Was Kant's philosophy of mathematics right for its time?

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Early in the last century, Kant's views on mathematics, however loosely interpreted, held considerable sway among philosophers. This influence was often for the worse—so much so that "philosophy" (almost invariably understood to be Kantian, or neo-Kantian) could come to be used as a term of abuse among scientists. In a recent book, Michael Friedman remarks that the blame here cannot be laid at Kant's door.[1] It is not reasonable to expect Kant to have made his doctrines relevant to the concerns of twentieth century science.

This comment is most welcome. It was not Kant, but rather his epigones who launched ill-considered challenges to contemporary scientific developments, berating those like Helmholtz, for instance, who dared to question the necessity of Euclidean geometry. This was not at all in the Kantian spirit. For Kant had a healthy respect for the opinions of experts, and might well have sided with Gauss and Riemann on the subject rather than with those who, to use the phrase of Hans Freudenthal, claimed to show that these men were blockheads.[2]

This defense is but a preamble, however, to Friedman's larger purpose, which is to show not only that Kant's remarks should be read with some

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[1]M. Friedman, *Kant and the Exact Sciences* (Cambridge: Harvard University Press, 1992), Part 1, Chapter 2 "Geometry"; this is a slightly revised version of "Kant's philosophy of geometry," *Philosophical Review* **94**(1985)455-506; reprinted in C. Posy ed., *Kant's Philosophy of Mathematics* (Dordrecht: Kluwer, 1992).

[2]H. Freudenthal, "The main trends in the foundations of geometry in the 19th century" in E. Nagel, P. Suppes, A. Tarski eds. *Logic, Methodology and Philosophy of Science* Proc. of 1960 Intl. Congress (Stanford: Stanford Univ. Press, 1962) 613. Among them, as he pointedly remarks, was Frege, who once compared non-Euclidean geometry to alchemy.

sensitivity to historical context, but, much more, that, when thus read, they reveal the depth of Kant's insight into the exact sciences. Kant may not have been right for all time, in his view; but he was certainly right for his own:

> ...Kant is surely not to be reproached [he writes] for failing to anticipate the leading logical and mathematical discoveries of a later age; he is rather to be applauded for the depth and tenacity of his insight into the logical and mathematical practice of his own.[3]

This is an interesting claim, and a marked improvement on the view that Kant was right for all times. The burden of this essay is to show that it too cannot survive an inquiry into the historical details.

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Friedman's defense of Kant's greatness as a philosopher of mathematics rests on several large claims:

First, Kant's celebrated remarks that purely conceptual reasoning is by itself inadequate for mathematics is interpreted in terms of a distinction, borrowed from twentieth-century logic, between first order predicate calculi which contain only monadic, and those which contain polyadic predicate symbols. Kant's conceptual analysis is assimilated first to something like derivability within traditional syllogistic and then to derivability within a fragment of a predicate calculus of the former type. Adducing a well known theorem to the effect that a satisfiable set of monadic formulas with a finite number of predicate symbols has a finite model, Friedman suggests that it was just this feature which Kant singled out in his remarks (in the Transcendental aesthetic) on the incapacities of conceptual reasoning for mathematics.

Here is what Kant had to say:

> Space is represented as an infinite *given* magnitude. Now every concept must indeed be thought as a representation which is contained in an infinite number of different possible representations (as their common character), and which therefore contains

[3] *Kant and the Exact Sciences*, 95. Friedman's claim is considerably broader than this, finding merit in Kant's engagement with the exact sciences generally. This note concerns only Kant's remarks on mathematics.

> these *under* itself; but no concept, as such, can be thought as if it contained an infinite number of representations *within* itself. It is in this latter way, however, that space is thought; for all the parts of space *ad infinitum* exist simultaneously. Consequently, the original representation of space is an *a priori* intuition, not a concept.[4]

Friedman comments:

> We can now begin to see what Kant is getting at in his doctrine of construction in pure intuition. For Kant logic is of course syllogistic logic or (a fragment of) what we call monadic logic. Hence for Kant, one cannot represent or capture the idea of infinity formally or conceptually.... Kant's point [*sc.* at B40] is that (monadic) conceptual representation is quite inadequate for the representation of infinity: (monadic) concepts can never contain an infinity of objects in their very idea, as it were. In particular, then, since our idea of space does have this latter property, it cannot be a (monadic) concept.[5]

In the second place, we are told that logic was trapped in this meagre form until at least 1879, when Frege published his *Begriffsschrift*. Before that time, Friedman maintains, no one could possibly have done any better than Kant did. The depth of Kant's insight is felt to resonate throughout nineteenth century mathematics, and to explain some of its notable lacunae:

> ... the logical resources necessary for a purely axiomatic characterization of continuity (or even of denseness or infinite divisibility, which in Kant's time is of course not clearly distinguished from continuity) simply do not exist until the latter part of the nineteenth century.[6]

We read elsewhere, again, that the distinction between continuity and denseness "...was not even articulated until late in the nineteenth century...,"[7] and also that "the notion of infinite divisibility or denseness ... cannot be represented [*sc.* in Kant's time] by any formula such as $[\forall a \forall b \exists c (a < b \rightarrow a < c < b)]$: this logical form simply does not exist."[8] Because of this putative inad-

[4] *Critique of Pure Reason*, B40 (N.B. References of the form x.y are to Volume x, page y of the academy edition of Kant's works; references to the *Critique of Pure Reason* follow the pagination of the 1781 (A) and 1787 (B) editions, and the translation of Kemp Smith.)

[5] Friedman, *Kant and the Exact Sciences*, 63 f; see also 67 f.

[6] Friedman, *Kant and the Exact Sciences*, 26.

[7] Friedman, *Kant and the Exact Sciences*, 60.

