism. 'The puzzle of Cohen's *Infinitesimalmethode*' (Moynahan, 2003), as we will try to show, can easily be explained if one looks beyond the book itself, and instead focuses on the philosophical style it exemplified. "The special study of the infinitesimal principle," as Natorp recognized in a famous 1912 article celebrating the Marburg school, "reveals in a single glimpse the philosophical depth of [Cohen's] concern with the history of the exact sciences" (Natorp, 1912, 195). It is this detailed attention to the history of science that can be found again and again in Natorp's studies on Descartes, Galileo, etc., in Cassirer's great historical monographs, and in the works of many other minor figures. "Every contribution our school has made since then to the history and critique of the sciences," Natorp concluded rhetorically, though not without sincerity, "was the fruit of Cohen's inspiration" (Natorp, 1912, 195).

2. The history of the infinitesimal method: from *Das Princip der Infinitesimal-Methode* to the Second Edition of *Kants Theorie der Erfahrung*

On 24 February 1881, Cohen wrote to August Stadler—one his early followers from the time he was a young *Privatdozent* in Berlin in the 1870s (cf. Cohen, 1910)—that he "outlined a formulation of the Anticipations" in which Stadler's "previous concerns seem to be acknowledged and at the same time eliminated" (Cohen to Stadler, 24. Feb. 1881; Cohen, 2015, 128-129). In the previous decade Cohen had attempted to read Kant's principle of the Anticipations of Perception (A, 166-177; B, 207-218)—the second of the four principles of pure understanding listed in the *Kritik der reinen Vernunft*—against the background of psychophysics (Cohen, 1871, 215-216), the emerging nineteenth-century science that attempted to measure the intensive magnitude of sensations, by assuming that the latter are a continuous function of the stimuli producing them (Fechner, 1860; see the classical Heidelberger, 2004, for more details). In the second edition of the *Kritik*, the Anticipations of Perception attribute *a priori* to the 'real, which is an object of the sensation'[1] (B, 207) an intensive magnitude, which can increase or decrease continuously (B, 210-212). In the first edition, in contrast, the intensive magnitude seems to be attributed to sensation itself (A, 166). In Cohen's reading, Kant's new formulation was motivated by the need to indicate the 'real' as an objective correlate of sensation that exerts an influence on the senses, something that would play the role of the 'stimulus' in modern empirical psychology (Cohen, 1871, 215-216).

Stadler accepted this psychological reading of the Anticipations of Perception, although he was critical of Kant's *a priori* claims about the continuity of the intensive magnitude of sensation (Stadler, 1876, chap. 8). However, Stadler also forcefully denied that psychophysics (Stadler, 1878) could vindicate *a posteriori* what Kant was unable to prove *a priori* (Stadler, 1880, 585-586). Cohen's early students in Marburg addressed the question of the measurability of sensation (Darrigol, 2003) within the same framework. Adolf Elsas, a physicist by training, won a 1880 philosophical *Preisaufgabe* suggested by Cohen on the relationship between Kant's Anticipations of Perception and psychophysics (Sieg, 1994, 130f. and Holzhey, 1986, 1:381f.)—and Ferdinand August Müller wrote a dissertation on the topic under Cohen's guidance (Müller, 1882; see Heidelberger, 2004, 215ff. and 229ff. for further details).

Cohen's letter to Stadler reveals that at the beginning of 1881, at the latest, he must have realized that this approach had to be abandoned. Cohen agreed with Stadler's criticism of psychophysics (Stadler, 1880, 585-586); however he had clearly become convinced that the issue was deeper. It was not the intensive magnitude of psychological quantities that was at stake in the Anticipations of

[1] Kant distinguishes elsewhere between the 'reality that is the object of sensation' (*realitas phaenomenon*) from the 'reality that is the object of understanding' (*realitas noumenon*). Cf., e.g., Ak. 28:559. On Kant's usage of the terms 'real' and 'reality', see below fn. 3.

Perception, but the intensive magnitude of physical quantities, such as velocity. Unfortunately, the correspondence between Stadler and Cohen from this period is no longer extant (Cohen, 2015). However, reading between the lines of a monograph that Stadler finished a few years later, *Kants Theorie der Materie* (Stadler, 1883), one can glimpse Cohen's new project.

In fact, Stadler, whose previous contributions (Stadler, 1874, 1876) were strongly related to Cohen's results, was not ready to follow in his footsteps this time around. He conceded that there are passages where Kant seems to attribute intensive magnitude not to sensation, but to 'velocity' (cf. Ak. 4:540-41) or to the 'moment of velocity'—the infinitesimal tendency to fall downward at the beginning of a falling motion (cf. Ak. 14, Refl. 67, 1788-91). However, he insisted that one should resist confusing these intensive magnitudes with the ones Kant attributes to 'reality' in the Anticipations of Perception (Stadler, 1883, 37), as if Kant were trying to provide a foundation for the "objective validity of the differential calculus" (Stadler, 1883, 39): "those who make the intensive magnitude correspond with the differential," Stadler argues, without mentioning Cohen, "confuse the form with the content" (Stadler, 1883, 39).

Stadler's book was finished in October 1883. The *Vorwort* of Cohen's *Das Princip der Infinitesimal-Methode* is dated August 1883 (Cohen, 1883). Although Stadler never refers to Cohen, Cohen himself later read these passages (Cohen, 1910) as being directed towards his coming *Das Princip der Infinitesimal-Methode,* of which Stadler might have read the drafts. In a letter from 27 September 1883 to Paul Natorp—who had arrived in Marburg in the early 1880s to write his *Habilitation*—Cohen revealed that "the Infin. Principle" was finished. He planned to send it to the "evil world" in mid-October. He asked Natorp for a sympathetic reading: "justice will speak anyway" (Cohen to Natorp, 27 Sep. 1883; Holzhey, 1986, 2:148). As we will see, these words proved quite prophetic.

Even if there is no evidence that the object of Stadler's criticism was Cohen, it is undeniable that Cohen's book put forward the exact project that Stadler vehemently rejected (cf. also Cohen, 1910). By that time, Cohen, after having abandoned the framework of psychophysics, became convinced that Kant's second principle could be understood precisely by looking at the connection between the concepts of 'moment', 'intensive magnitude' and 'reality', which was suggested by the Kant passages mentioned by Stadler. According to Cohen, by establishing this connection, Kant had expressed in a *systematic* way in his principle of Anticipations of Perception, the very same problem that, from an *historical* point of view, Galileo, Leibniz and Newton had tried to answer by introducing 'infinitesimally small quantities'. Thus, Cohen's ambitious project was to use Kant's second principle to provide nothing less than a 'foundation' for what he called the 'infinitesimal method' from the point of view of the critique of knowledge (*Erkenntisskritik*)—a term which Cohen considered less prone to psychologistic misunderstanding (Cohen, 1883, secs. 8, 9 and 10).

2.1. *The infinitesimal method and the history of its discovery*

Cohen was of course aware that "starting from D'Alembert mathematicians usually seek the foundation of the infinitesimal calculus in the method of limits" (Cohen, 1883, 1). By referring to the 'method of limits', Cohen seems to have in mind the eighteenth-century literature, i.e., authors such as the Swiss mathematician Simon L'Huilier (1786), who, drawing on D'Alembert,[2] considered the method of limits as a sort of extension of the demonstration using the ancient 'method of exhaustion' (Cohen, 1883, 96). More in general, Cohen seems to include in the category of the 'method of limits' all attempts to justify the calculus without using any methods extraneous to traditional algebra: Leibniz's own 'Archimedean-style' demonstrations, Euler's identification of the differential with zero (Euler, 1748; cf. Cohen, 1883, 91ff.), Lazare Carnot's 'compensation of errors' (Carnot, 1881; cf. Cohen, 1883, 97ff.), Joseph–Louis Lagrange's method of power-series (Lagrange, 1797; cf. Cohen, 1883, 96), etc. For Cohen, these approaches represent a sort of 'repression' of a concept that was apparently unbearable to the mathematical consciousness, the concept of the 'infinitesimally small'.

Cohen does not want to question the 'internal' legitimacy of such justifications of the 'calculus' (Cohen, 1883, 121), even if he refers us to the work of Antoine Augustin Cournot (1841) for a recent alternative approach. However, he claims that such justifications conceal the authentic motive that induced the 'discoverers' of the infinitesimal 'method' to introduce a new type of magnitudes unknown to previous mathematics: "This reading obscures the discovery and its tendency; the positive, autonomous, irreplaceable element that forms the basis of this new type of magnitudes is smoothed out [nivellirt]" (Cohen, 1883, 95). A proper 'foundation' of the infinitesimal calculus from the point of view of the critique of knowledge, Cohen argues, should instead try to grasp the problem that the discoverers of the infinitesimal method had tried to solve when they were forced to introduce infinitesimally small quantities.

For this reason, Cohen states at the very beginning of his controversial booklet that "nothing seems more necessary to me, and nothing more immediately useful than following, simultaneously with the unfolding of a decisive systematic idea, its historical development" (Cohen, 1883, 3). This, as we will see, would quickly become the trademark of Cohen's early circle. During the same years, Natorp had also started to produce several influential (and philologically more accurate) sketches on what he called the 'prehistory' of criticism (Natorp, 1881, 1882a,b, 1884), by insisting on the necessity of "the treatment of the history of science from a philosophical point of view" (Natorp, 1882b, 228).

In precisely this same vein, Cohen insisted that the philosophical sense of the infinitesimal method could be clarified only by sketching the *history of its discovery*. Cohen is of course aware that "*[a]fter* [...] *the discovery* was completed and determined," the discoverers themselves attempted "to affirm and defend it [the infinitesimal method] through the *traditional* method of limits" (Cohen, 1883, 88). However, Cohen believes that the justification of calculus "from the point of view of the critique of knowledge [erkenntniskritische Beleuchtung] belongs to the *discovery* of the idea" (Cohen, 1883, 88). Cohen therefore wrote a history of the infinitesimal method and did not limit himself to abstractly determining its 'logical' foundation. This is essential in trying to make sense of his work: it is only "through the representation of scientific relations that led to the *discovery* of the calculus," Cohen insists, that "the possibility of understanding its significance from the point of view of the critique of knowledge is better guaranteed" (Cohen, 1883, 11). This emphasis on a strong relationship between transcendental philosophy and the history of the sciences would become the trademark of Marburg Neo-Kantianism, and was probably the reason why Cohen's book continued to exert an influence despite its controversial results.

