ontologically committing quantification over worlds. But I cannot pursue that point here.

[7] I say that I *recommend* that the primitivist should construe the world predicate 'W' as applying to at most one object in any possible situation since the modal primitivist, as she has been characterized so far, is not committed to the essential singularity of the world. The primitivist's official commitments end with the acceptance of '$\Diamond$' as her only modal resource and so a primitivist could consistently endorse a metaphysic according to which there is, or could be, a plurality of worlds construed as mutually isolated spacetimes or *island universes* that have, so to speak, no special modal significance. But I take the position that combines modal primitivism with the toleration of a plurality of co-existing island universe worlds to be philosophically unmotivated and I will assume hereafter that the modal primitivist denies the possibility of the co-existence of a plurality of worlds. This assumption comes into play especially in meeting the remaining objections to (PGS). I am grateful to the anonymous reader whose objections have made me see the need to make the assumption explicit.

[8] And I *am* assuming that the primitivist holds that there cannot be a compossible plurality of worlds (see note 7).

[9] I use the existential quantifiers 'E' and universal quantifier 'Π' to represent the modal realist's unrestricted quantification over the totality of possible entities.

[10] This point emerges clearly from consideration of (PSS) and of (PGS*) in note 5 above. It also bears emphasis that what I am claiming is that, from a primitivist point of view, strong and (non-trivial) global supervenience theses have a common logical form and so, we might say, such global supervenience claims are strong supervenience claims about objects of a special kind i.e. worlds. But this is a quite distinct claim from the erroneous claim (c.f. Kim (1984)) that strong and global supervenience claims are equivalent.

[11] Justification: consider all of the possible worlds in which the sense of all English expressions are as they are in the actual world. It is evident that within each of these worlds synonymous expressions (e.g. 'The fastest man' and 'The quickest man') will have the same referent. It is also evident that one expression type (e.g. 'The fastest man'), although it has the same sense across the worlds in question, may pick out distinct referents across those worlds. The Fregean thesis that sense determines reference thus emerges as the thesis of the weak supervenience of reference on sense.

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REVIEW ESSAY: THE IMPORTANCE OF THE HISTORY OF SCIENCE FOR PHILOSOPHY IN GENERAL

Daniel Garber: 1992, *Descartes' Metaphysical Physics*, University of Chicago Press, Chicago and London, xiv + 389 pp.

Michael Friedman: 1992, *Kant and the Exact Sciences*, Harvard University Press, Cambridge, Mass., and London, xvii + 357 pp.

Since the sixties and seventies a discussion has been simmering about the relevance of the history of science for the philosophy of science, and *vice versa*. The appearance of Garber (1992) and Friedman (1992) draws attention to a different relationship between the history of science and philosophy: the direct relationship between science and the central philosophical topics of metaphysics and theory of knowledge (or of cognition) in the early modern period. These books remind us that the philosophical projects of the two most prominent early modern philosophers cannot be understood without taking full cognizance of their relations to early modern science: Descartes as an initiator of the new science, and Kant as one who believed that the fundamental principles of metaphysics must take into account and account for basic scientific concepts and principles.

Whether for good or ill, our current conception of philosophy is conditioned by our understanding of the early modern period and our relation to it. But our understanding of the early modern period has been distorted by the use to which early modern texts were put, especially during the middle decades of this century, by sense-data epistemologists and their interlocutors. The resulting image of modern philosophy as seeking a position of detached and imperious judgment on the "cognitive claims" of all other disciplines is a fabrication of that period (Rorty's 1979 portrayals of Descartes and Kant notwithstanding). That image of philosophy was deleterious to the practice of theoretical philosophy during those decades: among other things, it resulted in an epistemology that tediously revised abstract necessary and sufficient conditions for "S knows that P". During the same time, the philosophy of science ignored the example set by the classical texts from the early modern period and carried on discussions of

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topics such as scientific explanation and the relation of theory to evidence that were largely or wholly divorced from the sciences themselves.