[8] Friedman, *Kant and the Exact Sciences*, 64.

equacy, pure intuition steps (apparently, for the time at least, successfully) into the breach: "Pure intuition...allows us to capture notions like denseness without actually using quantifier dependence. Before the invention of polyadic quantification theory there simply is no alternative."[9]

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The first set of claims leads us into thorny matters of Kant interpretation. Let us begin with the passage at B39-40. The argument is clearly stated, and may be summarized as follows:

> Space is represented as something with infinitely many parts. But no concept has infinitely many parts. So space is not a concept. But there are only two kinds of representations of things, namely, intuitions and concepts; *tertium non datur*. Therefore, etc.

It will be recognized that the first inference involves the tacit assumption that if space has infinitely many parts, the representation of space must also have infinitely many parts. This assumption might seem like a gross error: why should a representation resemble its object, any more than the word "steam" resembles steam? But in fact it is not arbitrary in the slightest, as Kant makes clear elsewhere. For according to him space, like time, *is* a representation; in particular, the representation of space just is space. Now it is certainly true that no terms are to be met with in monadic logic within which we can find (or rather construct) triangles, circles, Cleveland, Jupiter, etc. Somehow, though, it seems strained to suggest that Kant's remarks were aimed, however indirectly, at supporting this claim.

It should be noted, further, that Friedman's construal of Kant's claims about mathematical truths as concerning the incapacities of logic do not have a strong basis in the text. For Kant's discussion of mathematical truths trades not on what he thought *logic*, but on what he thought *conceptual analysis* could accomplish. When he wrote that mathematical truths are not (for the most part) established by analysis, he had something quite specific in mind, namely, that mathematical truths cannot be established by reducing the subject concepts of these judgments into their constituents and finding the predicate there (on such occasions Kant somewhat unreflectively assimilates all judgments to the subject-predicate, universal affirmative form).

[9] Friedman, *Kant and the Exact Sciences*, 64.

Consider, for example, Kant's discussion of the judgment that a triangle has angles which sum to two right angles. If a philosopher attempts to ground this judgment using his method [*nach seiner Art*] (*viz.* the analysis of concepts), Kant writes, he gets nowhere:

> He has nothing but the concept of a figure enclosed by three straight lines, and possessing three angles. However long he meditates on this concept, he will never produce anything new. He can analyse and clarify the concept of a straight line or of an angle or of the number three, but he can never arrive at any properties not already contained in these concepts.[10]

The philosopher, that is, does not ask whether the proposition can be proved by means of others, but instead confines his attention to the terms immediately at hand. The proposition in question is universally valid. If it is analytic, then the predicate "has angle sum equal to two right angles" should be contained in the subject concept "triangle." But nothing of the sort is observed. Instead, we find in the concept "triangle" parts like "straight line", "three", "plane figure"; and Kant quite plausibly maintains that the concept "having angle sum equal to two right angles" cannot be found among the parts of concepts like "three" or "straight". It follows that something else must be involved, and it is here that Kant appeals to pure intuitions.

> [M]athematics must first present all of its concepts in intuition (and in the case of pure mathematics in pure intuition), that is, construct them. As long as pure intuition, in which alone the material for synthetic judgments *a priori* can be given, is lacking, it is impossible to take a single step—for mathematics cannot proceed analytically, i.e. by dissection of concepts, but rather only synthetically.[11]

Later on Frege, with improvident generosity, was to "clarify" Kantian conceptual analysis as logical derivation, and put much more into his logic than Kant could ever have imagined. And Friedman, like many others, has followed Frege at least partway here, equating reasoning which proceeds

[10] A716/B744.

[11] *Prolegomena*, §10 (4.283): "... Mathematik muß alle ihre Begriffe zuerst in der Anschauung und reine Mathematik in der reinen Anschauung darstellen, d.i. sie construiren, ohne welche (weil sie nicht analytisch, nämlich durch Zergliederung der Begriffe, sondern synthetisch verfahren kann) es ihr unmöglich ist, einen Schritt zu thun, so lange ihr nämlich reine Anschauung fehlt, in der allein der Stoff zu synthetischen Urtheilen *a priori* gegeben werden kann." Cf. A6/B10 f.

"analytically according to concepts" with logical reasoning.[12] But there is no compelling reason to read Frege's views back into Kant's remarks. Kant tells us that the Principle of Contradiction, *viz.* "that no predicate contradictory to a thing can belong to it," is "The Highest Principle of all Analytic Judgments," but by this he does not mean, as Frege would have it, that analytic judgments are those which can be established on the basis of logic and definitions alone. Rather, his point is that the judgment "A, which is B, is not B" is self-contradictory, and hence its opposite, "A, which is B, is B" (the typical analytic judgment) is known to be true by the principle of contradiction.[13] By appealing to this "Highest Principle" Kant in no way drags logic in through the back door. Further proof of this is furnished by remarks added in the second edition of the *Critique of Pure Reason* to the effect that the entire burden of analyticity is borne by single judgments. Synthetic propositions, Kant allows, may be derived by means of the principle of contradiction; but "...this can only be if another synthetic proposition is presupposed, and it can then be apprehended as following from this other proposition; *it can never be so discerned in and by itself.*"[14] It might sometimes seem as if Kant were arguing that in any case at least *some* synthetic propositions will have to be accepted without proof. But his words here, as well as his practice elsewhere, tell against this—remember, the mathematician is *always* guided by intuition (B744-745). Kant asks not whether a given proposition can be derived from others, but whether its truth (or falsity) can be discerned within it standing alone, with nothing but the tools of conceptual analysis. To put it bluntly, "...in the analytic proposition the question is only whether I think the predicate in the representation of the subject."[15] If so, it is analytic; otherwise, synthetic. Proof is entirely irrelevant to this determination.