Cohen's reference to the category of 'reality' was of course a major source of misunderstanding among readers unconformable with his 'Kantian' jargon. Cohen surely emphasized Kant's distinction between reality (*Realität*) and existence (*Wirklichkeit*) (Cohen, 1883, 60). Consequently, "the question of the reality of the infinitesimally small can only concern their character and value," not "their existential occurrence in the material world" (Cohen,

[2] Cf. the entry "Différentiel" in Diderot and d'Alembert, 1751–1772, 4:985ff.

1883, 55). However, Cohen glosses over Kant's peculiar usage of the word 'Realität',[3] which conflicted with its everyday and philosophical use (Holzhey, 1984). This is one reason the book remained obscure to most of his non-philosophically-trained readers. On the other hand, Cohen was convinced that by precisely following the history of the infinitesimal calculus, one could properly grasp the actual meaning of Kant's category of reality.

The premise and result of Cohen's research can thus be roughly summarized as follows: In the history of science, the infinitesimal method and the concept of the infinitely small were introduced in order to solve the problem that Kant later formulated, even if only imprecisely, in the *"the category of reality"* and *"thus in the principle of intensive magnitudes and of anticipations"* (Cohen, 1883, 14). Conversely, the meaning of the category of reality and of Kant's second principle can be understood as the expression of an unavoidable problem which emerged in the history of scientific thought: "the *lack of a foundation for the concept of the differential from the point of view of the critique of knowledge [erkenntnisskritisch] is at the same time the reason for the lacuna* that the fundamental concept of reality represents in the series of categories" (Cohen, 1883, 26). Cohen wanted to show that the 'infinitesimal method' was discovered in response to a problem that Kant tried to distill into the category of reality and the principle of Anticipations of Perception. Thus, the difficulty of *Das Princip der Infinitesimal-Methode* is, one might say, partly the result of the 'hermeneutic' circularity of the argument: Kant's second principle is the presupposition of the historical investigation, and the historical investigation serves to reveal the real meaning of the principle.

According to Cohen, in fact, *"the character of the infinitesimal magnitude* as intensive magnitude is the necessary *mediation"* (Cohen, 1883, 15) between these two tasks, "since the critical meaning of reality is *preferentially* expressed in the *infinitesimal intensity"* (Cohen, 1883, 14). Cohen admits that Kant did not explicitly point out this connection, as one might have hoped (even if several pieces of textual evidence are provided; see below Section 1.3), though this is only due to the fact *"that the identity of the intensive and the infinitesimal was a general assumption in the time of Kant"* (Cohen, 1883, 14). Making this assumption explicit could shed light on the problem that the introduction of infinitesimally small quantities was trying to solve. The foundation of the concept of the 'differential', which mathematics could not provide, can then be found in the critique of knowledge.

Cohen was convinced that the notion of the 'infinitesimal' did not emerge historically from the investigation of algebraic (infinite series) or geometrical (tangent and quadrature) problems, but rather in the context of 'mechanics', i.e., the emerging modern science of motion. He tirelessly reasserts that "[t]he mechanical motif of the concept of the differential" (Cohen, 1883, 21, 52), was *"pivotal* [ausschlaggebend]" "for the discovery of the concept of the differential" (Cohen, 1883, 21). According to Cohen, it is "precisely this new motif" that explains why "neither the geometers nor the analysts could manage what they had in their hands" (Cohen, 1883, 21). On the contrary, "in defining this concept, Leibniz and Newton met with *Galileo* and Kepler, whose genius was dedicated precisely to the other motif," namely, the mechanical motif (Cohen, 1883). Thus it is *"from the source and from the beginning of the mechanical problems"* that *"in the last instance the concept of the differential ultimately emerged"* (Cohen, 1883, 22-22). And it is precisely in this mechanical meaning that "the differential corresponds to a fundamental concept of pure *thought*, the category of reality. And the discoverers of the calculus insist clearly on this final motif, too" (Cohen, 1883, 23).

Cohen's hypothesis is not without some intuitive plausibility. In a varying motion, where velocity should be thought of as having a different value from moment to moment, it should be possible to determine the velocity of a moving body in every indivisible instant. In the case of a uniform motion, "velocity can still initially be represented in its sensible primitiveness [in sinnlicher Naivetat]" as a change of position in time (Cohen, 1883, 49). On the contrary, "in the case of *acceleration* […] one cannot avoid assuming infinitesimals from the beginning" (Cohen, 1883, 49). The velocity has to be defined in the instant where no change of position occurs. This is what Galileo sensed, but was not able to fully express conceptually:

> The first prototypical example of force is the law of a falling body. And thereby we recognize the method of the infinitesimally small quantities as the creative fundamental concept […] On the other hand it could appear odd that Galileo already used the instrument that he needed, even if others after him had introduced it. […] [However,] in spite of the geniality of Galileo's presentation, he was not able to fully cover the conceptual gap that separates the indivisible from the differential. His shortcomings are testified to by the expressions that Galileo accumulates in order to distinguish the emerging motion from the finished one. *L'Impeto, il talento, l'energia, il momento del discendere*[4]*; il momento o la propensione al moto*[5]*; quali furono gl'impeti… tali… i gradi.*[6] Through these expressions the tendency [to move] is sensibly described but not conceptually determined. Leibniz seems to have grasped this lack of clarity by using the expression 'embryonal impetuosity' in the very interesting passage in which he touches on his relationship with Galileo (cf. GM, 4:159). Indeed, it is the infinitesimal acceleration not the embryo, but the productive reality (Cohen, 1883, 51).

In finding the laws of a falling body, Galileo had to define the tendency of a body to continue with the same velocity and direction, even in an instant in which there is no motion (no change of position); Galileo saw the need for a new conceptual tool, which he tentatively defined as 'the impetus, energy, ability, the moment of descent'.

According to Cohen, Galileo is expressing in a confused way the "desideratum of this epoch-making thought" (Cohen, 1883), of the concept that should have been introduced to make 'motion' the object of the mathematical science of nature. Already at this point, in the law of free fall, "the infinitesimal principle is revealed as creative," and for this reason we regard it "as a fundamental *mechanical concept* [mechanischen Grundbegriff]" (Cohen, 1883, 47). In some sense, "before the concept of the differential was secured, its effectiveness and validity were latent" in Galileo's 'new' science of motion, (Cohen, 1883, 51) and precisely in the 'productive' meaning that was at Cohen's heart (Cohen, 1883, 45): a falling body, dropped from a position of rest, has to pass through 'infinite degrees of slowness', so that a finite momentum might be considered as an accumulation of momenta, which are assumed to be infinitely small with respect to ordinary motion (Dühring, 1873, 30ff.).

[3] Kant adopted the word usage which was standard in the Leibnizian–Wolffian tradition (cf. e.g., Heinekamp, 1968; Maier, 1930). The term *realitas* is derived from *res*, thing. Being 'real' was synonymous with being something in opposition to something else (negation). Kant attempted sometimes to render *realitas* with *Sachheit* or *Dingheit* (thingness) (cf. e.g., Ak. 28:1145; see also Giovanelli, 2011).

[4] Galilei, 1855–, 13:174.

[5] Galilei, 1855–, 13:176.

[6] Galilei, 1855–, 13:177.

2.2. Leibniz as the discoverer of the infinitesimal calculus

Cohen's allusion to the relationship between Galileo and Leibniz at the end of the long passage just quoted is essential to his argument. His interpretative strategy is particularly clear in his analysis of Leibniz's contribution to the discovery of the calculus. Cohen maintains that, even in Leibniz's case, the first formulation of the concept of the 'differential' through algebra and geometry is only the premise for the solution of the authentic problem: "If we want to understand the concept of the differential in the principled meaning that it had for its discoverers, we must direct our attention to the interest from which this concept has emerged" (Cohen, 1883, 54). Also in the case of Leibniz the "third motif [the mechanical motif after the geometrical and arithmetic ones]" is the one we regard as "the decisive motif itself for the discovery of the concept of the differential" (Cohen, 1883, 47). Cohen speaks of a "prevalent [vornehmliche] tendency towards mechanics," of a "converging in this of all operations with the differential" into mechanics (Cohen, 1883, 63). The significance of the infinitesimal "was first appreciated in the purest form through the discovery of *geometry*. But the roots of this valorization are nevertheless in its realizing value, thus in *mechanics*" (Cohen, 1883, 63).

Cohen could emphasize that "[j]ust as Galileo, Leibniz also described velocity as *intensio*." He distinguishes *diffusio actionis in motu vel actionis extensio* from the *intensio ejusdem actionis*, the striving towards some determinate motion (Cohen, 1883, 71, cf. GM, 6:355). In particular, Cohen could refer to the passages where Leibniz explicitly claims that motion, if regarded as mere geometrical change of position in a certain time span, does not have any physical reality *per se*; there is nothing real in motion except that momentary something (*momentaneum illud*)—the tendency to continue with the same velocity—which Leibniz describes as "something beyond extension, in fact, something prior to extension" (*alicquid praeter extensionem, imo extensione prius)* (Cohen, 1883, 71; cf. GM, 6:235). In Cohen's reading, Leibniz introduced the notion of the 'differential' as a means for conceptually grasping the *momentaneum illud* that is beyond extension (Cohen, 1883, 71).

In the context of this unilateral appreciation of Leibniz's mechanical applications of the calculus, Cohen gives particular importance to the passages in which Leibniz considered ordinary motion that extends through time as the result of the summation of an infinite number of infinitely small increments: the dead force is the element or differential of motion, and the integration of dead force with respect to time gives rise to ordinary motion. According to Cohen, this shows the precise 'philosophical' meaning of the 'infinitesimal method': ordinary motion, "the finite inasmuch as it aspires to have objective validity" (Cohen, 1883, 144), is not simply 'given'; to become a legitimate object of scientific experience, it should be regarded as a 'result', "as the sum of those infinitesimal intensive realities, as a *definite integral,*" the infinite repetition of elementary, infinitely small elements (Cohen, 1883, 144).

