Kant, Axiomatics, Logic and Geometry

Review by Irving H. Anellis of Immanuel Kant, *Logic,* translated, with an introduction, by Robert S. Hartman & Wolfgang Schwarz (New York, Dover, 1988; reprint of the New York & Indianapolis, Bobbs-Merrill, edition, 1974.)

This is the third English translation of *Immanuel Kants Logik: Ein Handbuch zu Vorlesungen,* first published in Königsberg in 1800 by Friedrich Nicolovius. The first complete English translation was the *Logic, from the German of Emmanuel Kant, M.A. To which is annexed a Sketch of his Life and Writings* made by John Richardson, and printed in London for W. Simpkin and R. Martin in 1819. The second (incomplete) English translation, which contained only the "Introduction" of the *Logik,* was made by Thomas Kingsmill Abbott, published as *Kant's Introduction to Logic, and his essay On the Mistaken Subtlety of the Four Figures (with a few notes by Coleridge)* in London by Longmans Green, & Co. in 1885, and reprinted in New York in 1963 by the Philosophical Library. The present translation includes Jäsche's "Preface", along with a one hundred page "Translators' Introduction" which discusses the role of logic within the Kantian philosophy, and a new "Preface" by Schwarz. (A consideration of the role of logic in Kantian philosophy is also to be found in [Collins 1976/77], which may perhaps also serve as an introduction to Kantian philosophy.)

In a detailed textual examination of Kant's *Logik* (1800), Terry Boswell [1988] has shown that the book is a compilation by Kant's student Gottlob Benjamin Jäsche, principally from Kant's lecture notes, of annotations to Kant's copy of the textbook *Auszug aus der Vernunftlehre* (Halle, J.J. Gebauer, 1752) of Georg Friedrich Meier (1718–1777) (which Kant used beginning with his first logic course, not merely since 1765, as stated by Jäsche), but also from Meier's *Vernunftlehre* (Halle bei Gebauer, 1752), of which the *Auszug* was an abridgement. Kant's notes included marginal notations as well as notes on interleaved sheets of paper. The

Logik itself consists largely of an "Introduction" designed to elucidate various concepts of Kant's philosophical system, especially his epistemology. The remainder of the book presents definitions of some of the concepts of syllogisitic logic from the perspective of the Kantian philosophy. According to the table compiled by [Fang 1986, p. 66], Kant (1724–1804) taught logic courses fifty-four times over a period of four decades, from 1755/56 to 1796 (although he did not receive the post of Professor of Logic and Metaphysics until 1770). This is confirmed by Boswell [1988, p. 195], who cites Emil Arndt's compilation of Kant's curriculum vitae. We may fairly assume, then, that the marginalia and inserted notes which Jäsche somehow managed to collate into a Kantian logic book is an accumulation over a long period of Kant's attempts to work through and assimilate Meier's textbook to his own thinking on logic and philosophy.

A number of philosophers, among them Hartman and Schwarz, have argued that Kant's *Logik* is particularly valuable as a tool for understanding certain technical points of Kantian philosophy. Collins [1976/77] has been so keen to convince that the Kantian *Logic* is such a tool that he has even gone so far as to provide an unwarranted "justification" for the unsupported claim by Hartman and Schwarz that Jäsche's edition is based on Kant's 1782 logic course; specifically, he has claimed ([Collins 1976/77], especially p. 443) that Kant requested that the book be based on the 1782 course so that "it would represent the mature approach made just a year after publication of the *Critique of Pure Reason* ... ." This justification and its attendant claim, however, are unsupported (as Boswell [1988, p. 203] also pointedly remarked) even by Jäsche himself. The evidence adduced by Boswell for the nature of Jäsche's edition clearly contests the assertion made by Hartman and Schwarz (p. iv of the Bobbs-Merrill edition, p. xvii of the Dover reprint), and repeated by [Collins 1976/77, p. 443], that the book is based principally on Kant's 1782 lectures on logic. In fact, both Jäsche and Boswell specifically attest to the nature of the writings that had gone into the *Logik* as having been accumulated over a long period.

Particularly interesting for the historian of logic are the few brief remarks which are to be found (at pp. 23-24) on the history of logic in the

"Introduction". "Present day logic," Kant said (p. 23), "developed out of Aristotle's *Analytic*." In the T.K. Abbott translation (p. 10), this reads: "Logic, as we have it, is derived from Aristotle's *Analytic*." He went on to declare (p. 23) that "Logic, by the way, has not gained much in *content* since Aristotle's times and indeed it cannot, due to its nature." In Abbott's translation, (p. 10), we read: "Since Aristotle's time Logic has not gained in extent, as indeed its nature forbids that it should." Thus, Kant's view is that logic has no history, although he admits that some textbook presentations are better than others – and (at Hartman & Schwarz, p. 24; Abbott, p. 11) he singles out Christian Wolff's "general logic" (it is unclear whether Kant was referring to Wolff's *Vernünftige Gedanken von Kräften des menschlichen Verstandes und ihrem richtigen Gebrauch in der Erkenntnis der Wahrheit*, Halle, 1712, 1754 or – most likely – to his *Philosophia Rationalis, sive Logica*, Frankfurt & Leipzig, 1728, 1740, or even to his two-volume *Elementa Matheseos Universae*, Halle, 1713-1715) as the best of the day. Other writers, such as Johann Heinrich Lambert (1728–1777) in his *Neues Organon* (i.e., his *Neues Organon oder Gedanken über die Erforschung und Bezeichung des Wahren und dessen Unterscheidung vom Irrthum und Schein*, 2 volumes, Leipzig, Johann Wendler, 1764) are said to engage in useless subtleties rather than add anything substantial to the subject; still other writers of the day are said to confound logic and metaphysics (Hartman & Schwarz, p. 24; Abbott, p. 11). Moreover, Aristotle's treatment of logic in the *Organon* is already essentially complete. "In present times there has been no famous logician, and we do not need any new inventions for logic, because it contains merely the form of thinking" (Hartman & Schwarz, p. 24); "In our own times there has been no famous logician, and indeed we do not require any new discoveries in Logic, since it contains merely the form of thought" (Abbott, p. 11). Leibniz is there dismissed as a "philosopher" who helped get the subject back "in vogue". We find this same view expressed in Kant's *Critique of Pure Reason* (B viii), where he wrote that "logic from the earliest times has followed this sure path...that since Aristotle it has not had to retrace a single step, unless we choose to consider as improvements the removal of some unnecessary subtleties or the clearer exposition of its doctrine... . It is remarkable also that to the present day it has not been able to advance a step and is thus to all appearances complete and perfect." One might, along with Charles Peirce [MS L237:97, May 1, 1901, *Criticism of Mrs. Ladd-Franklin's article: "Proposition"*], rightly regard this remark as an admission on Kant's part that his own pamphlet *Von der falschen Spitzfindigkeit der vier syllogistischen Figuren* (Königsberg, Johann Jacob Kanter, 1762) on the four syllogistic figures contained nothing not already to be found in Aristotle's logic.