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Even supposing Friedman's interpretation to be true to the spirit of Kant's remarks, however, would it indeed point to a "fruitful philosophical engagement with the sciences"?[16] This would depend materially on an assumption which perhaps comes so naturally to modern readers that it is

[12] *Kant and the Exact Sciences*, 56.
[13] A150/B189 f.
[14] B14, emphasis added; cf. *Prolegomena*, 4.268.
[15] Cf. A164/B205.
[16] Friedman, *Kant and the Exact Sciences*, xii.

all but invisible: namely, that logic has a good deal to do with mathematics. In the eighteenth century, however, the same could not be said. Ever since the scientific revolution, logic—that is, the school logic to which the label was for the most part attached—was widely and accurately regarded as largely irrelevant to scientific practice.[17] Rather than the universal conceptual tool dreamt of by Leibniz, logic was more often thought of as intellectual cod-liver oil, recognized as unpalatable but nevertheless administered by a variety of sour-faced individuals with the avowed aim of improving character in unspecified ways.

The thought that this or any sort of formal logic had an important role to play in mathematics was not seriously entertained. When the article "logic" for the *Encyclopédie* was assigned, for example, it was not d'Alembert or another mathematician who was called upon, but rather Diderot. In the article itself, we find a few disparaging remarks on "artificial" (i.e. Aristotelian/scholastic) logic, and a good deal of lavish praise for "natural" logic. Natural logic is just the correct use of reason: to attempt to formalize it often hinders rather than helps. And so indeed we often find that peasants reason more exactly on the matters which concern them than do the adepts of formal logic, the much derided doctors of the Sorbonne.[18] The thought that one should formalize reason in order to use it correctly is rejected as profoundly misguided: it would be like saying that we have to dissect our legs, study the bones, muscles, etc., in order to learn how to walk.

In ancient times, the article continues, logic justly fell into disrepute. It was characterized by useless subtleties, a multiplicity of recondite terms, and futile squabbling, and was regarded more as a method of dispute than

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[17]The contempt was mutual. Consider for example these remarks from the Port-Royal Logic (*The Art of Thinking*, tr. J. Dickoff and P. James (New York: Bobbs-Merrill, 1964), 7-8):

> A wise man engages in science not to employ his mind but to exercise it. A scientist without this viewpoint fails to see that the study of the speculative sciences—geometry, astronomy, physics—is little else than a somewhat shallow pastime scarcely better than ignorance. Ignorance at least is not painful to acquire and gives no excuse for that foolish vanity so often accompanying sterile and fruitless knowledge.
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> Not only does science contain otiose and barren areas, but even as a whole science is useless. Man was not born to spend his days measuring lines, examining the relations between angles, or considering the many motions of matter. His mind is too large, his life too short, his time too precious to be occupied in such trivia.

[18]Diderot, Art. "Syllogisme," *Encyclopédie, ou Dictionnaire raisonné des sciences, des arts, et des métiers* (Berne and Lausanne, 1780).

of correct thinking:

> Logic was then merely an art of words which often lacked all meaning, but which were exquisitely apt for hiding ignorance instead of perfecting judgment, mocking reason rather than strengthening it, depicting rather than clarifying truth.[19]

The scholastics fared no better, carrying on stupid and seemingly endless quarrels; even today, Diderot remarks, logic has a poor reputation, and the schools are still doing their part to justify it.

I have suggested that even the limited school logic was irrelevant to Kant's division of judgments into analytic and synthetic, and hence to the pronouncements of the critical philosophy on mathematics generally. Even were this not so, as Friedman contends, little merit would accrue to Kant's remarks. To have said that logic was incapable of doing the heavy work in mathematics would have been merely a matter of stating the obvious. If, for example, someone had suggested to Euler that he should look to logic for help in establishing the equation $e^{\pi i} = -1$, he would quite appropriately have been looked upon as singularly ill-informed. But why speak of Euler here? Even *Aristotle* had put many a mathematical proof outside the scope of logic. In the *Prior Analytics*, he took up one of Kant's examples, namely, that the sum of the angles in a triangle equals two right angles. He remarked:

> We must not always seek to set out the terms in a single word: for we shall often have complexes of words to which a single name is not given. Hence it is difficult to reduce syllogisms with such terms. Sometimes too fallacies will result from such a search, e.g. the belief that syllogism can establish that which has no mean. Let A stand for two right angles, B for triangle, C for isosceles triangle. A then belongs to C because of B: but A belongs to B without the mediation of another term: for the triangle in virtue of its own nature contains two right angles, consequently there will be no middle term for the proposition AB, although it is demonstrable.[20]

Thus from the very beginning of syllogistic it was acknowledged that not all demonstrations can be put in syllogistic form.

[19] *Encyclopédie*, Art. "Logique," 20.241: "La logique n'étoit alors qu'un art de mots qui n'avoient souvent aucun sens, mais qui étoient merveilleusement propres à cacher l'ignorance au lieu de perfectionner le jugement, à se jouer de la raison plutôt qu'à la fortifier, à defigurer la verité plutôt qu'à l'éclaircir."

[20] *An. pr.*, I, 35, ($48^a 29 - 39$).

We should not therefore ask what logic could do for eighteenth-century mathematics, but instead return to Kant's original question, namely: could mathematics make do with concepts alone? Was there indeed no alternative to pure intuition before Frege? This brings us to Friedman's second set of claims, and, coincidentally, also back to logic for a moment.