In turn, such elementary increments arise from an infinite number of lower-order elements, which are 'infinitely small in comparison with the preceding', and so on (cf. GM, 6:238). If dx is the difference between two minimally different values of x—since dx is not always constant—ddx is a difference of differences, and further proceeds to $dddx$ or d^3x etc. d^ex. (cf. GM, 7:222-3). As expected, Cohen emphasized that in the differentials of higher orders the concept of the differential reveals again its mechanical meaning (Cohen, 1883, 73); if the first differential is speed, the infinitesimal distance traveled by a body in an instant, the second is acceleration, an infinitely small change in a body's speed (Cohen, 1883, 74).

Cohen was of course aware that Leibniz, on many occasions, deemed the infinitesimal a *modus loquendi*, a *façon de parler*, since, properly speaking, there are no 'infinitesimal magnitudes' (GM, 5:289). However, Cohen tried to downplay this undeniable tendency by claiming that it was the consequence of Leibniz's "own lack of confidence" in light of the "prejudices of his time" (Cohen, 1883, 88). For this reason, he tried to defend his calculus "with the old methods" (Cohen, 1883, 88). Even if "Leibniz had emphasized the productive force of his concept enough," he nevertheless "did not want to dispense with the advantage of presenting his procedure to the public opinion as equivalent to the method of limits" (Cohen, 1883, 78). As a result, Leibniz insisted that he was able to reformulate any proof involving infinitesimals into a proof in the style of Archimedes (cf., eg., GM, 1:322): "as if the differential had nothing more definite to say than the limit of the differences" (Cohen, 1883, 78).

Paradoxically, Cohen's reading of Leibniz is particularly interesting from our point of view because in many respects it is untenable. Leibniz discovered the calculus in the context of the theory of number sequences, then extended it to the sequences of ordinates and abscissas (Bos, 1974, 1986). It was only later that Leibniz applied his calculus to mechanical problems. Moreover, Leibniz never defended an 'accumulative' conception of the continuum as a sum of infinitesimally small quantities, an approach he took in his writings on physics only as a manner of speaking (Rutherford, 2008). In marked contrast, he saw the continuum as an inexhaustible possibility of division, where the whole is given prior to the possible parts; and thus for Leibniz there is neither the infinitesimally small nor the infinitely large (Breger, 1986). This is probably Leibniz's most important contribution to the topic, and Cohen completely misunderstood it.

However, Cohen's attempt to force Leibniz into his conceptual scheme makes features of the latter more evident. Cohen wanted to show that Leibniz discovered the calculus to conceptually establish the *momentaneum illud*, which confers physical reality to geometric motion, which otherwise would be a mere relative change of position. According to Cohen, this is the same problem Kant cryptically expressed in his second principle, by establishing a relation between reality, intensive magnitude, and the mathematical concept of the 'moment'. Through this connection one can see, at the same time, *"the meaning of the category of reality and the secret of the concept of the differential*" (Cohen, 1883, 28).

2.3. The Second Edition of Kants Theorie der Erfahrung

Cohen's second revised and augmented 1885 edition of his *Kants Theorie der Erfahrung* (Cohen, 1885) incorporated many of the innovations he had developed in the previous decade. It was actually an entirely new book, in which Cohen, in an often precarious equilibrium between textual analysis and systematic reconstruction, attempted to rebuild the entire apparatus of Kant's philosophy in stronger idealistic terms. Cohen did not just insist on the central role of the synthetic principles (in opposition to the categories; cf. Edel, 1988) as the condition of the possibility of mathematical the science of nature (as epitomized in Newton's *Principia*). He also made the more hazardous claim that "the arrangement of the principles could become more transparent, after it was discovered that the principle of the intensive magnitude is the key principle" (Cohen, 1885, XII).

Cohen thought he had showed that it was only by reconstructing the history of the infinitesimal method that one could grasp the problem that formed, at least *in nuce*, the basis of the Anticipations of Perception. To provide evidence for this claim, Cohen referred his readers to several passages in which Kant seems to move in this direction. In formulating the principle, Kant argues that "if one regards this reality as a cause," then "one calls the degree of reality as a cause a *'moment*', e.g., the moment of gravity" (B, 210), that is, the moment dv of acceleration of a free-falling body. According to

Cohen, in Kant's use of the concept of a 'moment', one can glimpse "the mechanical motif, which ultimately produced intensive magnitude as differential" (Cohen, 1885, 436).

In the second "Analogy of Experience" Kant returns to the concept of 'moment', affirming that change is "possible only through a continuous action of causality, which, insofar as it is uniform, is called a moment" (B, 254). An alteration "does not consist of these moments, but it is generated through them as their effect" (B, 254). In these passages, although "Kant does not refer anywhere to inertia" (Cohen, 1885, 465), according to Cohen, he probably uses the term 'moment' to indicate the tendency of a body to continue in rectilinear and uniform motion, if the acceleration were to vanish (Cohen, 1885, 464-465). The finite velocity acquired by a body can then be described as the effect of the successive accumulation of such momenta.

Referring to a 1789/90 *Reflexion* on the moment of speed at the beginning of free fall (Ref. 67, Ak. 14:497), which Stadler had already mentioned, Cohen emphasizes (Cohen, 1883, 111; Cohen, 1885, 430) that Kant defines the moment of velocity (or better, the moment of acceleration) "not as velocity itself," but as "the tendency of bodies to communicate a certain velocity". Even more significantly, Kant defined it not as "extensive, but as an intensive magnitude," (Ref. 67, Ak. 14:497). Thus the finite 'motion' is generated, starting from rest, as the sum of the infinitesimally small motions; if the moment of velocity was zero,[7] "through their summation there would not emerge any finite magnitudes" (Ref. 67, Ak. 14:497). Therefore, as Kant put it in another *Reflexion* from the late seventies, the "production of reality has a moment of the *qvanti extensivi* which is a sort of element: *differentiale*" (Refl. 5582, Ak. 18:240). Just as different moments produce different degrees of velocity, Kant continues, so do different impressions produce different degrees of sensation (Refl. 5582, Ak. 18:240).

These are precisely the passages Stadler had quoted in his 1883 monograph (see Section 1), where he tried to show that it was incorrect to identify the intensive magnitude Kant attributes to speed with the intensive magnitude he attributes to reality. Cohen, as Stadler had sensed, unabashedly embraced the opposite interpretation, and saw in these *Reflexionen* one of the key passages to support his interpretation. Cohen nevertheless acknowledged his debt to Stadler. It was Stadler who led him to completely rethink his own interpretation of the Anticipations of Perception (Cohen, 1885, 437). However, Stadler still thought that psychophysics offered the conceptual framework to understand the problem Kant was facing (Cohen, 1883, 156; see also Elsas, 1885, 146). According to Cohen, however, this framework was inadequate for understanding Kant's concept of the "intensive": "the so-called intensity of sensation must absolutely be distinct from the intensive magnitude or reality of sensation" (Cohen, 1883, 156).

3. Hatchet jobs: Cohen's book and its reviewers

Cohen's attempt to interpret the 'differential' as 'intensive magnitude', or even as 'intensive reality', immediately prompted negative reactions in the mathematical community. At the end of 1883, Georg Cantor sent a review of Cohen's book to the *Deutsche Literaturzeitung* (Cantor, 1884). Cantor recognized Cohen's book as a "clever and learned work" (Cantor, 1884, 267). However, he dissented strongly from Cohen's insistence on the 'reality' of the infinitesimals. Whereas every rational or irrational number is continually well-defined, and is in some sense part of the 'being', in contrast, "the so-called infinitely small numbers or differentials do not belong to the sphere of the *being*," but rather to those of the "*non-being, changing, becoming*" (Cantor, 1884, 267): a quantity is not infinitely small, but rather becomes infinitely small (decreases without bound toward zero). They cannot be conceived of "as proper magnitudes, as it happens here, even if only in the sense of the intensive or protensive magnitudes" (Cantor, 1884, 267). According to Cantor, this was also Leibniz's opinion; in particular, though, it was Gauss who considered the differential as a "variable finite" (Cantor, 1884, 267).

Cohen was actually pleased that Cantor bothered to review the book and he wrote him a letter expressing his gratitude (Sieg, 1994, 143). On 21 February 1884, Cantor wrote to Kurd Lasswitz: "some days ago I received from Cohen a nice [letter] about my review, with some counter-arguments that I do not really understand" (Cantor to Lasswitz, 21 Feb. 1884; Eccarius, 1985, Doc. 7, 19). Lasswitz's reply has not been preserved. However, it seems plausible that he tried to defend Cohen's point of view. In fact, in a letter dated 9 March 1884, Cantor replied with the following: "for the time being, I maintain my objection that the differential cannot be conceived as an *autonomous* magnitude" (Cantor to Lasswitz, 21 Feb. 1884; Eccarius, 1985, Doc. 8, 20). According to Cantor, the differential is only a sign that a certain finite magnitude is conceived of as a variable that decreases indefinitely in such a way as to fall below any given number, i.e., that it has zero as a limit. For this reason, "the meaning of the differential lies in the differential-*quotient*, which is nothing but the limit of the difference-quotient" (Cantor to Lasswitz, 21 Feb. 1884; Eccarius, 1985, Doc. 8, 20). This issue, as we shall see, returns again and again in discussions of Cohen's book.

Cantor's letter is a good example of the standard reaction of the mathematical community (see e.g., Günther, 1884): in his criticism of the 'method of limits', Cohen seemed to have been completely unaware of the process of 'rigorization' of the analysis developed in nineteenth-century mathematics (Cantor, 1872, 1879; Dedekind, 1872) culminating in Weierstrass's so-called '*Epsilon- tik*' (cf. e.g., Bottazzini, 1986). Even if other non-mainstream approaches were present in the mathematical literature at the time (cf. e.g., Stolz, 1883; see Ehrlich, 2006 and, with reference to Cohen, cf. Mormann and Katz, 2013), it is not evident in the early reception of Cohen's book. In fairness, though, it must be emphasized that Cohen's mathematician critics were probably not sufficiently comfortable with his terminology to grasp the philosophical point he was trying to make. Thus, Cohen's sympathizers, such as Elsas, took some pains while reviewing the book to 'translate' Cohen's philosophical jargon "for the non-philosophically-trained mathematicians and physicists" (Elsas, 1884, 560). Nevertheless, also philosophically sophisticated mathematicians like Gottlob Frege (Gabriel, 1986) simply could make no sense of Cohen's parlance (Frege, 1885).