Some relief from this unhappy situation can and has come from within: philosophers have begun to see that it makes no sense to announce grand claims about the possibility or impossibility of a "scientific psychology" while remaining blissfully ignorant of the structure, content, and theoretical positions within the extant scientific psychology. More permanent relief will be found by rethinking the story we tell about the modern origin of our philosophical problems and concerns. Here, Garber's and Friedman's important books provide a wealth of information and analysis. They make evident that Descartes and Kant were not engaged in unmotivated projects to assess the "accuracy of representation" (Rorty, 1979, Chapter 3) afforded the human mind, but were instead deeply engaged in the central theoretical pursuits of their time in physics and mathematics. More generally, these books display the historical connection between science and philosophy itself. They do not aim to reconstruct Descartes' and Kant's general "philosophies of science" (e.g., their views on methodology and explanation); rather, they show the place of science in, or the scientific context of, these authors' central metaphysical doctrines, pertaining to substance and its properties, the metaphysics of force and causal agency, the relation of geometry to matter and to space, and the structure of cognition. In so doing, they provide us with an image of philosophy as an intellectually and culturally engaged enterprise, an image according to which it makes little sense to make pronouncements about the foundations of knowledge or the possibilities for science without direct engagement with the living practice of scientific and other cognitive enterprises. In this connection, Garber's and Friedman's books can contribute to the reshaping of contemporary philosophy itself.

1. GARBER: METAPHYSICS AND "PHYSICS" IN DESCARTES[1]

Descartes was a mathematical scientist a decade before he was a philosopher in our sense, and his mature philosophical works were undertaken largely in the service of his program to found a new physics. Any genuine understanding of Descartes' philosophical work must accommodate these basic facts. They have long been acknowledged in French scholarship (e.g., by Gilson, Gueroult, and Koyré) and they were fully acknowledged in earlier English-language commentaries by Kemp Smith (1902, 1952), Gibson (1932), and Keeling (1934/1968). However, after mid century, following the attempted purge of history from Anglo-American philosophy, these facts were largely ignored in English-language work on Descartes,

in favor of responses to systematic "problems" posed by Descartes, viz., attempts to blunt the skeptic's wedge of the First Meditation, ruminations on the *cogito* of the Second Meditation, rejections of the theory of ideas and the supposedly attendant "veil of perception" problematic, examinations of Descartes' foundationalist program considered in the abstract, analyses of his arguments for the existence of God, and critical diagnoses of his arguments for a mind-body distinction.[2]

Against this background, Garber (1992) is a welcome sight. It unabashedly sets out to provide a general treatment of Descartes' physics in relation to his metaphysics, as a corrective to the neglect of Descartes' "larger program" by Garber's teachers (p. ix). Garber's approach is contextual: he seeks to understand Descartes' project in its intellectual setting. After recounting Descartes' scientific and philosophical development, the book examines his method, his theory of body and its contrast with Aristotelian and atomist theories, and his analysis of motion, motion's laws, and their grounding in God's immutability. Garber does not claim to present a new, general interpretation of Descartes' philosophical and scientific thought (p. 3); rather he has synthesized the work on Descartes' metaphysical physics, drawing heavily on French scholarship, on recent specialist studies of Descartes' science in English, on the literature of the history of science, and on his own comprehensive study of Descartes' writings and those of other seventeenth-century figures.

Garber claims novelty (p. 3) for his interpretations of method and of motion, in chapters two and six. With regard to method, he contends that Descartes had abandoned his method in actual practice by the time of the *Discourse*. He rightly stresses (pp. 48–50) that Descartes' famous method as worked out in the *Rules* and summarized in the *Discourse* was a method for solving particular problems or discovering particular principles, and did not prescribe the mode of reasoning Descartes adopted later, when the very framework of thought was to be challenged and rebuilt. This claim, though not new,[3] provides a helpful corrective to easy talk of a single "Cartesian method." What I find most remarkable about Garber's treatment of Descartes' method is his seeming assumption that Descartes uniformly demanded certainty (p. 59) and rejected hypothetical reasoning (pp. 46, 62) in his scientific work. Garber all but ignores the method of hypothesis suggested in Part Six of the *Discourse* and in the correspondence of 1637–38 (Descartes, 1984–91, 3: 60–111, Passim), at one point dismissing the use of a hypothetical mode of presentation in the *Optics* and *Meteorology* as "experiments in exposition" (p. 46). Garber does not distinguish Descartes' hypothetical introduction of general principles of matter and motion in the *Meteorology* and *Optics* from his recognition that many of his particular mechanical explanations were at best one hypothesis among many, to be supported only by empiricist principles of hypothesis-choice, including comprehensiveness and simplicity of explanation. The hypothetical introduction of matter and motion, Descartes believed, would be secured with absolute certainty by his metaphysics; but the hypothetical status of his micro-mechanisms could not be discharged by metaphysics, and so these posits were subject to the vicissitudes of empirical research.[4]