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The substance of Friedman's remarks on logic is that new logical resources—notably nested quantifiers and relation-terms—first came into being with the work of Frege, and that before Frege such logical forms did not exist. Now if what is meant by this is that there was no adequate general theory of quantificational logic supporting such forms before Frege, one might well agree. But this says very little. There was no adequate general theory of differential equations in the eighteenth century, to take a similar example, and yet there were those at that time who indisputably knew a lot about the subject. So too with logic and conceptual reasoning. Before Frege, there were certainly those who indicated knowledge of certain aspects of quantificational reasoning—enough certainly, to outstrip the promise of a vague appeal to intuition. (That such reasoning was not usually tagged with the dubious honorific "logical" matters not in the slightest.) Friedman himself recognizes this in the work of Weierstrass,[21] whose conceptual analyses helped pave the way for Frege's construction of a logical system. But we needn't look so late to find evidence of such things. Already in antiquity, the theories of proportion set out in Books V and VII-IX of Euclid's *Elements* make essential use of relation terms (proportion, for example, is a four-term relation), and precise and sophisticated quantificational reasoning. Aristotle, for his part, also made use of relation terms and quantificational expressions when setting out the theory of the syllogism. Here is one example from the *Prior Analytics*:

> Whenever three terms are so related to one another that the last is contained in the middle as a whole, and the middle is either contained in, or excluded from, the first as in or from a whole, the extremes must be related by a perfect syllogism.[22]

Closer to Kant's time, we find Leibniz remarking that conceptual reasoning is not, in general, reducible to syllogistics:

[21] Friedman, *Kant and the Exact Sciences*, 80n39.
[22] *An. Pr.*, I, 4 ($25^b 32f$).

> ... there are valid non-syllogistic inferences which cannot be rigorously demonstrated in any syllogism unless the terms are changed a little, and this altering of the terms is the non-syllogistic inference. There are several of these, including arguments from the direct to the indirect—e.g. 'If Jesus Christ is God, then the mother of Jesus Christ is the mother of God.'[23]

Leibniz also included in his generous notion of logic the ratio theory of Book V of the *Elements*—a body of results which is not readily accommodated within ordinary syllogistic.[24] It is clear from his remarks not only that he appreciated these capacities of conceptual reasoning, but also that he was prepared to recognize them as logical.

Lagrange affords us an example from mathematics. Often portrayed as the last great proponent of the eighteenth-century "algebraic" calculus, he was nevertheless capable of giving a rather precise, purely arithmetical statement of what a limit is. Working with the remainder of a general Taylor series in his *Leçons sur le calcul des fonctions*, he arrives at the following expression:

$$f(x+i) = f(x) + i\left[f'(x) + V\right]$$

where V is a function of i which is zero when i is.[25] He then gives the following description of the behaviour of V near $i = 0$.

> ... one can always assign i a value such that the corresponding value of V will be less in absolute value than a given quantity, and that for smaller values of i, the value of V will also be smaller [*sc.* than the given quantity].[26]

A perfectly acceptable statement of what we would now express by saying that $\lim_{i\to 0} V = 0$.

[23] *New Essays on Human Understanding*, tr. P. Remnant and J. Bennett (Cambridge: Cambridge University Press, 1982), Book IV, chap. xvii.

[24] Leibniz, *Loc. cit.*; also Book IV, chap. ii: "... geometers' logic—that is, the methods of argument which Euclid explained and established through his treatment of proportions—can be regarded as an extension or particular application of general logic." For an illuminating discussion of attempts to shoehorn ratio theory into syllogistic form in antiquity see I. Mueller, "Greek mathematics and Greek logic," in J. Corcoran (ed.) *Ancient Logic and its Modern Interpretations* (Dordrecht: Reidel, 1974) 35-70.

[25] J.-L. Lagrange, *Leçons sur le calcul des fonctions*; *Œuvres*, Vol. X, 9^e leçon.

[26] "... on pourra toujours donner à i une valeur telle que la valeur correspondante de V, abstraction fait du signe, soit moindre qu'une quantité donnée, et que pour les valeurs moindres de i la valeur de V soit aussi moindres." Cf. *Théorie des fonctions analytiques*, *Œuvres*, 9.28.

To cite but one more example, consider the following theorem, stated and proved by Bolzano in his 1817 paper on the intermediate value theorem:

> If a property M does not belong to all values of a variable x, but does belong to all values which are less than a certain u, then there is always a quantity U which is the greatest of those of which it can be asserted that all smaller x have the property M.[27]

A plausible rendering of this statement is (with the variables ranging over the reals):

$$(\neg\forall x Mx \wedge \exists u \forall x(x < u \rightarrow Mx)) \rightarrow \\ (\exists U \forall x(x < U \rightarrow Mx) \wedge \neg\exists z(U < z \wedge \forall x(x < z \rightarrow Mx)))$$

This rather complex formulation is in effect a statement of the completeness of the real numbers. It is a remarkably precise one, indicating that German was quite up to the task of advanced mathematical research. It is worth noting, in passing, that a similar characterization of the least upper bound property may be found in an unpublished manuscript of Gauss from around 1800,[28] and that Bolzano (in the 1830s) did in fact distinguish precisely between continuity and denseness, as well as between pointwise and uniform continuity on an interval.[29] This is not to say that before Frege all the problems of quantificational reasoning were solved; but to say that such reasoning did not or, still more, *could* not then have existed, seems patently false.

Nor should we be surprised by this: Lagrange and Gauss undoubtedly possessed talent, but they got such results in large part because they thought it worthwhile to try. With Kant, things were quite otherwise: nothing prevented him from uttering a sentence like "Between any two points there lies a third" (he *did* write that "Between two instants there is always a time. . . ."[30]), and trying, with the help of such statements, to construct

[27] Bolzano, *Rein analytischer Beweis*, §12; quoted after S. B. Russ, "A translation of Bolzano's paper on the Intermediate Value Theorem," *Historia Mathematica* **7** (1980) 156-185, 174.