However, even among his own acolytes, Cohen's book was not well-received. Stadler, as we have seen, had already attempted to refute *ante litteram* Cohen's interpretation, and apparently would never agree with it in subsequent years, despite their continuing friendship (Cohen, 1910; see also Cohen, 2015 for their late correspondence). The most critical among his students was surely Ferdinand August Müller (see above Section 1). In his *Das Problem der Continuität im Mathematik und Mechanik* (Müller, 1886), he offered a polite yet severe criticism (he would be much less polite in private correspondence; see below in this section) of Cohen's treatment of the differential dx as something isolated from the differential quotient $\frac{dy}{dx}$, and what is more endowed with an intensive magnitude (Müller, 1886, 96n.; 99-100).

This criticism of Cohen was also raised by much more sympathetic readers, like Lasswitz. In 1885 he wrote a positive review of Cohen's book. Although he complained about "the difficulties of the subject matter and obscure writing style" (Lasswitz, 1885, 494), Lasswitz accepted Cohen's main claim that "the infinitesimal

[7] Cohen interprets this passage as a critique of Euler's conception of the differentials as zero (Cohen, 1883, 111).

concept of the differential" has "its historical origins in mechanical problems" (Lasswitz, 1885, 499). Lasswitz was ready to recognize "the infinitesimal reality as a highly effective mean of thinking [Denkmittel]," even if one still "doubts the relationship with [the symbol] *dx* [as used by] the mathematicians" (Lasswitz, 1885, 502).

Lasswitz in fact wondered whether the intensive reality "can really be identified with the mathematical differential or rather with the functional connection with the differential of a second variable," that is, in the relation between *dx* and *dy* in the differential quotient (Lasswitz, 1885, 502). This is particularly evident in the case of the differentials of higher order (as Cantor pointed out to Lasswitz in their correspondence). In the d^2x, d^3x, etc., there is nothing qualitative that can distinguish them from one another: If *x* is a length, then d^2x, d^3x, etc., are also lengths smaller than any given lengths. Only in the *differential quotient*, when set in relation to other infinitesimally small quantities, do differentials assume a qualitative meaning. Thus if $dx: dt$ is a velocity, then $d^2y: dt^2$ is an acceleration; if $dy: dx$ is the slope of a tangent, then $d^2y: dx^2$ is the curvature, etc. (Lasswitz, 1885, 502).

Cohen was of course pleased by Lasswitz's review. As he admitted in a letter of 7 April 1886, he had not been "spoiled" by positive reviews (Cohen to Lasswitz, 7 Apr. 1886; Holzhey, 1986, 2:166). Cohen made clear that he did not want to contradict the mainstream presentation of calculus: "the terminology of the Diff.-Calculus is correct and absolutely compatible with the intensive reality" (Cohen to Lasswitz, 7 Apr. 1886; Holzhey, 1986, 2:166). He suggested that in a second edition of *Das Princip der Infinitesimal-Methode* he would have shown "how he thought it was possible to express the elementary Diff.-Calculus and its rules in this language," that is, in the language of the 'intensive reality' (Cohen to Lasswitz, 7 Apr. 1886; Holzhey, 1986, 2:166). Lasswitz's positive review, however, did not seem sufficient to improve the chances of Cohen's book (see e.g., Husserl to Brentano, 29 Dec. 1886; Husserl, 1994–, 5). Unsurprisingly, the Marburg community soon became aware that a more accessible presentation of Cohen's philosophy of the infinitesimal method was needed.

Elsas pointed this out in writing to Lasswitz at the beginning of 1887, when he thanked him for the review (Lasswitz, 1887) of his book (Elsas, 1886). One could have taken into account the hostility coming from outside Marburg; but Müller's attacks, "who for some years was part of our restricted circle" came as a wake-up call (Elsas to Lasswitz, 7 Jan. 1887; Holzhey, 1986, 2:171). Elsas confessed to Lasswitz "that Cohen's exposition could be made transparent only through strenuous intellectual work" (Elsas to Lasswitz, 7 Jan. 1887; Holzhey, 1986, 2:172). It was not easy "to translate the scholastic language of philosophers into the poorer language of physicists" (Elsas to Lasswitz, 7 Jan. 1887; Holzhey, 1986, 2:171). Elsas was ready to act as an interpreter, despite the fact that, because it lay on the boundary between the two disciplines, this work of mediation was not appreciated by his colleagues in the physics department. However, he confessed that "often I myself am not sure if Cohen really means and says what I read off or hear from his elliptic remarks" (Elsas to Lasswitz, 7 Jan. 1887; Holzhey, 1986, 2:172).

A month later, on 8 February 1887, in another letter to Lasswitz, Elsas again expressed his uneasiness about Cohen's work, adopting an ambiguous attitude that can be found again and again among members of the Marburg school. Elsas felt entitled to defend, explain and translate Cohen's book for the world outside Marburg (cf. Elsas, 1886, fn. 35, 75-f.); at the same time, he was uncomfortable with the obscurity and paradoxicality of Cohen's foundation for the infinitesimal analysis. Elsas admitted that "in Cohen I am missing a concise formulation that could substitute for Kant's failed 2nd principle" (Elsas to Lasswitz, 8 Feb. 1887; Holzhey, 1986, 2:173). Most of all, Elsas conceded to Lasswitz that the relation between Cohen's use of *dx* and that of the mathematicians was unclear: "Cohen should not have used the math. sign". Elsas agreed with Lasswitz that *dx* has no meaning without the relation to a *dy*: "But what remains of the 'principle of connection' between *dx* and *dy, x* and *y*? You feel that there is something missing here. I cannot help you, because I have the same feeling and I'm myself looking for something that fills the gap. Maybe there is nothing of the sort" (Elsas to Lasswitz, 8 Feb. 1887; Holzhey, 1986, Br. 12, 2:182).

It was Lasswitz who, in a long 1888 paper, "Das Problem der Continuität" (Lasswitz, 1888a) tried to 'fill the gap'. In order to make the scientific concept of motion possible, Lasswitz explains, it is necessary to express the tendency of a body in motion to continue with the same velocity, a tendency that can be defined even in the durationless instant, i.e., even when there is no change of position, and thus no motion. Without this dynamical element, motion would not be a physical object. This element beyond extension therefore represents the "reality" of motion, what distinguishes physical motion from mere geometrical change of position. For this reason, according to Lasswitz, "the category of reality" (Kategorie der Realität) corresponds to what he preferred to call the "means of thinking of variability" (Denkmittel der Variabilität) (Lasswitz, 1888a, 29).

In Lasswitz's words, "without the means of thinking of variability the flying arrow would be at rest at every point of its path" (Lasswitz, 1888a, 29), that is, the very definition of physical motion would be impossible. However, he also notices that "the connection of the principle of the intensive magnitude and the category of reality with the infinitesimal method indeed only becomes clear via the relations to the *concept of function*" (Lasswitz, 1888a, 29). Although Lasswitz defended Cohen from Müller's criticisms, he pointed out that "the constitutive, realizing character" can be found not "in the differential as such, but only in the differential that is the subject of a differential equation" (Lasswitz, 1888a, 30), $dy = f'(x)dx$. Lasswitz's remarks were intended to be an explanation of Cohen's point of view: "the umbrage that mathematicians have taken at Cohen's interpretation of the *dx* should be eliminated through the distinction that we have tried to make" (Lasswitz, 1888a, 30).

After receiving Lasswitz's paper, Müller was clearly still unconvinced. He replied to Lasswitz on 26 October 1887, in an amicable tone (Müller to Lasswitz, 26 Oct. 1887; Holzhey, 1986, Br. 13; 172), but he did not refrain from calling the very notion of 'intensive reality' "one of the most monstrous births in the entire history of philosophy" (Müller to Lasswitz, 26 Oct. 1887; Holzhey, 1986, Br. 13; 172). Such a harsh judgment from an ex-student might suggest that personal issues were at stake. However, it is undeniable that those who were ready to concede the 'honors of war' to Cohen's book were forced to extract the valid parts of his contribution, explaining away the more questionable ones, such as the identification of the 'differential' *dx* as such with the 'intensive reality'.

As we will see, however, Cohen always insisted on considering the 'differential' as such, in isolation from the differential quotient, as a specific philosophical problem encoded in the category, or later, the 'judgment' of reality. However, this was clearly not accepted at face value by even in the restricted Cohen-circle. Natorp, the thinker closest to Cohen, in an 1891 programmatic paper (Natorp, 1891a), emphasized that "central concept in calculus is the 'differential quotient'"; Despite Cohen's insistence, "the 'differential' does not need any particular foundation" (Natorp, 1891a, 153).

4. Kurd Lasswitz and the urbanization of the Cohenian Province[8]

During the 1880s, a group of scholars began to gather around Cohen's Marburg chair. Natorp was surely the most successful, as he

[8] The title of this section alludes to Habermas, 1979.

had already published several respected papers and monographs (Natorp, 1884, 1887, 1888). However, outside of the Marburg school, the previously mentioned Stadler from Zurich and the *Gymnasiallehrer* Lasswitz from Gotha were also engaged in fruitful dialogue with Cohen. Stadler did not follow Cohen after his 1883 'idealistic' turn, but Lasswitz made an enormous, though largely forgotten, effort to implement Cohen's interpretation of the infinitesimal calculus in his own historical work and to translate Cohen's insight into a language more accessible to those unfamiliar with his often obscure jargon. The first part of an 1888 paper on Galileo's theory of matter (Lasswitz, 1888b) marks one of the first examples of how Lasswitz regarded Cohen's work on the infinitesimal calculus.