Thus, for Kant, logic is merely a set of rules for manipulating thought, not a science, or even so much a subject for independent study, but a mental faculty (or *Rechnen*, perhaps an *ars combinatoria* in a Leibnizian sense) requiring practice for felicitous use. Moreover, Kant held, according to [Gensler 1985, p. 279], that all of the "principles" of logic are self-evident and require neither proofs nor derivations from other principles. If logic is a mental faculty and, as Gensler says, self-evident and foundational, then we can understand why Kant held that logic has had no history since Aristotle. However, according to Adamson [1911, p. 115], Kant's *Logik* makes an attempt to "deduce the forms and relations of thought from the mere notion of understanding;" but this attempt to "deduce the forms and relations of thought from the mere notion of understanding, even when coupled with the principles of of formal consistency and consequence," Adamson [1911, p. 115] tells us, was an unmitigated failure. Kant's sole contributions to logic consisted precisely in an attempt to remove one of the "unnecessary subtleties" and in introducing the concepts of formal consequence and of non-contradictoriness into logic as kinds of relations among elements of thought. Thus, in his *Von der falschen Spitzfindigkeit der vier syllogistischen Figuren*, Kant sought to reduce all syllogistic reasoning to the single principle "Nota notae est nota rei ipsius." But Kneale & Kneale [1962, p. 354] remind us that it is a mistake to attempt to find one formula for syllogisitic reasoning without taking into consideration Aristotle's procedure for reduction. Kant's one original attempt to contribute to logic, then, is a failure. Similarly, in his Harvard lecture on Kant of 1865, Charles Peirce [1865, pp. 252-253] has shown that Kant's classification of judgments is erroneous. Jean van Heijenoort [1957] went so far as to specifically number Kant among those who lacked *Spitzfindigkeit*, lacked subtlety in questions of logical form. Moreover, it was Lambert, in his *Neues Organon*, rather than Kant, who introduced consistency into logic as a basic element of the concept of logical truth and non-contradictoriness, as Peters [1961/62, pp. 52-53] reminds us. For Lambert, the hallmark of a "simple concept" is "thinkability" ("Gedankbarkeit"); and for Lambert, "possibility" and "thinkability" are logically synonymous. According to Lambert, "thinkability" is precisely what makes logical truth, i.e. to be logically true is to be "absolutely thinkable".

A significant portion of Peirce's 1865 Harvard lecture was devoted to a "consideration of certain logical distinctions between different judgments, which play an important part in the main body of the *Critic* " ([1865, p. 251]). Peirce, one of the best-versed historians of logic of his day (and an expert on medieval logic), noted (p. 251) that "none of these distinctions is original with Kant; he either copied them out of the logics of the day or revived them from older systems." He then stated that Kant

> ...holds that judgments may be distinguished in four different ways according to their quantity, their quality, their relation, and their modality.
>
> In quantity, they may be either universal,
> particular, or singular
> In quality, they may be either affirmative,
> negative, or infinite
> In relation, they may be either categorical,
> hypothetical, or disjunctive
> and In modality, they may be either assertoric,
> problematic, or apodictic.
>
> All four of these divisions are objected to by modern logicians.

Peirce went on to argue (p. 252-253) that "Kant's division of quality must certainly be given up, [because in] the first place there is no logical distinction between universal judgments such as *all men are mortal* and singular judgments such as *George Washington was a great man*" and then because Kant's distinction between infinite judgments, such as *homo est non* *quadrupes*, in which the negation is applied to the copula, and negative judgments, such as *homo non est quadrupes*, in which negation is applied to the predicate, is unjustified, since "such an infinite judgment has the sense of an affirmative and Kant is wrong in distinguishing them in logic."

Kant's introduction into logic of the concepts of formal consequence, as the relation between a conclusion and its premises, without regard to the truth or falsity of the premises, and of non-contradictoriness as a basic criterion, not for axiomatic systems, but among propositions or judgments, was certainly important, but, as Adamson [1911, p. 114] reminds us, was not original with Kant. In Kant's logic, Adamson [1911, p. 119] detected three main types of deduction: (1) deductions of the understanding, in which conclusions follow simply and directly from a change in the form of a given judgment, i.e. by immediate inference; (2) deductions of reason, in which the necessity of conclusions deduced is shown by reference to a general rule for which the conclusion is an example, i.e. by syllogistic reasoning, whether categorical, hypothetical, or disjunctive syllogisms; and (3) deductions of judgment, in which the conclusions are obtained by application of general rules based upon experience, i.e. by inductive and analogical reasoning. In short, Kant merely accepts and helps to reintroduce the Aristotelian logic as found in the *Organon*, ostensibly without the metaphysical baggage which his contemporaries had allowed to become a part of the subject. But even here, a problem is detected by Peirce with respect to the second type of deduction in Adamson's list. For in a fragmentary manuscript review of Abbott's translation of Kant's *Introduction to Logic* (MS 1368; Fall–Winter 1885; to appear as Item 40 in volume 5 of the Peirce Edition Project's *Writings of Charles S. Peirce: A Chronological Edition*), Peirce states that Kant "wholly fails to see that even the simplest syllogisitic conclusion can only be drawn by observing the relations of the terms in the premises and conclusion."