In chapters six through nine, Garber presents a comprehensive and detailed treatment of the concepts of motion and force in Descartes' natural philosophy. He shows that Descartes' treatment of motion appeals to an attribute beyond those proper to extension *per se*, inasmuch as it requires the concept of duration in time, convincingly rebutting Koyré's (1978) attempt to atemporalize motion as conceived by Descartes. He uses Gabbey's (1980) analysis of Descartes' principles of "least modal mutation" (rechristened as "least modal change") to guide an extensive and subtle analysis of the evolution of Descartes' thought on the rules of impact between the Latin and French *Principles* (Chapter 8). And he (pp. 163–72) extends Gueroult's (1980) analysis of the relativity of motion, maintaining that although Descartes must indeed allow that in individual cases motion and rest will be assigned differently from various frames of reference, he can nonetheless distinguish genuine local motion from genuine rest, by appealing in the latter case to the absence of any relative transference of bodies in relation to one another. If there is relative transference, there must be genuine motion; if not, not.[5]

Garber's most original and philosophically interesting claims come in chapter four, on the refutation of hylomorphism. He contends that Descartes failed to present a metaphysical argument in support of his fundamental doctrine that matter consists only of extension and its modes, to the exclusion of "real qualities" and substantial forms (p. 110).[6] Garber finds something close to such an argument in *Principles*, II.23 and 64, where Descartes claims to have explained everything in the universe using only size, shape, and motion. As Garber suggests (p. 110), these passages do not specifically rule out real qualities. I think a direct metaphysical argument is offered in II.3–4 (recalling I.66–74), but that Garber misclassifies it as a version of what he calls the argument from elimination (pp. 78–80). The latter argument attempts to exclude color, sound, etc., from bodies on empirical grounds, moving from the empirical absence of color (and other real qualities) in some cases to the general elimination of color from matter itself. Garber correctly observes that it is weak: "from the fact that *some* bodies are not colored it does *not* follow that *no* bodies are colored" (p. 80). The crux of the matter lies in whether the argument of *Principles*, II.3–4, is properly classified as eliminative.

In the cited passage Descartes asked the reader to concentrate on perceiving the nature of matter using "the intellect alone", thereby perceiving that weight, hardness, and color do not pertain to it. Far from tendering an empirical argument, I think Descartes was appealing to what Kant later called the "real use" of the intellect, that is, that he sought to establish metaphysically that color is not a real property of body by directing the reader to achieve a clear and distinct intellectual perception of body as extended without being colored, and noting that such a perception of color as a property of body is sought in vain. Indeed, at I.68 Descartes contended that when color and pain "are judged to be real things existing outside our mind, there is no way of understanding what sort of things they are", and he went on to conclude that color, pain, and other sensations are to be "referred to the senses" (I.69; see also IV.198–9); he recalled this use of the intellect at II.3–4, concluding that "the true nature of body consists in extension alone" (II.5; 1983, p. 41).[7] This argument may seem thin because it appeals directly to intellectual intuition. It may also seem like an instance of confusing epistemology with ontology, that is, of moving from inconceivability to impossibility (or at least non-existence). But that sort of move – where "conceivability" relies on intellectual perception, not imagination – grounds Descartes' most important metaphysical arguments, including the exclusion of real qualities from extension as well as the argument for a mind-body distinction (1984–91, 2: 54). If we readily reject these arguments now, that is because we reject a fundamental methodological principle of Descartes': that substantive truths about the essences of things can be established by direct appeal to intellectual intuition. But the fact that we now have a substantive disagreement with Descartes about the real use of the intellect does not mean that he did not give an argument: it means that he gave an argument we now think is unsound.