[28] "Grundbegriffe der Lehre von den Reihen" *Werke*, X.1.391-392.

[29] Cf. the manuscript quoted in D. Johnson, "Prelude to dimension theory: the geometrical investigations of Bernard Bolzano," *Arch. Hist. Ex. Sci.* **17**(1977)261-95, 282; P. Rusnock and A. Kerr-Lawson, "Bolzano and uniform continuity" *Historia Mathematica* (in press).

[30] A208/B253. Compare also A168/B210: "Every sensation, however, is capable of diminution, so that it can decrease and gradually vanish. Between reality in the [field

more adequate theories. Nothing, that is, but his firm conviction that that pursuit was both unnecessary and futile. Why strain after purely conceptual descriptions, after all, when mathematics already has a perfectly solid foundation in intuition? If we were to do this, would we not simply import into mathematics all the endless disputes which plague philosophy?

Frege reminds us that one of the central tasks of the logician is to "... conduct an unceasing struggle against psychology and those parts of language and grammar which fail to give untrammelled expression to what is logical."[31] Such a struggle would be pointless if language and grammar did not reflect *something* of logical significance. The resources of Kant's German, for example, were sufficient for him to be able to write that "The relationship of the highest authority in the state to the people may be conceived in three ways: a single person in the state has command over all, or several persons who are equal and united have command over all the rest, or all the people together have command over each person, including themselves"[32] —a statement which indicates that relational and quantificational terms were very much ready to hand. This point is abundantly confirmed by the merest survey of the mathematics of Cauchy, Abel, Dirichlet, and others. They did not need Frege to teach them how to develop mathematical analysis using quantificational concepts. Rather it was the other way around: Frege could securely draw on a wealth of quantificational reasoning in mathematics (and indeed in ordinary language) while developing his logic. Thus it was not a lack of logical forms (whatever that might mean) which led Kant to pure intuition, but rather a simple failure to notice the richness of the conceptual resources he had.

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The picture of Kant's work in the philosophy of mathematics which emerges from Friedman's discussion is one of deep, intense research, based on a wide-ranging knowledge of contemporary mathematics, turning on rather

of] appearance and negation there is therefore a continuity of many possible intermediate sensations, the difference between any two of which is always smaller than the difference between the given sensation and zero or complete negation."

[31] "Logic "(between 1879 and 1891), *Posthumous Writings* ed. H. Hermes, F. Kambartel, F. Kaulbach tr. P. Long, R. White (Oxford: Blackwell, 1979), 6-7.

[32] *Metaphysics of Morals*, Part I (Metaphysical Elements of Justice), §51 (6.338) quoted after J. Ladd tr., *The Metaphysical Elements of Justice* (Indianapolis: Bobbs-Merrill, 1965) 109-110.

subtle points, and involving a good deal of sophisticated technique. But this portrait is a most unlikely one. It is easy to forget, perhaps, given Germany's status as a mathematical powerhouse in the second half of the nineteenth century, that in Kant's time it was still a backwater. Leibniz, in the seventeenth century, confessed that before going to Paris he was almost entirely ignorant of mathematics. Predominantly French islands like the Berlin Academy aside, little had changed by Kant's time. Well into the nineteenth century, Crelle (editor of the famous *Journal*) told Abel before pushing him towards Paris that "the knowledge of the majority of *mathematicians* [in Germany] amounts to a little geometry and to something that they call analysis, but which is nothing other than the theory of combinations."[33] Dirichlet, around the same time, would make the same trip Abel had, and for similar reasons: mathematics was still largely a French product (Gauss, of course, was a notable—but lonely—exception).

If German mathematicians were—to put the thing politely—not entirely up to date, how much more surprising would this be for a philosopher. And indeed, we find that Kant was strikingly out of step with the leading mathematicians of his time. In the very years that he was underlining the crucial importance of construction and intuition, Euler and Lagrange were leading the charge to purge all such elements from analysis and even mechanics. The preface of Lagrange's *Mécanique analytique* (1788), for example, announced that:

> No figures will be found in this work. The methods which I set forth here require neither constructions nor geometrical or mechanical reasonings, but only algebraic operations, subject to regular and uniform procedures. Those who love analysis will be pleased to see mechanics become another branch of the field, and will be grateful to me for having enlarged its domain.[34]

Now the approach to analysis which Lagrange appeals to here (which began at least as early as Leibniz), where geometry, along with intuition, construction, etc., has no role to play, did raise all manner of important philosophical problems, problems which were treated with great depth and subtlety by, among others, Euler, Gauss, Cauchy, Bolzano, Abel, and Dirichlet. This point has received some recognition,[35] but not nearly as much as it deserves.

[33] Quoted after U. Bottazzini, *The higher calculus: a history of real and complex analysis from Euler to Weierstrass*, tr. W. Van Egmond (New York: Springer, 1986), p. 83-4 n.17; emphasis added.

[34] J.-L. Lagrange, *Mécanique analytique* (Paris, 1788), Avertissement.

[35] See for example A. Robinson, *Non-standard analysis*(Amsterdam: North-Holland,

Needless to say, Kant did not contribute anything to these developments, nor was he in a position to do so.