The logician and historian of logic Aleksandr Arkhipovich Ivin has written [1983, p. 34] that Kant was the first to use the term "formal logic", but that he introduced it primarily to distinguish it from new "transcendental logic." He also adds that Kant enlarged the domain of formal logic. Kant's formal logic, says Ivin, is concerned with elucidating the most basic and universal categories of thought, such as "quantity" and "quality", that operate in every act of comprehension. There are two problems here with Ivin's statements. The first is that Kant's distinction was between *general* ("allgemeine") logic – not *formal* logic – and transcendental logic (Hartman & Schwarz, p. 18; Abbott, p. 5), the former being concerned with all objects universally and every use of the understanding, the latter only with objects taken as objects of the understanding; the second is that this "enlargement" of the domain of formal – or general – logic by Kant is essentially an attempt to incorporate Aristotelian logic into a general theory of knowledge. Thus the expanded Kantian "general logic" is "formal" only insofar as it includes Arisitotelian logic and in contrast with the "informal" transcendental logic. Therefore Ivin's assertion that Kant added anything to logic is untrue if by his assertion he means that Kant added anything to logic as we understand it. And as we have already seen, Kant himself believed that there was nothing to add to Aristotle's logic in any case.

Ivin's conception of the relationship between formal and transcendental logic, however, is not totally unique. In "Transzendentale und mathematische Logik", Max Bense [1950] argued that, since Kant, the goal of transcendental logic has ben to ground the categories and forms of intuition, as well as the axioms and rules of formal logic. Bense treats transcendental logic one the hand as a variable-free logic which has the sole aim of expressing the processes of thought, on the other as the basis of all possible logical systems (i.e. "metalogic", in the sense of Vasil`ev), and on the third hand as a logic representing the "theoretical unity of consciousness." I would argue that only Bense's third sense of "transcendental" is *transcendental* in the strictly Kantian sense. Bense then identifies combinatorial logic, as developed by Schönfinkel, Curry, Church, Kleene, and Rosser, because it is variable-free, as a "formal representation" of transcendental logic. Those who, presumably like Ivin, understand a formal logical system in Bense's sense, as a formal representation of transcendental logic, might reasonably conclude that Kant did indeed, by distinguishing formal and transcendental logic and working out the relationship between them, extend formal logic in an important way. Ivin does not make this point quite as explicitly as Bense did.

Nevertheless, it is difficult for us to understand how Bense can justify the treatment of a formal system, variable-free or not, such as combinatory logic and the lambda calculus, as *transcendental*. It is even more difficult, from the standpoint of modern mathematical logic, to understand how being variable-free is sufficient to render a formal system a "formal representation" of transcendental logic, or more particularly how the condition of being variable-free or of being the basis of all possible logical systems is equivalent to representing the "theoretical unity of consciousness." As van Heijenoort [1957a] has said, combinatorial logic can indeed be understood to be transcendental in the sense of being variable-free and even in the sense of supplying a basis for all possible logics, but not in the sense of representing the theoretical unity of consciousness.

Kant's view of geometry is quite similar to his view of logic. In the *Critique*, he expressed the view that geometry, as found in Euclid, is also "complete and perfect". That being the case, we must reject the view, recently presented by John Watling [1990], that Kant recognized the possibility of non-Euclidean geometries. This claim runs counter to Kant's statement regarding the completeness, perfection, and ahistoricity of Euclidean geometry. It also seems to suppose that Kant knew about non-Euclidean geometries. This is an historically dubious assumption at best. Moreover, whether or not one assumes that Kant knew about non-Euclidean geometries, this claim is based on the patently erroneous assumption that, because Kant held the propositions of geometry to be *a priori synthetic* rather than analytic, he therefore concluded that alternative systems of geometry are possible. This view is rather typical of Frege, but not of Kant. Thus Dummett [1982, p. 245] reminds us that Frege accepted the Kantian view that the propositions of geometry are synthetic, but that for Frege, "what does show the synthetic nature of geometrical propositions is the logical consistency of a denial of the axioms of geometry," which leads to the conclusion that their syntheticity guarantees that the independence of the Euclidean axioms cannot be proven. But Kant's belief in the possibility of alternative geometries is ruled out by his belief in the completeness, perfection, and ahistoricity of Euclidean geometry. Let us examine Watling's claim more carefully.

In his review of Bertrand Russell's *Cambridge Essays, 1888-99*, Watling [1990] takes the editor to task for failing to include an editorial note declaring that it is untrue that Kant was unaware of the possibility of non-Euclidean geometries. This criticism is unfair to the editor.