4.1. From Galileo to Huygens: Lasswitz's history of Atomism

"The main difficulties" that Galileo faced in the definition of the concept of motion, Lasswitz argues, is that "[m]otion presupposes the course of time" (Lasswitz, 1888b, 461), namely, it is a change of position in time. Thus, in every instant where there is no change of position, there is in fact no motion. There must therefore be something that defines motion as a physical object in the durationless instant as well: there must be "something in the instant of time that differentiates the body in motion from one at rest, a reality of motion that cannot be eliminated when we abstract from time" (Lasswitz, 1888b, 461). This can be found "in the dynamical capacity of producing effects of the body in motion" (Lasswitz, 1888b, 461). The body in motion, considered in the instant, is essentially different from the body at rest, "though not through an extensive magnitude, which is eliminated by the abstraction, but rather through an intensive one" (Lasswitz, 1888b, 461).

At this point, according to Lasswitz, a new conceptual problem arises. It is necessary to "ground this tendency to produce effects in a mathematical concept and subject it to knowledge" (Lasswitz, 1888b, 462). Lasswitz explicitly refers to Cohen's monographs (Cohen, 1883, 1885) "for the connection between the concept of continuity and of intensive magnitude, as well as for the relation to Kant's thought" (Lasswitz, 1888b, 462n.). According to Lasswitz, the great achievement of Galileo's concept of 'moment' was precisely that it tried to provide an expression for this conceptual element (cf. also Galluzzi, 1979).

In 'statics', when bodies are in equilibrium, "there is already the moment as a tendency to fall" (Lasswitz, 1888b, 470). The moment is the tendency, the impetus to move downward; bodies are trying to descend but are mutually hindered, and hence have not achieved any actual motion yet. Galileo discovered that "the concept of velocity is here already, even if only in a virtual sense" (Lasswitz, 1888b, 470). Galileo was then able to transform the 'static' concept of moment into a 'dynamic' one by postulating that one can define velocity in every arbitrarily (*beliebig klein*) small part of time. By modeling his dynamical concept of 'moments of velocity' upon the static concept of 'moments of weight', Galileo was able to understand acceleration; the finite 'moment' of a body can be regarded as composite and aggregate of infinitely many momenta, with a direct causal relationship between the accumulation of the momenta and the acceleration of the motion. "The gap between being and non-being is bridged, since the becoming is regarded as growing out of the infinitesimal increments" (Lasswitz, 1888b, 473).

In the second part of the paper, published a year later (Lasswitz, 1889), Lasswitz tried to show that the *Denkmittel der Variabilität,* which was applied successfully to *Zeiterfüllung*, 'the filling of time', cannot be expected to apply with equal success to the *Raumerfüllung*, the "filling of space" (Lasswitz, 1889). Galileo's attempt to solve the problem of the rarefaction and condensation of matter by resorting to the *rota Aristotelis*—that is, by assuming that a body could be composed of an infinite number of unquantifiable atoms, just as the total speed of a body is the sum of an infinite number of indivisibles of speed (Palmerino, 2001)—was bound to fail. According to Lasswitz, if Galileo's attempt was pursued systematically, it would lead to a dynamical conception of matter, like those later formulated by Kenelm Digby, Roger Joseph Boscovich and, of course, Kant. However, this concept of matter is incapable of explaining how "the parts of space act as wholes," that is, and thus of accounting for the subject of motion. This problem "cannot be solved through the concept of intensive reality, but only via the concept of substance" (Lasswitz, 1889, 144).

The results of these and others historical investigations were incorporated into Lasswitz's masterpiece, his monumental *Geschichte der Atomistik* (which is still worth reading), published in two volumes in 1890 (Lasswitz, 1890) and covering nearly a thousand pages. If the 'Denkmittel' of variability defines the reality of motion in the instant, the identity of the subject of motion through time should be realized only through the application of another 'Denkmittel', that of substance, that finds its expression in extended atoms. It is the causal connection between atoms that must be considered continuous, not matter itself (Lasswitz, 1890, 381). Lasswitz's philosophical 'hero' was thus Christiaan Huygens, the founder of 'kinetic atomism' and the first to clearly separate these two problems. On the other hand, according to Lasswitz, the confusion of these two means of thinking inevitably leads to Leibniz's substantialization of force, epitomized in the notion of a primitive force or monad, a metaphysical entity that tries to entail both the continuous change of states and the permanence of substance (Lasswitz, 1890, 2:580; cf. Willmann, 2012 for more details).

4.2. Cohen's reaction to Lasswitz and the concept of field

In his review of the *Geschichte der Atomistik* (Natorp, 1891b), Natorp pointed out that Lasswitz had provided perhaps the most convincing attempt "to further explain Cohen's path-breaking book about the principle of the infinitesimal method" (Elsas, 1891, 301). But as Cohen immediately recognized, Lasswitz's 'explanation' was also an 'urbanization'. It made the most paradoxical aspects of Cohen's philosophy of the 'infinitesimal' more palatable, but also watered down their philosophical implications. In particular Cohen did not fully agree with Lasswitz's choice of the term 'variability', as he explains in a letter from July 1891 (Holzhey, 1986, Cohen to Lasswitz, 27 July 1891, Br. 21, 2:202-203).

In 1896—the year Cassirer arrived in Marburg—Cohen published a long "Einleitung mit kritischem Nachtrag" to the 5th edition of Lange's *Geschichte des Materialismus* (Cohen, 1896). The paragraphs on Galileo seem to confirm that Lasswitz had grasped the spirit of Cohen's 1883 book (Cohen, 1896, XXVIIff.). Cohen in fact expressed appreciation for Lasswitz's attempts to present his interpretation "in his excellent work *Die Geschichte der Atomistik*" (Cohen, 1896, XLVII). However, he considered Lasswitz's presentation as a "weakening," where the "agreement with which he has supported my theory, has at the same time compromised it again" (Cohen, 1896, XLVII). Cohen believed that in Lasswitz's work, "the concept of reality [is reduced] to a *concept of relation of variability*" (Cohen, 1896, XLVII). However, according to Cohen, "[i]n the infinitesimal one does not find merely the origin of the magnitude, but at the same time the being itself, the real". For this reason, Cohen insists, "for the logical legitimation of the concept of the differential I have highlighted the fundamental concept of reality" (Cohen, 1896, XLVII). The proper 'being' of physical quantities is at stake, not simply their 'variability'.

Thus, Cohen considered Lasswitz's separation between 'substantiality' and 'variability'—between the discreetness that defines the subject of motion and the continuity of motion and change—to be unacceptable. In the "Kritischer Nachtrag", the first work in which Cohen attempted a more systematic confrontation with nineteenth-century physics (Hertz, Planck, etc.), Cohen seems to suggest that the development of the concept of a 'field' had made Lasswitz's distinction unnecessary. According to Cohen, in Faraday's treatment of "the problem of the acting-at-a-distance forces" (Cohen, 1896, XLV) and his "[r]eluctance towards atomism" (Cohen, 1896, XXIX), the old physics of particles has been substituted by a new physics of fields. "The fundamental hypothesis of the Faraday-Maxwell Theory" (Cohen, 1896, XXX), in Cohen's view, means "the overcoming of the problem of matter by the problem of force" (Cohen, 1896, XXIX), where matter appears as an epiphenomenon of the field. Here we observe the "greatest transformation in the concept of matter and the transformation of matter into force and energy," in which Cohen could celebrate "the victory of idealism" (Cohen, 1896, XXIX).

Cohen's reference to the field concept was brief but not isolated. In 1902, a philosophical *Preisaufgabe* was announced, in which candidates were asked to explicitly deal with Faraday's relation to Boscovich's theory of matter (Sieg, 1994, 501). The prize went to the 'cand. phil' Otto Buek (1873–1956), a German–Russian scholar born and raised in St. Petersburg, who had landed at Marburg for his doctorate (Sieg, 1994, 207ff.). Responding to the *Preisaufgabe*, Buek developed Cohen's insights about the field concept and the Maxwell-Faraday theory of electricity, in what would later become his dissertation, "Die Atomistik und Faradays Begriff der Materie", to appear in the *Archiv für Geschichte der Philosophie* in 1905 as a long article (published in 1905 as *Separatdruck*).

Even if Buek was careful not to burden his historical reconstruction of the Boscovich–Farday relationship with philosophical debate internal to Neo-Kantianism, one of his polemical targets was clearly Lasswitz's 'critical atomism'. Lasswitz had summarized Cohen's connection between intensity, reality and the infinitesimal in his 'Denkmittel der Variabilität', and applied it to motion alone, while, he had hypostatized in the extended atom the subject of motion (Buek, 1904, 161). Faraday's concept field and its attempt to reduce matter particles to the field knots could be presented by Buek as a historical example of an alternative model, in which Cohen's connection between intensity, reality and the infinitesimal could find universal application: here, "the determination fixed in the point is what is actually real" (Buek, 1904, 108).

5. The Young Cassirer's 'Dialectical' confrontation with Cohen's interpretation of the infinitesimal calculus

When Buek, one of Cohen's most beloved students (Sieg, 1994, 501, n. 74), was preparing his dissertation, the Marburg school was perhaps at the beginning of its golden age, thanks primarily to Natorp's academic engagement there (he became *ordinarius* in Marburg in 1893; cf. Sieg, 1994, 174). The *Preisaufgaben* (see above on Section 1) became a powerful tool whereby doctoral students developed some of the core insights of the school through their often sophisticated historical investigations. "Once again," Natorp wrote to one of the recipients, Albert Görland see Cassirer Bondy, 1981, 144, on 21 November 1902, "we got an excellent Preisarbeit from Mr. Bueck [sic] about Faraday; the 4th in a row ([18]96, [18]98, [19]00, [19]02, you [Görland], Cassirer, De Portu, Bueck" (Natorp to Görland, 21 Nov. 1902; Holzhey, 1986, Br. 72, 2:302). The *Preisarbeiten* were often expanded into dissertations and monographs of some value (e.g., Buek, 1904; Cassirer, 1902; Görland, 1899; Portu, 1904). Even without the wisdom of hindsight, the young Cassirer was immediately recognized as the most promising of this group of students (Ferrari, 1988).