The main purpose of Garber's book is avowedly historical: he sought to provide "a kind of handbook of Cartesian physics, a general introduction to the mechanical philosophy as Descartes or a sympathetic but not uncritical contemporary of his might have understood it" (p. 3). This aim is admirably contextualist. It sets the goal of understanding Descartes' philosophical and scientific achievements on their own terms and in their historical context, prior to forming a more general evaluation. In Descartes' case, as Garber emphasizes, the proper intellectual context for understanding his philosophy must include his attempt to replace the Aristotelian philosophy wholesale. Garber set out to examine the replacement physics or natural philosophy, together with its metaphysical foundations. The resultant "handbook" gives careful consideration to the concepts of body, motion, motion's laws, and the grounding of the latter in God's immutability.

Do body, motion, and motion's laws fairly represent the physics of Descartes? I think the answer must be "no", because Descartes accepted something similar to the Aristotelian conception of the subject-matter of physics. "Physics" as an Aristotelian discipline[8] included the study not only of the general principles of natural things (including form, matter, and causation, and the "common properties" of things, e.g., quantity, place, time, and the existence of a vacuum), but also of the various characteristics of all natural things, inanimate and animate. Among inanimate things it treated both the heavens and earth. The heavens were assigned their own incorruptible form, distinct from the forms of the four sublunary elements, and were treated non-quantitatively in physics (as opposed to the mixed-mathematical science of astronomy). Among inanimate earthly things, the four elements, their actions and passions, generation and corruption, and alteration were examined, along with "mixed bodies", including metals, stones, fossils, and "meteorological" phenomena. The discussion of animate earthly things included plants, animals, and human beings, right up to the rational soul. (Only rarely was the rational soul placed under metaphysics, rather than physics.) Aristotelian physics was a general science of nature, subsuming not only the basic principles and properties of natural things and the basic natural kinds, but also the subject-matters of what we might call physical astronomy, chemistry, biology, physiology, and psychology.

Descartes understood what he called "physics" to have similar boundaries. In describing his "physics" in the *Discourse* (Pt. 5), he ranged from the nature of matter and the laws of governing it, to the formation of the heavens, including the stars, comets, and planets, to falling bodies, the tides and oceanic currents, the formation of geologic features such as mountains, rivers, and metals, the growth of plants, the nature of fire, light, and color, the structure of animal and human bodies, and the joining of a rational soul to the human body. He included a similar range of subjects under the rubric of "physics" in the Author's Letter to the *Principles* (1983, p. xxiv), and covered all of these topics (and more), up to but not including plants, animals, and humans, in Parts II–IV, having been pressed by time to omit the latter topics. Descartes, then, regarded physics as a general science of nature, not as a discipline restricted to the analysis of body and the laws of motion, and his readers and followers understood the term in the same way.[9] This concept of physics differs dramatically from the one structuring Garber's discussion.

In fairness to Garber, it should be said that on one occasion he acknowledges that biology was included within physics in Descartes' scheme of things (p. 53), and that in the Afterword he mentions that the broader Cartesian program in physics was influential in the seventeenth century (p. 307). These statements, though, are lost amidst the persistent tendency to distinguish Descartes' "physics" from his "biology" (pp. 19, 20, 39, 51) and the implied equation of physics with the "theory of matter and motion" (p. 2). More generally, they do not make up for the lack of attention to the cosmological, geological, minerological, magnetological, and other sections of the *Principles*. If Garber wanted to produce a handbook of "physics" or of "the mechanical philosophy" as Descartes and his contemporaries understood these, he needed to cover the whole of Descartes' physics. If, as seems apparent at book's end, he intended to focus on a proper part of the Cartesian physics and its relation to the metaphysics, he should have informed the reader that he understood by "Cartesian physics" something quite different from Descartes and his century. It would seem that he chose his topics by retrospectively applying the notion of physics as it came to be understood in the late eighteenth century, when the theory of matter and the laws of motion were taken as the core of physics, and chemistry, geology, biology, physiology, and psychology came to be excluded.