Watling bases his complaint on the fact that Giovanni Girolamo Saccheri (1667–1733) was in correspondence with Kant and that Saccheri had published a non-Euclidean geometry (in 1733). In fact, Fang [1973, p. 199] casts doubt on the existence of any correspondence between Saccheri and Kant, and appears to doubt even that Kant knew of Saccheri. Moreover, Saccheri thought that he had vindicated Euclid – hence the title of his book, *Euclides ab omne naevo vindicatus* (Milan, 1733), i.e. "Euclid purged of every error". Saccheri set out to prove that Euclid's fifth postulate (the parallel postulate) follows from the first four Euclidean postulates. What may have led Gottfried Martin, especially in his [1953] book *Kant's Metaphysics and the Theory of Science*, on whom Watling relies, and hence Watling, to apparently suppose that Saccheri showed that non-Euclidean geometries are possible, was no doubt the manner in which Saccheri constructed his proof, together with the well-known fact that, from Euclid onward, attempts were undertaken to derive the parallel postulate from the first four postulates, since the converse of the parallel postulate was provable within Euclid's system. Saccheri's "proof" is a proof by contradiction, or *reduction ad absurdum*; that is, he assumed that the fifth postulate was false, and sought to derive a contradiction. As Coolidge [1963, p. 69] said of Saccheri, "this careful logician undertook to prove the correctness of Euclid's postulate by showing that when it is replaced by another, a contradiction is sure to arise." By showing that the first four Euclidean postulates together with the *negation* of the fifth postulate yields a contradiction, Saccheri would have proven that the first four postulates, together with the fifth postulate, is a valid system. This is precisely what Saccheri thought he did – *vindicated* Euclid by proving that the assumption of the negation of the fifth postulate together with the first four postulates yields a contradiction. In particular, he considered an isosceles birectangle *ABDC*, a quadrilateral in which $AC = BD$ and angles *A* and *B* are right angles. Using only the first four postulates and the first twenty-eight theorems of Euclid which may be derived from them without the aid of the fifth postulate, Saccheri easily showed that angles *C* and *D* are equal to each other. Using an assumption invalid under the obtuse angle hypothesis (that straight lines are infinitely long), but without the aid of the fifth postulate, Saccheri was able to eliminate the possibility that angles *C* and *D* are obtuse. But he was unable to eliminate the possibility that they might be acute. To do so would have in fact required the fifth postulate. The best that Saccheri could do was employ some questionable characterizations of infinity to derive an unconvincing and irrelevant contradiction to eliminate the possibility that the angles are acute. Eves [1981, p. 69] has gone so far as to express his opinion that Saccheri himself was not fully convinced by his argument. (Dou [1970] gives a detailed analysis of Saccheri's arguments and discusses Saccheri's possible influence on the subsequent development of non-Euclidean geometry.)

Most of those of Saccheri's and Kant's contemporaries working on the parallel problem, like Saccheri himself, in fact really thought that they had shown that the fifth postulate does follow from the remainder of Euclid's axioms, and all of them were attempting, like Saccheri, to find such a proof. This is evidenced by the dissertation of A.G. Kästner's student Georg Simon Klügel (1739–1812), *Conatuum praecipurorum theoriam parallelarum* (1763), which collected all of the available attempts to prove the parallel postulate – twenty-eight in all, and showed that each of these proofs were inadequate.*

Today, we know of course that Saccheri's attempt actually failed to do what it was meant to do – that what it actually did was prove that non-Euclidean geometries (in which the negation of the fifth postulate, together with the first four of Euclid's postulates) are possible after all. More concisely, Saccheri "proved", so he thought, the dependence, by "conse-

* Russell's [1897, p. 7] assertion that Adrien Marie Legendre (1752–1833) was the first to refuse to accept the parallel postulate without a proof, and the first to attempt to deduce it from the others, is clearly false. Legendre spent some twenty years working on the problem of parallels and his results are scattered through the several editions (from 1794 to 1823) of his *Éléments de Géométrie*. These results were brought together in Legendre's *Refléxions sur différentes manières de démontrer la théorie des parallèles ou le théorème sur la somme des trois angles du triangle*, Mémoires de l'Académie royale des Sciences de l'Institute de France XII (1833), to which Russell referred.

quentia mirabilis," of the parallel postulate, though of course we know now that he proved its independence. Only in 1868, in *Saggio di interpretazione della geometria non-euclidea*, was Eugenio Beltrami able to use Saccheri's proof of the independence of the parallel postulate to develop the concept of *relative consistency proof* for non-Euclidean geometry, showing that non-Euclidean geometries are inconsistent only if Euclidean geometry is inconsistent, or conversely, that if Euclidean geometry is consistent, then so are non-Euclidean geometries. But to impute to Saccheri himself – and hence to Kant – the view that Saccheri proved that non-Euclidean geometries are possible, is anachronistic, and thus a misunderstanding of the history of geometry. Under these circumstances, it is difficult to believe that Russell could have concluded that Kant could have believed in the possibility of non-Euclidean geometries.

Kant came as close to the subject of non-Euclidean geometries as it would be possible for anyone of his day to come through a reading of the work of the mathematician Abraham Gotthelf Kästner (1719-1800), including in particular, for example, his *Anfangsgrüde der Arithmetik, Geometrie, ebenen und sphärischen Trigonometrie und Perspectiv* (Göttingen, 1792) Kästner, Gauss' professor at Göttingen University was one of a number of mathematicians in the late eighteenth-century attempting to prove Euclid's fifth postulate from the first four. Kästner was disappointed that his attempts failed, and in the 1790s wrote a series of papers (one of which, the one with which we are here concerned, was entitled *Was heißt im Euclids Geometrie möglich?*, Philosophische Magazin **2**, Stück 4 (1790), 391-402) on the nature of the possible in Euclid's geometry. Kästner argued that "possible" for Euclidean geometry means that the system is consistent; a proposition is "possible" if it can be obtained within the system of Euclidean geometry without leading to any contradictions (see [Lachterman 1989, p. 53]). Bonola [1955, p. 65], relying upon evidence presented in [Engel and Stäckel 1895, pp. 139-142], went a step further and concluded that Kästner accepted and supported the necessity of accepting the parallel postulate. Kästner went on to assert with Leibniz that mathematics consists solely of analytical truths. In reply to Kästner, Kant wrote (and published under the name of his student Johann Schulze, i.e.