Despite the preponderance of direct and circumstantial evidence, however, the tradition of attributing mathematical and scientific prowess to Kant is a long one. Already in 1924, Erich Adickes was able to collect an impressive list of tributes to Kant's scientific genius. His detailed familiarity with Kant's work had given him quite a different view of things. Kant's manuscript writings on mathematical topics, of which Adickes had produced an exemplary edition, he qualifies as being "without any significance for mathematics as a science, or for its history."[36] Instead of setting out sharp definitions and sticking to them, Kant leaves his concepts vague and ambiguous, interpreting them now one way, now another; as a consequence, his remarks are liberally peppered with "serious logical mistakes, confusions, homonymies, and similar maladies."[37] Kant does not often use symbols in his writings, and had little facility with them: "Only relatively rarely does he set out exact equations and when he does ... they are frequently incorrect."[38] Kant's approach to scientific questions is assessed quite harshly:

> Not even in those days would anyone familiar with the methods of science have proceeded in this way. To him, Kant's method would have seemed not merely useless, but disgraceful, in that instead of taking us out of confusions it can only lead us deeper into them. The thought never would have occurred to him to make things easier by renouncing formulae, calculations and symbolization in favor of setting out half-correct or completely false propositions full of ambiguous concepts.[39]

1966): "Nowadays the Philosophy of Mathematics is considered in connection with more general notions such as consistency, predicativity, or constructivity. So it might seem to us that the evolution of the foundations of the calculus amounts merely to the development of certain mathematical techniques, and the rejection or modification of such techniques on the grounds of obvious inconsistency. The picture is incomplete. It ignores the fact that, from the seventeenth to the nineteenth century, the history of the Philosophy of Mathematics is largely identical with the history of the foundations of the Calculus."

[36] E. Adickes, *Kant als Naturforscher* (Berlin: de Gruyter, 1924), 1.19.

[37] Adickes, 1.17: "schwere logische Fehler, Verwechselungen, Homonymien und ähnlichen Unzuträglichkeiten."

[38] Adickes, 2.483-4.

[39] Adickes, 1.19: "Auch schon zu jener Zeit [würde] ein an naturwissenschaftliche Arbeitsweise gewöhnter niemals so vorgegangen sein. Ihm wäre ohne Zweifel Kants Art nicht bloß nutzlos, sondern geradezu schändlich erschienen, weil sie statt aus Unklarheiten heraus nur tiefer in sie hineinführen kann. Nie wäre ihm der Gedanke gekommen, sich die Sache dadurch zu erleichtern, daß er unter Verzicht auf Formeln, Rechnungen und Zeichnungen halb richtige oder ganz falsche Sätze voll von vieldeutigen Begriffen niederschrieb."

At the same time, as Adickes documents, praise for Kant's greatness, not as a philosopher, but as a scientist was both effusive and widespread. No doubt astonished by the discrepancy between Kant's accomplishments and the tributes heaped on him, Adickes' exasperation is palpable. He writes:

> Du Bois Reymond says somewhere that with Kant there ended "the series of philosophers who, being in complete command of the natural science of their time, themselves participated in the work of the natural scientist." With this he clearly intends to include Kant in a series with men such as Descartes and Leibniz. But most incorrectly! According to their entire frame of mind these two philosophers were real natural scientists and mathematicians. Not so Kant. He could avail himself of neither of the two important tools through which modern natural science has expanded: experiment and mathematics.[40]

In illustration of this point, Adickes adduces several examples, among them Kant's attempt (dating from the late 1770's) to approximate the circumference and area of a circle via inscribed polygons.[41] This, he remarks, is a problem of elementary geometry which is easily solved by an average upper level high school student. Kant, however, gets tangled up in difficulties without finding his way, and indeed shows himself all but incapable of reaching a solution. Further examples are equally painful. But these by themselves do no particular harm to Kant's standing as a philosopher; rather only to some of the claims which have been made concerning his abilities as a scientist.

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Kant's philosophy of mathematics was not—I have argued—right for its time in the sense that Hilbert's was; that is, Kant's thoughts did little or nothing to articulate and inform contemporary mathematical practice. In

[40] Adickes, 1.6: "Du Bois Reymond sagt einmal, mit Kant ende 'die Reihe der Philosophen, die im Vollbesitz der naturwissenschaftlichen Kenntnisse ihrer Zeit sich selber an der Arbeit der Naturforscher beteiligten.' Damit will er offenbar Kant in eine Reihe mit Männern wie Descartes und Leibniz stellen. Aber sehr mit Unrecht! Diese beiden Philosophen waren ihrer ganzen Geistesart nach zugleich wirkliche Naturwissenschaftler bzw. Mathematiker. Nicht so Kant. Er hat sich nie der beiden wichtigen Hilfsmittel zu bedienen gewußt, durch welche die moderne Naturwissenschaft groß geworden ist: des Experiments und der Mathematik."

[41] Adickes, *Kant als Naturforscher*, 1.19 f.

another sense, however, we can say that Kant's views were right for their time; namely, in that they more or less reflected the conventional wisdom of his day. To illustrate this, I would like to draw attention to some of Kant's remarks on mathematics from the Prize Essay of 1764.[42]

Kant prefaces his piece with a remark that he is merely going to set out indubitable empirical propositions about the practice of mathematics and philosophy. Thus, he was not claiming any novelty for his views, but rather attempting to stress that they were generally accepted and based on an examination of the practice of mathematicians. He begins by stating that mathematics and philosophy differ in that mathematics always arrives at definitions of its concepts synthetically, while philosophy does so analytically. His remarks should not be overinterpreted. What he means is that mathematicians take some concepts as given and combine them (more or less arbitrarily) to make others: so, to take his example, starting from the concepts "straight line", "four", "enclose", "plane", "not", "parallel", we form the concept of a trapezium as "a figure bounded by four straight lines enclosing a plane surface with the opposite sides not parallel". Philosophers begin, on the other hand, with complex concepts which they (along with everyone else) already possess, and attempt to discern their parts.