In November 1901, Cassirer had finished transforming his thesis on Descartes into the first chapter of his Leibniz monograph *Leibniz' System in seinen wissenschaftlichen Grundlagen* (Cassirer, 1902). Cassirer dedicated the book to "his teacher" Cohen, "with sincere respect and gratitude". Both stylistically and terminologically, the book is profoundly tied to the work of Cohen and the Marburg school. As Natorp wrote to Görland on 13 January 1902, Cassirer's book "was for a good part prepared by Cohen and, if I may say it, by myself," even if "[a] part of the theses are new outside of Marburg" (Natorp to Görland, 13 Jan. 1902; Holzhey, 1986, Br. 59, 2:271). However, "I find great maturity and autonomy and also a lot is new for me" (Natorp to Görland, 13 Jan. 1902; Holzhey, 1986, Br. 59, 2:271). Cassirer's reliance on the 'Cohenschen Methode' was the explicit reason Cassirer was unable to obtain the *Habilitation* in Berlin. Natorp was surprised, however, that even Wilhelm Dilthey (one of the three full professors in the commission) was unable to see "that Cassirer, in spite of the Cohenian terminology, is *sui juris*" (Natorp to Görland, 13 Jan. 1902; Holzhey, 1986, Br. 59, 2:271).

5.1. Cassirer on Leibniz and the infinitesimal calculus

That Cassirer was *'sui juris'* emerges from, among other things, the fact that he was careful not to rehash Cohen's view on Leibniz's philosophical justification of the infinitesimal calculus. Cassirer never mentions the notion of 'intensive magnitude' or the category of reality in the long chapter dedicated to the topic (Cassirer, 1902, chap. 4). This sort of language emerges only in the chapter on Leibniz's concept of 'force' (Cassirer, 1902, chap. 6). It is when the "fundamental idea of the infinitesimal" are applied to space, time and motion that it produces, "the content as intensive magnitude" (Cassirer, 1902, 303). Thus, Cassirer attempted to make sense of Cohen's 1883 book in a way which is not dissimilar to that of Lasswitz (see Cassirer, 1902, 336-337), insisting on the role of the 'infinitesimals' in transition from the phoronomic/geometrical to the dynamical concept of motion.

Motion, writes Cassirer, paraphrasing Leibniz,[9] "is a continuous arising and disappearing of determinations in succession"; therefore, it also lacks the consistency of identity that is demanded as a logical presupposition of reality (Cassirer, 1902, 291). The reality of motion that was lost when motion was only a geometrical change of position "is obtained again by fixing the overall process in the single temporal element [im einheitlichen Zeitmoment]" (Cassirer, 1902, 291), in the 'derivative force' as "the differential of motion" (Cassirer, 1902, 291). In turn, Leibniz considers "the emergence of a finite 'quantity of motion' from the elementary impulse" and "the developement of the element of velocity from continuous repetition of acceleration (as element of second orders)," as an "expression of integration as a continuous summation of infinitesimal moments" (Cassirer, 1902, 170).

These passages, so clearly 'Cohenian' in flavor, should not mislead. Cassirer explicitly emphasized that in this context, Leibniz's intuitive parlance must not be confused with a rigorous foundation of the infinitesimal calculus. Mathematically, Leibniz's strategy is precisely the opposite: not the 'production' of the finite magnitude as a summation of infinitesimal elements, but the 'vanishing' of the finite magnitudes while their ratio is preserved (Cassirer, 1902, 173). It is precisely "this second direction that prevails in considering the pure mathematical derivation of the differential [...] finds its final expression in Leibniz's mechanics" (Cassirer, 1902, 173). Thus, despite the undeniable Marburg rhetoric that permeates Cassirer's book, the difference from Cohen's position remains very significant.

In contrast to Cohen, Cassirer asserted that the foundation of the infinitesimal calculus resides precisely in the concept of the 'limit':

[9] GM, 6:235.

"the zero of the limit," he pointed out, "indeed has a positive meaning" (Cassirer, 1902, 174). In the 'passage to the limit' it is shown how "the magnitude must first disappear for the sensible apprehension, so that we can grasp its determination in its purity" (Cassirer, 1902, 174): The *dx* is zero in its extensive quantity, but in its conceptual, qualitative aspects it is fully determined through all the relationships that defined the *x* (Cassirer, 1902, 174). For this reason, however, according to Cassirer, the concept of the 'differential quotient' is the fundamental one, not the 'differential': "the differential quotient is the mathematical expression of the autonomy and the originality of the relation with respect to the element from which the relation is obtained" (Cassirer, 1902, 177).

Cassirer's reading of Leibniz's famous definition of continuity—*Datis ordinatis etiam quaesita sunt ordinata*[10] ('If the *data* are ordered, the *quaesita* must be ordered also')—is, in its unilaterality, revealing:

> While initially continuity meant for [Leibniz] the *origin* of a variable considered in methodological isolation, now he refers explicitly to the reciprocal dependence of variables. The concept of function is indeed everywhere already implicitly presupposed in the preceding remarks: the conceptual relations of *data* and *quaesita* are mathematically completely represented through the connection between the independent variables and the function. This is the meaning of the principle which states that, when the difference in the 'given' is diminished under an arbitrary value, it must be possible that the difference in the 'results' also becomes smaller than every quantity, however small: [Leibniz's] formula [...] [Datis ordinatis etiam quaesita sunt ordinata], as one can see, paraphrases the usual definition of the continuity of a function, according to which $|x' - x| < \varepsilon$ becomes $|f(x') - f(x)| < \delta$. If one considers this connection, one comes to the remarkable result that in the general definition of continuity the definition of function is already entailed (Cassirer, 1902, 239).

Thus Cassirer even credited Leibniz for having anticipated the *Epsilontik*! Cassirer's reading, from a historical point of view, is of course just as implausible as Cohen's, but for the opposite reason: Leibniz did not possess the concept of 'function' and reasoned in terms of variables ranging over a sequence of values (Bos, 1986).

The crucial point, however, lies elsewhere. Cassirer's treatment of the continuity problem in his Leibniz monograph dissents significantly from Cohen's position, and here it is not hard to glimpse the embryo of Cassirer's mature philosophy. At the beginning of the passage just quoted, the disagreement with Cohen seems to become nearly explicit. Even if there is no direct reference to Cohen, it is hard not to read the opposition between the '*Ursprung* of a variable in isolation' and the 'reciprocal relation of variables' as Cassirer's veiled attempt to distance himself from one of the major features of the philosophy of his *Doktorvater*.

In May 1902, the first volume of Cohen's 'system of philosophy', the *Logik der reinen Erkenntnis* (Cohen, 1902), appeared from the publisher Bruno Cassirer (Cassirer's cousin). As is well known, in the *Logik* the concept of *Ursprung* or 'origin' plays the central role. It would be impossible to spell out the details of this complex and obscure book here (for more on the topic I suggest the classical monographs of Holzhey, 1986 and Poma, 1997). However, it is clear that Cohen insisted on the precise line of thought that Cassirer criticized, modeling the concept of 'origin' on the relationship between an isolated *dx* and its integral *x* (Cohen, 1902, 124ff.), conceived again as the summation of infinitesimal elements. Cohen was careful to distinguish the historical 'discovery' from the mathematical 'justification' of the calculus (Cohen, 1902, 135), as Cassirer had also done. However, he also explicitly rejected the priority of the 'differential quotient' over the 'differential' (Cohen, 1902, 182-183). For Cohen, the notion of 'function' is derivative with respect to the productive force of the 'differential' considered in its isolation. The problem is in fact the origin of *x* itself and not its relationship to *y* (Cohen, 1902, 280ff.).

The dissatisfaction with Cohen's systematic work was far from isolated. In June 1902, Hans Vaihinger thanked Natorp for his decision to review Cohen's work in the *Kant-Studien*.[11] It was only a few months later, however, that Natorp wrote to Görland that he had renounced the project after realizing that Cohen felt he was being misunderstood (Natorp to Görland 21 Nov. 1902; Holzhey, 1986, Br. 72, 2:301-302). In the review, Natorp complained that one cannot regard the quality "as an autonomous, pre-mathematical knowledge" (Natorp, 1986a, 1516) independent from quantity. The concept of the infinitesimal—as Natorp wrote in another unpublished commentary on Cohen's logic—lies in the fact that in the vanishing of the quantity, the quality of the law is preserved; the 'origin' of the quantity has to be sought precisely in this qualitative unit. However, this can only be expressed in terms of a *functional relationship* between quantitative determinations: "To completely isolate the infinitesimal from the quantity and to establish it without regards to the latter, as a matter of fact, does not work" (Natorp, 1986b, 53).

5.2. Defending Cohen: the Cassirer–Nelson controversy

Thus, both Cassirer and Natorp seem to have fully acknowledged the necessity of accepting Cohen's presentation of the calculus with reservations. In an attempt to extrapolate the philosophical spirit of Cohen's 1883 book without endorsing its most controversial aspects, it is curious that they *de facto* embraced the very same elements of the critiques that Cohen's approach had received outside of Marburg. Bertrand Russell, in *The Principles of Mathematics* (Russell, 1903) remarked that aside from the "historical excellence" of Cohen's book, he was "led astray by very important mathematical errors." In particular, he failed to notice that "[t]he *dx* and *dy* of a differential are nothing in themselves, and *dy*/*dx* is not a fraction" (Russell, 1903, 325; but see Russell, 1900, 88 for a more benevolent reading). Even if Russell's remarks cast a negative light on Cohen's work, we have already seen similar remarks in the private correspondence and published writings of many in Cohen's own circle.

When, in 1904, the 23-year-old Göttinger philosopher Leonard Nelson wrote a long and devastating review of Cohen's *Logik der reinen Erkenntnis* (Nelson, 1905), the 'Marburgers' were probably not completely surprised. As Nelson pointed out with ruthless sarcasm, "with his views about differential, series and functions Cohen located himself completely outside of that domain of science that we today call mathematics" (Nelson, 1905, 621). Coming from Nelson, who was highly esteemed among Göttinger mathematicians like Felix Klein and David Hilbert, this review had serious consequences for the image of the Marburg school. Hilbert even took pains to borrow the book from Nelson: "it did not take him long," as Nelson wrote to Gerhard Hessenberg, "to declare the book a satirical piece [Bierzeitung]" (cit. in Peckhaus, 1990, 192).