Schultz (1739–1805)) that mathematical propositions are synthetic, and that this is why Kästner was unable to prove the parallel postulate. But Fang [1973, p. 206] takes advantage of this fact to declare that "Kästner was openly sceptic [sic] about the possibility to prove Euclid's parallel axiom." But this recognition of failure is hardly the same as asserting the possibility of non-Eucldean geometry. It is more likely that Kästner's failure to either prove the parallel postulate or to develop a non-Euclidean geometry grew out of his weakness as a mathematician; for, as Carl Friedrich Gauss, the young contemporary of Kant, had commented, Kästner was known as "the leading mathematician among poets and the leading poet among mathematicians," for example because he had summarily dismissed Gauss' construction of a regular 17-gon and the solution to the seventeenth-degree algebraic equation which Gauss had obtained for carrying out the construction with the comment that it is impossible to solve such an equation (see [Dunnington 1955, p. 24] and [Kline 1972, p. 754]). After noting that even before Gauss had arrived in Göttingen in 1795, and that the beginnings of non-Euclidean geometry were being laid, Fang 1970, p. 74] makes it clear not only that "Kästner was openly sceptic [sic] about the possibility to prove Euclid's parallel axiom," but adds that "under his influence, so was his pupil, Georg Simon Klügel [and so] again, was K.F. Seyffer, who was Extraordinarius in astronomy at Göttingen, 1789-1804, and had a deep interest in the foundation of geometry." So it would appear that Kästner's disbelief was not only strong, but influential.

Watling's argument that Kant did not deny the possibility of non-Euclidean geometries, like the corresponding arguments presented by Martin and Fang, is based precisely upon an assumption that the syntheticity of geometric propositions permits the possibility of alternative "constructions". If one is careful, one may speak, as did Fang [1967, pp. 12-14], of the "paradoxes of space" in Kant; but this relates to a treatment of an infinitesimal approach to calculus within the context of an atomistic philosophy. Moreover, mathematicians such as Gauss specifically rejected the distinction between "analytic" and "synthetic" propositions (see, for example, [Mansion 1909]). Thus, Kant is among the philosophers singled out for ridicule by Gauss ([vol. IV, p. 337]), in a famous letter of November 1, 1844 to the astronomer Heinrich Christian Schumacher, writing that "you see the same sort of [mathematical incompetence] in the contemporary philosophers... . Don't they make your hair stand on end with their definitions? ...Even with Kant himself it is often not much better; in my opinion his distinction between analytic and synthetic propositions is one of those things that either run out in a triviality or are false." Fang [1986, p. 119] attributes this negative appraisal of Kant to the acceptance of non-Euclidean geometry; but he does not make it clear whether it arises from Gauss also rejects Kant's assertion that our geometrical knowledge is entirely *a priori* (see, e.g., [Dou, 1970, pp. 403-404]). Gauss' view that Kant accepted (or rejected) the view that there is a non-Euclidean geometry, or from Gauss' acceptance of non-Euclidean geometry. The ambiguity is erased by reading [Fang 1967, p. 103], where we find, with reference to Gauss' letter to Schumacher, that "Kant has been a favorite whipping boy to almost everyone..., even from the day he was still alive, and especially since Gauss. The main reason for this fashionable enterprise was that Kant was so incompetent (!) as to anticipate neither non-Euclidean geometries (vs. his friend Lambert) nor symbolic logic (vs. Leibniz)." But Fang does suggest, here and elsewhere (e.g. in [1973, p. 207]) that Kant's philosophy paved the way for the abilities of Gauss and his Göttingen colleagues to accept the possibility of non-Euclidean geometry. But as for Kant himself, Fang [1973, p. 199] even casts doubt on any interest at all on Kant's part in the problem of parallels, and even goes so far as to say ([Fang 1986, p. 79]) that, even despite his correspondence with Lambert, "Kant knew absolutely nothing" about the work leading to the development of non-Euclidean geometries. In any case, the evidence presented by [Wolters 1980] appears to suggest that Lambert, at least, conceived the idea of studying the axioms of Euclid from Wolff, or at least while learning scholastic method and mathematics from Wolff's treatment of Euclid.

The situation is not as clear-cut or simple as Watling's remarks suggest. Besides Saccheri, it is also necessary to take account of a number of other mathematicians who were Kant's contemporaries, including Lambert, who contributed (in 1766) towards the future development of non-Euclidean geometry, and necessary also to determine Kant's knowledge and understanding of this work, and in particular of the work of his younger contemporary Gauss, who developed the first full-fledged and explicit non-Euclidean geometry, but did not publish it in his lifetime, and ask whether Kant could have known of this work, and if so, what he thought of it. Indeed, Fang himself ([1986, p. 79]) had gone so far as to admit (as we have already noted) that, despite his correspondence with Lambert, Kant was ignorant of any work in non-Euclidean geometry. [Mansion 1909] in fact went so far as to argue that "in the *Critique of Pure Reason* and elsewhere Kant has shown that he only had a very poor grasp of the elements of mathematics; he did not understand anything about what was going on in the contemporary research into the first principles of geometry." Martin [1985, p. xxiii] discovered that Kant's library contained a copy of Johann Schultz's philosophical book *Entdeckte Theorie der Parllelen nebst einer Untersuchung über den Ursprung ihrer bisherigen Schwierigkeit* (1784), a book which examined the theory of parallels from the perspective of Kant's early (1770) views of space and merely claimed that Kästner failed to prove the parallel postulate because he rejected the Kantian philosophical standpoint. But this certainly does not of itself contradict Mansion's claim, since possession does not by itself guarantee that Kant ever read, let alone understood, the book. (As it happens, Schultz's own attempt to prove the parallel postulate was badly bungled – compare it, e.g., with the "proof" in volume 2 of the *Development nouveau de la partie élémentaire des mathématique* (Geneva, 1778; vol. 2 reissued as *Éléments de géométrie* (Paris, 1812) of Jean Louis Bertrand (1731–1812)). In fact, Fang [1973, p. 207] baldly declares that "Kant was certainly unaware of any importance of the problem" of parallels on which Lambert (and Schultz) worked. Kitcher [1975, p. 29] goes even further, arguing that, for Kant, "Euclidean geometry is true in virtue of the fact that space is Euclidean," and goes on to claim that Kant not only failed to consider the possibility of some non-Euclidean geometry, but that Kant's view of the nature of the relationship between propositions and the world would collapse if he were to consider alternative geometries. Similarly, Kline [1972, p. 862] agrees that, for Kant, "certain principles about space are prior to experience; these principles and their logical consequences, which Kant called a prior synthetic truths, are those of Euclidean geometry. ...On the grounds just described Kant affirmed...that the physical world must be Euclidean," and [Kline 1972, p. 1032] that throughout the eighteenth century the "dominant view, expressed by Kant, was that the properties of physical space were Euclidean," and that no alternative was seriously considered – or at least vetted in public. Elsewhere, Kline [1982, pp. 76-77] unequivocally stresses that assertion that Kant held that "the mind necessarily organizes spatial sensations in accordance with the laws of Euclidean geometry," i.e. that Kant accepted no other alternative to Euclidean geometry. These interpretations are supported by evidence provided by Gauss himself which is cited by De Long [1970, pp. 46-47] and by Dou [1970, p. 403]. On the basis of this evidence from Gauss, De Long [1970, pp. 38, 46] concludes that Gauss refrained from publishing his early work on non-Euclidean geometry precisely because of concern that his work would be ridiculed by Kant and the Kantian philosophers. Dou [1970, p. 403] quotes directly from Gauss' 1829 letter to his colleague the mathematician Friedrich Wilhelm Bessel (1784-1846), and cites several other letters of these years, in which Gauss explicitly states that he will not in his lifetime publish his work developing non-Euclidean geometry specifically "because I fear the outcry of the boeotians," that is, the Kantians.