One may see in this characterization of mathematics an echo of Pascal's essay on the *esprit géométrique* (which had been summarized in the Port Royal Logic). Pascal had explained that the method of geometry was to start with concepts which are clear in themselves, and to define all others in terms of these. As for the concepts adopted as primitive—those which are so clearly known that no clearer terms exist with which they might be defined—Pascal saw no point in attempting to define them, thinking that this would result only in absurdities (as an example, he mentions an acquaintance who defined light as a luminary movement of luminous bodies).

So too with Kant; mathematicians have, so he believes, a relatively small stock of concepts which are clearly known in themselves. Even if they can be analysed—that is, broken down into their components—this analysis would not belong to mathematics. A more minute description of the concept of a straight line, he thinks, can have no impact on geometry (for the geometer already knows all he needs to about straight lines; a further definition would simply support what is already known).

In Philosophy, by contrast, one begins with concepts derived from common usage—concepts like "right", "virtue", "time". We cannot get at con-

[42] I remark in passing that the mature expression of Kant's doctrines in the *Critique of Pure Reason* (A713/B741 f) differs but slightly from this early account.

cepts like these by arbitrary combinations of given concepts. For one thing, philosophy has far more concepts than mathematics, and most of them are not clearly known. For another, it would be a real stroke of luck—almost a miracle—if by arbitrarily combining concepts we happened to hit upon the definition of a given, but confused concept:

> For example, everyone has a concept of time. But suppose that that concept has to be defined. The idea of time has to be examined in all kinds of relations if its characteristic marks are to be discovered by means of analysis: different characteristic marks which have been abstracted have to be combined together to see whether they yield an adequate concept; they have to be collated with each other to see whether one characteristic mark does not partly include another within itself. If, in this case, I had tried to arrive at a definition of time synthetically, it would have had to have been a happy coincidence indeed if the concept, thus reached synthetically, had been exactly the same as that which completely expresses the idea through which time is given to us. (Prize Essay, §1, [2.276-277])

Kant notes that some have attempted to define mathematical concepts analytically, but claims that this is a serious mistake. He then adduces an exquisitely inappropriate example:

> Mathematicians, on the other hand, it must be admitted, have sometimes offered analytic definitions as well. It was in this way that Wolff considered similarity in geometry: he looked at it with a philosophical eye, with a view to subsuming the geometrical concept of similarity under the general concept. But he could have spared himself the trouble. If I think of figures, in which the angles enclosed by the lines of the perimeter are equal to each other, and in which the sides enclosing these angles are in the same ratio—this can always be regarded as the definition of similarity between figures, and likewise for the other similarities between spaces. The general definition of similarity is of no concern whatever to the geometer. It is fortunate for mathematics that, even though the geometer from time to time gets involved in the business of analytic definitions as a result of a false conception of his task, in the end nothing is actually inferred from such definitions, or, at any rate, the immediate inferences which he draws ultimately constitute the mathematical definition it-

> self. Otherwise this science would be liable to exactly the same wretched discord as philosophy. (Prize Essay, §1 (2.277))

The question of similarity has been treated elsewhere.[43] At the risk of repetition, here is a sketch of what is involved. In classical Greek geometry, the word "similar" is used in a wide variety of contexts, but is never given a general definition. Instead, we have a collection of so-called "definitions" for individual cases: polygons, segments of circles, triangles, polyhedra, and so on. Although there is no general definition, it seems reasonable to conjecture that the intention was to capture the meaning of something like "sameness of shape". In particular, it seems to have been desired that geometrical objects which are similar and equal (that is, have the same shape and size) should be congruent.

Thus viewed, the "definitions" of individual cases of similarities really aim at setting out necessary and sufficient conditions for figures of equal size to be congruent. That is, the Euclidean definitions look more like theorems than what we nowadays usually call definitions.

Now had Kant reflected on this matter, he would have realized that arriving at such conditions is much more like the case of defining "time" than "trapezium". Yes, we could arbitrarily combine concepts and arrive at a concept of similarity for polygons; but to arrive at the usual one by this method would be something of a miracle.[44]

Again, if sameness of form or shape is what we want to capture in our notion of similarity, and supposing that the concept of having the same shape is a given concept (i.e. we can reliably recognize when two figures have the same shape), Wolff's procedure would be entirely appropriate. We do want to bring the geometrical concept of similarity under the general concept of similarity. What Wolff had done, following Leibniz as best he could, was to provide a list of geometrical characteristics, and to divide these into two categories: those pertaining to form, and those pertaining to size. He could then define the similarity of two geometrical objects as follows: A and B are similar if they are identical as regards their form.

This is a special case of what later mathematicians would call homomorphism; and the list of characteristics given by Leibniz and Wolff as pertaining

[43] P. Rusnock and R. George, "A Last Shot at Kant and incongruent counterparts" *Kantstudien* **86** (1995) 257-277.

[44] Even in simple cases it may be difficult to formulate an adequate definition. Kant himself illustrates this in the *Critique of pure reason* when, immediately after stating that mathematical definitions can never be mistaken (A731-2/B759-60), proceeds to define a circle as a line "...every point in which is equidistant from one and the same point," a condition satisfied by any squiggle on a sphere.

to form are in fact invariant under the usual Euclidean group of motions, along with uniform changes of scale and reflections through a plane. A modest success, perhaps, but among the first (if not the first) in similarity theory since antiquity. Kant cites it as an example of a profoundly mistaken approach to mathematics. It seems fair to say that he did not have a good feel for these things.