"In the *Gött[ingische] Gel[ehrte] Anz[eigen],*" Natorp wrote to Görland on 25 September 1905, there is "a little 'double murder': my Platon a[nd] Cohen's Logic are killed one after the other" (Natorp to Görland, 25 Set. 1905; Holzhey, 1986, Br. 98; 2:352).

[10] GM, 6:29.

[11] Natorp Nachlass, Hs 831/1066, cit. in Ferrari, 1988, 107n. 130.

Natorp was not particularly upset by the review of his book (Goedeckemeyer, 1905); however, Nelson's review of the *Logik*, which immediately followed, clearly had a different effect: "Nelson put forward against Cohen mathematics and Kant. There, he must defend himself" (Natorp to Görland, 25 Sep. 1905; Holzhey, 1986, Br. 98; 2:352).

It was actually Cassirer who, in 1905, wrote an unusually harsh reply to Nelson (Cassirer, 1906b, and later to the Nelson's group, Cassirer, 1907b, 1909 vs. Grelling, 1907; Hessenberg, 1908; Meyerhof, 1907), which symbolically opened the series 'Philosophische Arbeiten', edited by Cohen and Natorp (cf. Holzhey, 1986, 1:384-385), as though he had to defend the honor of the Marburg school as a whole (Cassirer, 1906b, I-III). Cohen was grateful to Cassirer for having taken his side (Cohen to Cassirer, 22 Nov. 1905, ECN, 17, Doc. 19, 209ff.). However, Cassirer's defense of Cohen, when concerning the vexed question of Cohen's conception of the 'infinitesimal', was vague enough to conceal a much greater difference of perspective between him and his teacher. The "Nelson-Battle"—as Cohen ironically called it (Cohen to Cassirer, 2 June 1906, ECN, 17, Doc. 27, 229)[12]—once again revealed the delicate equilibrium between staying loyal to the school and avoiding some of Cohen's clearly unacceptable claims.

In 1906, in the first volume of the *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit* (Cassirer, 1906a), on the one hand, Cassirer seems to agree with Cohen that Galileo's concept of 'moment' was the first example of the infinitesimal (Cassirer, 1906b, 330). On the other hand, he draws a completely different conclusion. In Cassirer's this shows that "neither the differential of space, nor even that of time can show the way" (Cassirer, 1906b, 330). Instead he argued that, "historically and logically, the concept of the *differential quotient* was the point of departure" (Cassirer, 1906b, 330). Thus, it is not *dy* and *dx* taken alone, but rather the "functional equation" within which these assume their significance that "offers the most secure and 'substantial' base that scientific thought can give for the constitution of magnitude" (Cassirer, 1906b, 330-331).

In 1906, Cassirer registered the *Erkenntissproblem* as *Habilitationschrift* at the Friedrich-Wilhelms-Universität in Berlin.[13] Cassirer held the *Probevorlesung* on 26 July 1906. This time Dilthey did not want to enter the annals of history as the man who had rejected Cassirer, and thus Cassirer obtained the longed-for *venia legendi*—despite the opposition of Alois Riehl and Carl Stumpf (cf. Gawronsky, 1949, 12 and Cassirer Bondy, 1981, 101). The revealing title of Cassirer's lecture, "Substanzbegriff und Funktionsbegriff" (Cassirer, 1906c), suggests that his remarks about the infinitesimal calculus can be seen as just one example of his larger project, namely, describing the historical process of the prevailing concept of function over that of substance, as was systematized in his classical 1910 monograph (Cassirer, 1910).

6. The Marburg divide: Gawronsky, Cassirer and Natorp on the interpretation of the calculus

Cassirer probably did not consider his approach to be in opposition to Cohen's. A month before his 'Probevorlesung' on 28 June 1906, Cassirer wrote to Natorp that he could not completely understand the latter's disagreement with Cohen: "between both of you there is at least an agreement concerning the logical priority of quality over quantity. This is the key issue" (Cassirer to Natorp, 28 June 1906; Holzhey, 1986, Br. 100, 2:350). After all, Cohen does not want to claim anything more than this (Cassirer to Natorp, 28 June 1906; Holzhey, 1986, Br. 100, 2:351).

Cassirer's latter claim is of course quite doubtful. In particular, Natorp's concern was far from specious. After having read Cassirer's dispute with the neo-Friesians, Natorp confessed to Görland: "Cohen's treatment of the concept of the differential dismisses too quickly the fact that the *dx* is nothing in itself, it is completely dependent from the presence of a *differentiable* function, from the presence of a *limit value* (convergent series) etc." (Natorp to Görland 27 July 1907; Holzhey, 1986, Br. 108, 2:361). Although Natorp did not believe that Cohen's point of view depended on this, he had to admit that "some [of Cohen's] formulations are without doubt incautious" (Natorp to Görland 27 July 1907; Holzhey, 1986, Br. 108, 2:361).

The irenic picture Cassirer painted was indeed not the one perceived in Marburg. As the young Nicolai Hartmann—who had just finished his dissertation (N. Hartmann, 1908)—reveals in a letter to his friend Heinz Heimsoeth on 9 July 1908, the relationship between Cohen and Natorp was deteriorating. Natorp criticized Cohen in detail in public, for hours, even though according to Hartmann, many people were on Cohen's side: "The spokesman thereby is a certain Mr. Gawronsky, a really highly gifted man and a born mathematcian" (Hartmann to Heimseoth, 9 July 1908; F. Hartmann and R. Heimsoeth, 1978, 29). Hartmann was referring to the Russian émigré Dimitry Gawronsky, who was ten years younger than Cassirer and his close friend (Cassirer Bondy, 1981, 121 and 299-300; Sieg, 1994, 486). As Hartmann wrote to Heimsoeth on 5 October 1909, "Gawronsky will soon submit his dissertation" (Hartmann to Heimseoth, 9 July 1908; F. Hartmann and R. Heimsoeth, 1978, 29).

Gawronsky's dissertation, *Das Urteil der Realität und seine mathematischen Voraussetzungen*, was apparently poorly received by the Marburg mathematicians, and in particular by Kurt Hensel (Fraenkel, 1967,109). It was published a year later (Gawronsky, 1910) and it represents a significant example of a quite different attitude towards Cohen's controversial approach to the 'infinitesimal' within the Marburg school. Gawronsky attempted to provide a sort of 'update' of the *Logik der reinen Erkenntnis*, which, instead of 'urbanizing' Cohen's approach to the 'infinitesimals', tried to justify it in light of more recent, even if less mainstream, developments in mathematics.

Gawronsky considered Newton's presentation of the calculus as philosophically more radical than that of Leibniz. Newton had clearly separated the problem of the differential and of the differential quotient (Gawronsky, 1910, 83-85); by contrast Leibniz, as Cassirer has shown, relied entirely on the notion of function (Gawronsky, 1910, 35), so that the proper meaning of the concept of the differential does not become sufficiently apparent (Gawronsky, 1910, 91). For Gawronsky this latter conception is philosophically unsatisfying: "*dx* and *dy* are regarded as increments of magnitudes. The magnitude that is conceived as increasing must be presupposed" (Gawronsky, 1910, 82). A question therefore arises: "where do they come from, who made them? One cannot consider them as simply given" (Gawronsky, 1910, 82). For this reason, according to Gawronsky, "the concept of function is not sufficiently primordial," since "it presupposes magnitudes that are connected in a functional way, without being able to be justified [legitimieren] through the demonstration of their legitimate [rechtmäßigen] origin" (Gawronsky, 1910, 82).

To justify the use of 'differentials' separate from the differential quotient, Gawronsky relies on the fact that an algebraic theory of infinitesimals was actually present in contemporary mathematics, in the non-Archimedean characterizations of continuity that were suggested by working mathematicians, in particular by Giuseppe Veronese (Veronese, 1897, 1908). Gawronsky's approach was

[12] The pun refers of course to the Admiral Lord Nelson and the Battle of Trafalgar.

[13] Today's Humboldt-Universität zu Berlin.

indeed sophisticated. Via Veronese he introduced the Marburg community to a tradition going back to the work of Otto Stolz (1883, 1891) and Paul Du Bois-Reymond (1877), which Hans Hahn (1907) had recently shown was still fruitful (cf. Ehrlich, 2006 for more details). Unfortunately, Gawronsky's book is burdened by an apologetic agenda that probably rendered it unreadable outside Cohen's restricted group. At the same time, this is precisely why it constitutes an important document for understanding the moves of the orthodox 'Cohenian' front within the Marburg school.

In the introduction of his first big theoretical monograph, *Die logischen Grundlagen der exakten Wissenschaften* (Natorp, 1910) (the forward of which is dated March 1910), Natorp praised Gawronsky, "with his rich mathematical and physical knowledge" (Natorp, 1910, v), for having introduced the work of Veronese to the Marburg community. Natorp dedicated a detailed analysis of Veronese's work (see Mormann & Katz, 2013, sec. 4.5 for more details), insisting on a conception of the continuum as qualitative unity beyond infinite division into parts. However, he was far from using these results to 'save' Cohen, as Gawronsky had done.

Natorp of course defends Cohen against Russell's criticisms, emphasizing that they were based on "misunderstandings which are explainable through Cohen's difficult presentation" (Natorp, 1910, 222n.). However, this unavoidable defense does not conceal Natorp's uneasiness about Cohen's approach. Some of Cohen's "hyperbolic sounding" sentences could perhaps be understood and even, in some sense, endorsed, but could not be taken literally (Natorp, 1910, 218). In Natorp's view, "the main difficulty in Cohen's treatment [...] lies in his apparently gruff rejection of the method of limits as the foundation of the infinitesimal method" (Natorp, 1910, 220). For Natorp, the exact opposite is true: "the creative power of the infinitesimal procedure lies essentially in the passage to the limit" (Natorp, 1910, 220).