Martin [1972] also referred to an unidentified passage in Kant's obscure *Inaugural-Dissertation*, his *De Mundi Sensibilis atque Intelligibilis Forma et Principis Dissertaio* (Königsberg, 1770), concerning a "transcendental" or relativistic view of space (opposed to the Newtonian absolutist view of space), which he used in his own even more obscure doctoral thesis ([Martin 1939]) to argue that "under the Kantian presuppositions it is not only possible but necessary to assume the existence of non-Euclidean geometries. There can be no doubt of the correctness of this view... ." In reply, Fang ([1973, p. 207]; [1966]) more cautiously asserted that Kant may have "unwittingly" set the philosophical stage in his *Inaugural-Dissertation*, with its subjectivist view of space, so that it would be possible eventually for "some philosophically-minded mathematicians to toy with the bold alternative of constructing a non-Euclidean geometry instead of going on trying to prove the parallel axiom or to repudiate it...."

But he warns ([Fang 1973, p. 207]), contra Martin and Watling, that Kant "was never a mathematician enough to conceive and develop a non-Euclidean geometry, and never even dreamed of the possibility" that his *Inaugural-Dissertation* theory of space would eventually lead to attempts to erect non-Euclidean geometries. Indeed, when Fang and Takayama [1975, p. 273] declare that in his *Inaugural-Dissertation* Kant was "intellectually daring enough to suggest a geometry other than the Euclidean – 'die höchste Geometrie'," they make it quite clear that they mean only that throughout the eighteenth century, there were a growing number of works devoted to the problem of the parallel postulate, and that after 1770, the philosophical atmosphere had shifted to the point where mathematicians no longer felt a need to prove Euclid's fifth postulate, but felt sufficiently free to experiment with new geometries. Even so, [Fang and Takayama 1975, pp. 280-281] caution that Kant's direct influence on these mathematicians may have been minimal, or even that it was simply coincidental that non-Euclidean geometries began to flourish after the appearance of the *Inaugural-Dissertation*, to the extent that the intellectual tenor of the time was such that subjective views of space and non-Euclidean geometries might arise almost simultaneously, and more or less in tandem. In fact, Reid [1986, pp. 16-17] argues that Kant's view of the apriority of mathematics, and in particular of "the fundamental concepts of logic, arithmetic and geometry – among these the axioms of Euclid," cannot be supported by the possibility of non-Euclidean geometries (or, we might add, of non-Aristotelian logics), meaning that "the discovery of *non*-euclidean geometry in the first part of the nineteenth century had cast very serious doubt on this contention of Kant's, for it had shown that even with one of Euclid's axioms negated, it is still possible to derive a geometry as consistent as euclidean geometry. It thus became clear that the knowledge contained in Euclid's axioms was *a posteriori* – from experience – not *a priori* ." De Long [1970, pp. 40-41] adds that "it is easy to see how [the view that geometrical propositions are apriori synthetic] might hinder the development of a geometry different from Euclid's. For if such a view were influential it would be unlikely that anyone would look for a non-Euclidean geometry," and he goes on to explain that

> If someone did, it would take intellectual daring to assert the existence of a new geometry (remember that Euclidean geometry was thought necessarily true of the world), and finally it might take courage to publicly proclaim its existence. One can easily exaggerate the importance of Kantian philosophy in hindering the discovery of non-Euclidean geometry. The ancient authority of Euclidean geometry, as well as its seemingly unimpeachable presentation, were surely strong factors. Nevertheless Kant's influence was not insignificant in this regard.

If this is true, it is difficult to conclude that Kant's influence could have led, however unwittingly, to the discovery of non-Euclidean geometry, and even less to Kant's own discovery of non-Euclidean geometry.

Finally, Martin ([1955], cited by [Fang 1973, p. 199]) points to Kant as a precursor, if not the founder, of non-Euclidean geometry by citing Kant's first publication, in which Kant points out an error in Leibniz's *Theodicy* of the proof that space is three-dimensional, and suggests that "the true geometry" must admit the possibility of any number of dimensions. But Fang [1973, p. 199] adds the correction that Kant had said "higher (i.e. higher-dimensional) geometry", rather than "true geometry," but goes on to suggest that Kant did not therefore believe that geometries of more than three dimensions are actually possible. Additionally, Rosenfeld [1988,p. 179] cites exactly the same passage cited by Martin, and like Martin concludes that Kant recognized the possibility of multi-dimensional space. Furthermore, Rosenfeld [1988, pp. 179, 187] concludes on the basis of this passage and various remarks in Kant's *Critique* that Kant not only actually left open the possibility that one could generalize from three-dimensional to multidimensional space, but that the passage cited really suggests one such possible generalization. The passage in question says:

> It is likely that three-dimensionality is the result of the fact that in the world around us substances interact so that the forces involved are inversely proportional to thesquares of distances...another law would imply a space

> form with other properties and dimensions. A science of all such space forms would undoubtedly be the most sublime geometry (*höchste Geometrie*) which finite reason could pursue. ...If the existence of space forms with other dimensions is possible, then it is very likely that God had realized them somewhere.