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I have suggested above that Kant's philosophy of mathematics was a more or less faithful reflection of the conventional wisdom of his time. A common feature of the conventional wisdom is that it applies considerably better to the past than to the present. So too here. The views which Kant put forth in the Prize Essay (and again in the *Critique of Pure Reason*) fit the practice of classical Greek geometry fairly well (ignoring parts like Books V and VII-X of Euclid's *Elements*); they also fit the new mathematical analysis which Descartes had set out in his *Géométrie* of 1637. Descartes, memorably, had developed a constructive geometrical semantics for algebra. Algebraic operations (with exceptions like taking the square root of -1) were mapped onto geometrical constructions. This is all highly intelligible, constructive mathematics which fits Kant's description fairly well.[45]

But the publication of the *Géométrie* marked the beginning of a creative explosion of mathematics, in which the scruples which had preserved Cartesian intelligibility were soon cast aside. The constructive geometrical semantics was abandoned; infinitary processes, like those of the theory of series or the differential and integral calculus were added to Cartesian algebra, most of them not fully understood or constrained. By the mid-eighteenth century, mathematics was nothing like either Greek geometry or Cartesian algebra. One might hope that this new mathematics could be put on a footing of the sort described by Pascal and repeated by Kant, one beginning with concepts known clearly in themselves and axioms known clearly to be true, and proceeding through definitions and proofs to more advanced results. But this was just a hope, one which pointed to a project of immense

[45]Though at this point it is not out of place to remark that it fits Descartes' description better still. For while Kant is often hailed as a father of constructivism, he seems to have had little or no awareness of just how much of the mathematics of his day was non-constructive. Descartes, by contrast, was quite explicit about where constructivity (and thus mathematics) ended and something else began. See, for example, his discussion of "mechanical" curves in book 2 of the *Géométrie*.

scale. It was by no stretch of the imagination an accurate description of the current state of the field.

Infinitesimal analysis as Kant could have found it in the textbooks of Euler and others contained concepts like "function", "sum of an infinite series", "integral", "solution to a differential equation" (one might even add "number") which were by no means put together by arbitrarily combining concepts clearly known in themselves. Nor was it always the case, as Kant suggested, that mathematicians knew exactly what these concepts contained because they had themselves created them. Even when they did create concepts themselves, it was not always clear how to deal with them. The concepts of a logarithm and of -1 might be well known; but did that mean that $\log(-1)$ was understood? Why then the lengthy discussion among Leibniz, the Bernoullis, Euler on this question?

Again, Kant had said that because mathematicians know exactly what their concepts contain, ambiguities of the sort which plague philosophy cannot occur in mathematics (2.285). This lack of ambiguity allows mathematicians to use symbols, sensible aids to cognition, while philosophers are limited to words, and have to be careful at every moment that they have attached meaning consistently to their expressions.(*Ibid.*)

On the face of things, this observation seems fair enough: philosophers have protracted verbal disputes, mathematicians agreed upon algorithms. But these symbolic tools seem to be taken entirely for granted, almost as if they were products of nature.

> ... [mathematics] also constructs magnitude as such (*quantitas*), as in algebra. In this it abstracts completely from the properties of the object that is to be thought in terms of such a concept of magnitude. *It then chooses [!] a certain notation* for all constructions of magnitude as such (numbers), that is, for addition, subtraction, extraction of roots, etc. Once it has adopted a notation for the general concept of magnitude so far as their different relations are concerned, it exhibits in intuition, in accordance with certain rules, all the various operations through which the magnitudes are produced and modified. ... and thus in algebra by means of a symbolic construction, just as in geometry by means of an ostensive construction (the geometrical construction of the objects themselves), we succeed in arriving at results which discursive cognition could never have reached by means of mere concepts.[46]

[46] A717/B745, emphasis added.

Not even on the horizon is the problem of how to go about devising a calculus, or testing its adequacy for a given purpose. Why, for example, do algebraic calculations sometimes succeed in establishing results valid for geometry, and what are the conditions required for success? Are there any limits on the use of algorithms? Are any problems posed, in particular, by the passage from the finite to the infinite? Is it always correct to infer, for example, that

$$\sum_{0}^{\infty} \int f_n = \int \sum_{0}^{\infty} f_n$$

Or again, how are expressions involving power series to be understood? One has, for example, the formula

$$\frac{1}{1+x} = 1 - x + x^2 - x^3 + \cdots$$

D'Alembert had expressed doubts about the way some mathematicians applied this result. He was disturbed, in particular, by the nonchalance with which the series was assigned a sum even when it diverged. Did it really make sense, he wondered, to say with Euler[47] that, for instance:

$$\frac{1}{3} = 1 - 2 + 4 - 8 + 16 - \cdots ?$$

Lagrange,[48] thought it did, and had stated that one could always substitute one of these expressions for the other, regardless of the value of x—to contest this, he said, would be to overturn the most common principles of analysis. It is difficult to discern any important difference between this dispute and those dubbed "philosophical" by Kant. Both seem to involve conceptual clarification, and here the mathematicians' symbols and algorithms are no help, since their use and interpretation is precisely what is in question.

We find not the slightest hint in Kant's writings that he appreciated, or was even aware of, these and other *philosophical* difficulties faced by eighteenth-century mathematicians. It should be added that Kant not only made no effort himself to analyse mathematical concepts, but actively discouraged others from undertaking the task. Such attempts, he wrote pointedly, "result in mere chatter."[49] This opinion was—stunningly—incorrect.

[47] "De seriebus divergentibus,"(1755) *Leonhardi Euleri opera omnia*, (Bern, 1911-1975)(1)14,585-617.

[48] "Addition aux premières recherches sur la nature et la propogation du son," *Misc. Turin.*, II (1761); reprinted Lagrange, *Oeuvres*, I.319-332, 323.

[49] A727/B755.

It is just the sort of error one might expect from one, like Kant, whose acquaintance with mathematics did not embrace the details. It should come as no surprise, therefore, that his thoughts did not have much relevance to the mathematics of his time. More surprising are the often repeated attempts to show that, in some unexpected sense, they did.