It is the same qualitative aspect of the 'law', which does not lose its significance in the transition to a quantitative zero, in the passage from the difference quotient to the differential quotient, and that it is recovered again in the reverse process of integration: "there is no other mystery here" (Natorp, 1910, 215). Cohen's notion of 'origin' finds its scientific counterpart in precisely the 'qualitative totality' that lies beyond the quantity. According to Natorp, "[a]fter all Cohen wanted to say this, even if his way of expressing it was not sufficiently immune from misunderstanding" (Natorp, 1910, 219).

Of course, Natorp must have been aware that this was *not* what Cohen wanted to say. As Cassirer pointed out in a long technical letter he sent to Natorp on 30 October 1909 (after having read the drafts of his book), Natorp "broadly speaking" deduces "the concept of continuity from the concept of law, from the persistence of the one and the same *functional law*" (Natorp to Cassirer, 30 Oct. 1909; Holzhey, 1986, Br. 120, 2:382). As Gawronsky's book shows, this was precisely the opposite of the orthodox Cohenian approach. It was clearly closer to Cassirer's point of view, who, however, had concerns about Natorp's "presentation of the infinitesimal procedure" (Natorp to Cassirer, 30 Oct. 1909; Holzhey, 1986, Br. 120, 2:382). The idea of the law's permanence, he pointed out, is necessary for a definition of continuity, but it is clearly not sufficient: "non-continuous functions and divergent series are also strictly lawful. Concerning these mathematical problems I still see some difficulties." (Natorp to Cassirer, 30 Oct. 1909; Holzhey, 1986, Br. 120, 2:382). Cassirer of course agreed with Natorp on the usual fundamental point, "the priority of 'quality' over 'quantity'" (Natorp to Cassirer, 30 Oct. 1909; Holzhey, 1986, Br. 120, 2:384). For Cassirer, however, the infinitesimal calculus was significant, yet still just an example of this more general tendency.

The long letter to Natorp shows the technical knowledge Cassirer acquired in preparing his *Substanzbegriff und Funktionsbegriff* (Cassirer, 1910), which was finished in July 1910. The publication of Cassirer's monograph was a momentous event for the Marburg school; the book rapidly became the most respected and widely read scientific work outside of Marburg. However, by then, Cassirer had gone further than Natorp by questioning the philosophica centrality of the 'infinitesimal calculus' which was so dear to Cohen. For the first time, Cassirer even seems to cautiously but openly disagree with Cohen's approach to the topic (Cassirer, 1910, 130-131). According to Gawronsky's later testimony, Cassirer's pages on the *Logik* were adjusted in the proofs because Cohen took umbrage at those remarks (Gawronsky, 1949, 21). Although this claim is unsupported by further evidence (cf. ECN, 17:271), the anecdote is revealing of how Cassirer's remarks might have been perceived within the Marburg community.

On 24 August 1910, Cohen wrote a letter to Cassirer that Gawronksy later made famous, having cited part of it in his biographical notes introducing the Schilpp-Volume that was dedicated to Cassirer (Gawronsky, 1949, 21). The full original German has recently been made available, and the letter is worth citing at length:

> I congratulate you and all members of our philosophical community on your new and great achievement. If I shall not be able to write the second part of my Logic, no harm will be done to our common cause, since my project is to a large degree fulfilled in your book. [...] But this is less interesting for you than my present view about the difference in the disposition from which you have started. Despite the fact that the concerns about agreement between us seem to me unfounded [...] Yet, I admittedly confess, that after my first reading of your book I still cannot discard as wrong what I told you in Marburg: you put the center of gravity upon the concept of relation and you believe that you have accomplished with the help of this concept the idealization of all materiality. The expression even escaped you that the concept of relation is a category; yet it is a category only insofar as it is function, and function unavoidably demands the infinitesimal element in which alone the root of the ideal reality can be found (Cohen to Cassirer, 10 Aug. 1910, ECN, Doc. 45, 269; part. trans. in Gawronsky, 1949, 21).

Cassirer's response has not been preserved. However, he clearly would have disagreed with Cohen's criticism, if only for the simple reason that the notion of function does not actually presuppose that of the infinitesimal or even continuity (see also Gawronsky, 1949, 21). Cohen clearly felt that it was Gawronsky, and not Cassirer (Fraenkel, 1967, 109), who had grasped and defended his core message. The leitmotif of Cassirer's book, the priority of the relation over the *relata*, completely obscured Cohen's insistence that the problem was the 'origin' of the *relata* themselves; in contrast, Gawronsky had grasped this perfectly well: "The more excited I am now about your judgment concerning Gawronsky," Cohen admitted to Cassirer in the same letter, "you will understand by my grateful bias towards him" (Cohen to Cassirer, 10 Aug. 1910, ECN, Doc. 45, 269).

7. *Finis Marburgi*: the decline of the 'Marburg school'

In 1912, at the age of seventy, Cohen retired from his chair at Marburg and moved to Berlin. On the occasion of Cohen's seventieth birthday, Natorp and Görland planned to edit a *Festschrift* in his honor. Cassirer also suggested dedicating a special issue of the *Kant-Studien* to him (cf. Natorp and Görland, 17 Feb. 1911; Holzhey, 1986, Br. 124, 395). The correspondence between Natorp and Görland is an important document for understanding the atmosphere of *finis Marburgi* in which both of the celebrative collections were prepared: "In the meantime the 'Marburg school' is in serious danger of being thrown awry" (Natorp wrote to Görland on 6 June 1912; Holzhey, 1986, Br. 131, 2:411). On 22 May 1912, Gawronsky was refused his habilitation (Sieg, 1994, 360f.); hanging over the celebration was the Damocles' sword that an empirical psychologist might be taking over Cohen's chair. Moreover, some of the most promising young members of the school, Nicolai Hartmann and Heinz Heimsoeth (the latter of which was working on his H. Heimsoeth, 1912–1914), were expressing dissatisfaction about the Marburg orthodoxy (F. Hartmann and R. Heimsoeth, 1978).

After the publication of the special issue of the *Kant-Studien*, Heimsoeth wrote to Hartmann: "But then, alas, we have the four papers by Natorp [1912], Görland [1912], Cassirer [1912] u. Kinkel [1912]. How sad are things in Marburg. Should they all really say the same things? More precisely: should they all repeat the same words?" (Heimsoeth to Hartmann, 10 July 1912; F. Hartmann and R. Heimsoeth, 1978, 118). The dissatisfaction over Cassirer's contribution on Cohen's interpretation of Kant is especially evident: "From Cassirer in particular I do not hope for anything more after this paper. Never ever. One should simply look at the style" (Heimsoeth to Hartmann, 10 July 1912; F. Hartmann and R. Heimsoeth, 1978, 119). Heimsoeth's dissatisfaction derived probably from the fact, that Cassirer had carefully avoided confronting Cohen's most controversial claims—e.g., the identification of the differential with the 'intensive reality'—and thus also seriously engaging their philosophical implications (see also Cassirer, 1918, 1920, 1926, 1943). It is not surprising that in the last 1914 edition of his "Kritischen Nachtrag" to Lange's history of materialism (Cohen, 1914), Cohen refers us to Gawronsky's book as a useful update of his views on the 'infinitesimal', yet he does not even mention Cassirer or Natorp's monographs (Cohen, 1914, 88).

Cohen died on 4 April 1918. Thirty years later, in a completely different environment, nearly nothing remained of Cohen's approach to the infinitesimal calculus. *Das Princip der Infinitesimal-Methode* (Cohen, 1883) was an unsuccessful book. There was no significant reception in the world outside of Marburg, where it never rose to the status of a reliable source for the history of the infinitesimal calculus. Within Marburg the book was an awkward presence: its conclusions had to be defended from hostility (Section 2, Section 4.2), yet they could not be fully embraced without qualifications. The central concept of the calculus, as many around Cohen must have pointed out to him even in private conversation, is not the 'differential', but the 'differential quotient' as calculus textbooks say. The latter concept, after all, offers the clearest example of relations without *relata* that critical idealism is searching for. However, as we have seen, Cohen's stubborn reluctance to follow an apparently sensible piece of advice, was motivated by the fact that, for better or worse, he was searching for something deeper, something that could account for the *relata* themselves. As a result, however, one cannot even credit *Das Princip der Infinitesimal-Methode* for having paved the way to Cassirer's 'structuralism' (Reck, 2003; Yap, 2014), undeniably the most successful product of Marburg Neo-Kantianism.

The impact of an unsuccessful book: this is what we have termed 'the puzzle of Cohen's *Infnitesimalmethode*' The puzzle can be solved insofar as one can separate the book's method of investigation from the results it achieved. If *Das Princip der Infinitesimal-Methode* failed to meet the standards of historical scholarship, more than any other book it embodied a certain style of doing philosophy, where the armchair speculation of post-Kantian idealism was replaced by a detailed confrontation with the historical developments of the sciences. It is this style that survived in the much more successful works of Natorp, Lasswitz and Cassirer, as well as in the work of several minor figures like De Portu, Görland, Buek, etc. (cf. Ferrari, 1996, 20ff.). Boris Pasternak—winner of the 1958 Nobel Prize in Literature, and one of the many Russian émigrés who studied in Marburg (Mallac, 1979)—put it beautifully in his brief 1931 autobiography: "the [Marburg] school did not speak of the stages in the development of the *Weltgeist*, but, say, of the postal correspondence of the Bernoulli family" (Pasternak, 1931, cited from Pasternak, 1949, 42).

Abbreviations

A Kant, I. (1781). *Critik der reinen Vernunft* (1st ed.). Riga: Hartknoch. Repr. in Ak., 4.

Ak Kant, I. (1900—). In Preussische Akademie der Wissenschaften, Berlin-Brandenburgische Akademie der Wissenschaften, and Akademie der Wissenschaften in Göttingen (Ed.), *Kant's gesammelte Schriften*. 29 Vols. Berlin: Reimer.

B Kant, I. (1787). *Critik der reinen Vernunft* (2nd ed.). Riga: Hartknoch. Repr. in Ak., 3.

CW Cohen, H. (1977—). In H. Holzhey (Ed.), *Werke*. 14 Vols.

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