But if we examine the passage carefully, as quoted by Rosenfeld, and in the light of what Kant said about the apriority of geometrical propositions and the necessary relationship between Euclidean geometry and the physical world, we see that Kant is actually *denying* the possibility of multidimensional space. (Note, for example, the hypothetical mode of Kant's expressions.)

Under the impact of all of the circumstances which we have considered, it becomes evident that it is Watling's view that needs to be challenged, and that his criticism of the editor of [Russell 1983] should be taken at least *cum grano salis*, if not rejected outright.

Kant's views on logic and geometry are similar precisely because they are so closely related. Both rest upon a dogmatic belief that these subjects are ahistorical and written "in stone" by Aristotle and Euclid respectively. This is not altogether surprising, since Aristotle probably intended his *Prior Analytics* to set forth the form of geometric reasoning which Euclid intended in his *Elements* to follow.† Moreover, Kant's views of logic and geometry (and arithmetic) are built upon the artificial classification of propositions as analytic or synthetic. This classification, as we saw, was rejected by Gauss long before it was rejected by Quine [1963], whose argument was founded in part upon the possibility of building logics on various modifications of the law of excluded middle (suggesting the connection, made explicit by Vasil`ev, between Lobachevskii's non-

† This view of Aristotle's intention and of the relationship between Aristotelian syllogistics and the Euclidean axiomatic method was challenged in the 1970s, for example by Mueller [1974], Gómez-Lobo [1977], and Smith [1977-78]. It appears to have been unanimously accepted, however, by eighteenth-century logicians, who regarded Euclid's *Elements*, especially the first book, as the epitome of logical reasoning and who, following Wolff, sought to apply the Euclidean method to every field of study. This would help account for the urgency with which eighteenth-century mathematicians sought to prove the parallel postulate.

Euclidean geometry and Vasil`ev's own non-Aristotelian logic, for which Lobachevskii's geometry served as an analogy; see, e.g., [Bazhanov 1988, especially pp. 101-109] and [Duffy 1990, pp. 74, 76]).

A more crucial connection between Kant's views on logic and geometry is related to his professed contributions to axiomatics. Martin [1985, pp. 5-6] made Kant the father of modern axiomatics, "the first to recognize the axiomatic character of mathematics", on the basis of Kant's famous argument that arithmetic propositions such as 7 + 5 = 12 are *a priori* synthetic, on the basis of the purely technical mathematical work of some of Kant's mathematical followers, and especially on Kant's insistence that a Euclidean, axiomatic – as opposed to deductive – approach to mathematics is necessary; but this is clarified to mean that, for Kant, geometric and arithmetic proofs depend upon axioms, but are constructive in nature, meaning that theorems cannot be deduced from principles by pure logic.

Martin's [1985, p. 49] claim that Kant "discovered" the axioms of arithmetic is based upon the axiomatic system presented by Johann Schultz's *Anfangsgründe der reinen Mathesis* (Königsberg, Nicolovius, 1790) and *Prüfung der Kantischen Critik der reinen Vernunft* (Frankfurt & Leipzig/Königsberg, Nicolovius, 1789/1791-92). Schultz was Kant's mathematical former pupil and friend, who attributes the axioms of arithmetic to Kant although they appear to have originally come from Leibniz through Wolff's *Elementa matheseos universae* (1713-1715), and of similar works of several other of Kant's former students. (In fact, the axioms, as presented in Schultz's *Anfangsgründe der reinen Mathesis*, are quite muddled; Schultz confused, for example, commutation and association with composition, "deduced" distributivity and associativity from the laws of multiplication, and failed to prove the law of multiplicative associativity.) Against his own assertions, Martin [1985, p. 26] cites a passage from Kant's *Inaugural–Dissertation* in which it is clearly stated that "pure mathematics...is the organon of all intuitive...knowledge" – which suggests that Kant could have been a precursor of Brouwer, but hardly a precursor of Hilbert.

In the manuscript of the *Prüfung*, Schultz argued that the propositions of arithmetic are analytic; but following a discussion of the manuscript with Kant in 1788, he changed his mind, and the published version of the *Prüfung* asserts that arithmetical propositions are also synthetic, because they rest upon the "axiomatic nature" of arithmetic (see [Martin 1985, pp. 48-49]). From this, Martin suggests that Schultz learned the axioms from Kant. Did Kant merely pass on the axioms, which he himself learned elsewhere, or did he "discover" them himself, with Schultz in the course of their discussion? Schultz makes it clear in the *Prüfung* (Part I, p. 217; quoted by [Martin 1985, pp. 49-50]) that Kant convinced him of the syntheticity of the propositions; but he does not claim that Kant discovered or taught him the axioms. Again, against his own assertions, Martin [1985, p. 49] cites Kant's own words, from the letter of 25 November 1788 to Schultz which initiated the discussion of the *Prüfung*, according to which "to be sure, arithmetic does not have axioms... . But it does have postulates." Whatever axiomatic conception Kant may have held regarding arithmetic, there is no evidence to suggest that Kant "discovered" either the axioms themselves or the axiomatic nature of arithmetic. If Peirce [1865, p. 251] was indeed correct in his assertion that "none of these [logical] distinctions [between types of judgments] is original with Kant; he either copied them out of the logics of the day or revived them from older systems," it must be all the more true with respect to the unoriginality of Kant's axiomatic presentation of arithmetic. But having long taught mathematics courses (sixteen times altogether, in the course of the eight years from 1755/56 to 1763, by Fang's [1985, p. 66] tabulation), he could not have remained ignorant of the axiomatic presentations used by the authors of the textbooks from which he lectured. Against Martin, we are inclined to accept Fang's [1973, p. 210] conclusion that Kant could not possibly have developed his solution to the problem of the possibility that $7 + 5 = 12$ in late nineteenth- and early twentieth-century terms, along the lines of

$$7 + 5 = 7+(4 + 1) = 7 + (1 + 4) = (7 + 1) + 4 = 8 + 4 = \ldots$$

and thereby treating the proposition as analytic rather than synthetic. This treatment, which is Martin's rather than Kant's, would, from the Kantian perspective, just as much endanger Kant's view that the propositions of mathematics are a priori synthetic as the acceptance of the possibility of non-Euclidean geometries would, as Reid saw, have endangered that Kantian classification of mathematical propositions. Watling's [1990] criticisms of the editor of [Russell 1983] for failing to include an editorial note declaring that it is untrue that Kant was unaware of the possibility of non-Euclidean geometries is unfair to the editor also with respect to specific arguments made by Russell. In *An essay on the foundations of geometry* [1897, pp. 54–63], Russell argued that the development of "metageometry", i.e. the axiomatic foundations of (Euclidean and non-Euclidean) geometries, has shown that Kant's argument for the apodeicitcity of Euclidean geometry breaks down. But Russell did not accept either the position that non-Euclidean geometries are necessary (in any Kantian sense). Instead, Russell [1897, p. 6] concluded, *contrary* to Watling's (and Martin's, *et aliia*) interpretation of Kant's position, that only those axioms which are common to both Euclidean and non-Euclidean geometries are apriori, whereas the axioms specific to Euclidean geometry are "wholly empirical," as are those axioms specific to the various non-Euclidean geometries. This view was reenforced by Russell's [1898] reply to Louis Couturat's [1898] review of the *Essay*, in which Russell first accepts Couturat's assertion that Russell's argument in the *Essay* for the empirical character of Euclidean geometry is weak but then defends the empiricality of Euclidean geometry with new arguments. Russell [1902, p. 673] made the point much more clearly in his *Encyclopedia Britannica* article on non-euclidean geometry that, although Kant's view that geometry is synthetic permits the possibility that there might be non-euclidean geometries, "Kant maintained [that there] is *à priori* ground for excluding all or some of the non-Euclidean spaces."

In conclusion, not only must we look askance at Watling's view that Kant was familiar with and accepted non-Euclidean geometries, but we must reject, along with Fang, the more general conclusion – in Fang's term, "myth" – that Kant was a great mathematician. Likewise, the evidence which we have examined of Kant's writings in logic lead to the conclusion that Ernst Cassirer's classic [1907/8] appraisal of Kant as a great mathematician, and even as the precursor of modern mathematical logic, with Kant's contributions set on a par with the work of Russell and Couturat in foundations of mathematics, must be finally and unequivocally rejected. But it is doubtful in any case whether anyone today would even remotely consider Kant to have been a great logician – or a logician at all. If there are such people, they are certain to be disabused of that judgment by a reading of Kant's *Logic*. Refocussing our attention on Kant's *Logik* and considering it in connection with the broader issue of axiomatics, we are inclined to agree with Fang [1986, p. 117] when he wrote in opposition to Martin's view, that

> Kant did teach the course on 'Logic' fifty-four times and several versions of this lecture have been edited and published, but none of these shows a scintilla of evidence for Kant to be a David Hilbert manqué. This approach is contumelious to both, crassly ignoring either the epochal and eclectic way of Kant's epistemology (in philosophy) or the pathfinding mode of Hilbert's unique foundational methodology (for geometry in particular and, later, for mathematics in general).

Similarly, Fang [1970, p. 84] accounts for Hilbert's quotation from Kant at the start of the *Grundlagen der Geometrie* by the simple remark that "Hilbert merely liked to quote Kant, the most famous and greatest of all Königsbergers – that was all." This remark by Fang is quoted approvingly by [Peckhaus 1990, pp. 14-15], who, however, seeks to show that the Kantian epistemology provided Hilbert at most with an "orientation" around which the philosophical framework of axiomatics is set.

The most important thing to be learned from a consideration of Kant's *Logik* concerns Kant's role in the history of logic, and in particular what attitude was taken towards logic at the end of the eighteenth century. Did Kant's view typify the attitudes of logicians towards their subject during this historical period. Or was Kant's view atypical? Perhaps this is where Saccheri can help us. Saccheri's popular textbook *Logica Demonstrativa* (1697; 1701; 1735) had the intentions of applying the strict standards of geometrical proofs to logic and of reducing the number of "first principles" to a minimum. His logic is the syllogistic of Aristotle, and his approach is deductive, following the methodology of Euclid's *Elements*. It is in this work as well, significantly, that Saccheri reintroduced the concept of proof by contradiction which he said he had found in his reading of Euclid and which he used in his *Euclides ab omne naevo vindicatus*. In this sense, Kant's treatment of logic as Aristotelian syllogistic and his constructive approach to deduction is clearly in line with Saccheri's approach. Kant's logic textbook, Meier's *Auszug aus der Vernunftlehre*, falls roughly into this same category, although it takes its start from Leibniz, or more carefully, to Wolff's bawdlerization of Leibniz, doing its best, in the words of Styazhkin [1969, p. 100], "to bury any idea of making logic mathematical." It was from such the "scanty and confused logical compendia," in particular Meier's *Vernunftlehre* (1752), Styazhkin [1969, p. 100] continued, "that Kant was later spoon-fed formal logic." That being the case, we may fairly conclude that Kant's views of logic were quite in tune with the typical attitudes of most of his colleagues. However, within fewer than fifty years of the appearance of the *Logik*, Kantian philosophers with interests in logic, such as Johann Heinrich Loewe at the Salzburg Lyceum, realized that Kant's views on the history of logic were badly outdated. Thus, Loewe [1849, pp. 9-10] wrote that the logic of Kant's day was certainly not exactly the same as the logic of Aristotle's time, that it had shrunk in scope to a "study merely of the forms of thought" (*"eine Lehre bloß von der Form des Denkens"*).

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