This is not merely a small quibble over a failure of disclosure. Garber's tendency to focus on what seems "physical" in Descartes' natural philosophy from a post-Newtonian perspective masks much that is historically and philosophically interesting in Descartes. In particular, it avoids the question of the origin of the post-Newtonian conception of physics and Descartes' contribution to its genesis, it neglects the crucial question of the relation between the laws of motion and the actual body of Descartes' physics, and, partly as a result of the latter, it does not adequately characterize the justificatory relation between Descartes' metaphysics and his physics.

The origin of the discipline of physics is a philosophical question of interest in its own right. In 1640 there existed the "qualitative" Aristotelian physics described above, loosely allied with the "mixed mathematical" sciences, including optics and astronomy. By 1800, physics had changed into the science of the basic properties of matter and the laws of motion, together with the allied studies of hydrodynamics, optics, electricity, and magnetism. A crucial step in the development of this conception of physics was the publication of Newton's *Principia*, which unified celestial and terrestrial mechanics, thereby extending the "mixed-mathematical" style of science to a broad and unified subject-matter. The origin of Newton's vision of a unified physics based on laws wants explaining. Garber, by taking the laws of motion unproblematically to be "the physics" in Descartes, avoids this question of origin. The idea that there might be a few basic laws of motion and change that apply to all bodies, earthly and heavenly, was, so far as I can tell, new with Descartes. How did he come to formulate these laws? What role did they play in the physics he actually produced? Further, the idea of formulating a set of laws or rules of impact was also virtually new with Descartes. Garber tells us that he formulated them to achieve "completeness and system" (p. 252). But why should a hankering for completeness lead him to formulate rules of impact, rather than, say, explaining more fully why large bodies would be held within a vortex by the pressure of the second element?

Descartes' motivation for developing his laws of motion becomes even more curious once one realizes what a small role they play in his physics proper. They are nearly a free wheel in the system. At best, they are only tenuously connected with the real workhorse of Descartes' physics, the macro- and micro-mechanisms that he posited to explain the various phenomena of the world. A mechanistic program like Descartes', in which matter in motion is the basic explanatory principle in physics, must postulate regularity of action on the part of moving matter. Some regularity or lawfulness of motion is a requirement of Descartes' physical program. But unlike Newton's laws in his *Principia*, Descartes' laws hardly ever enter explicitly into the explanations that constitute the bulk of his physics in Pts. III and IV of the *Principles*, together with the *Optics*, *Meteorology*, and the *Treatise on Man*. The second law in the *Principles*, the law of rectilinear motion, is tacitly invoked at III.58 in the explanation of the outward pressure of a vortex, as Garber notes (p. 230). But Descartes introduced the forced circular motion of matter in a plenum, the driving principle of his vortical celestial system, without appealing to his laws of motion (II.33). And when examining the "physics" of vortices, he appealed not to the laws of motion, but to analogies with fluids going through large and small channels. Descartes does not explicate the circular motion in a plenum via the laws of motion, but by reflecting on the fact that circles are closed figures. Contrast this with Newton's inverse square law, which can be used directly to derive the elliptical orbits and the other planetary phenomena described in Kepler's three laws. By contrast with Newton, Descartes did not produce a mathematical physics; his physics is one of micro-mechanisms posited and investigated through analogy constrained by the metaphysics of extended matter (Galison, 1981; Hatfield, 1992).

Although Descartes claimed that he could "deduce" the more general features of the universe from his first principles, in the bulk of his physics he adopted a method of hypothesis. Unlike the usual hypothetico-deductive

method, Descartes' method of hypothesis includes *a priori* restrictions on the range of admissible hypotheses: hypothesized entities must be constructed from the explanatory elements of size, shape, position, and persisting motion, and must assume a plenum. He used this hypothetical method in framing explanations of earthly phenomena, including the observed properties of water, salt, oil, magnets, and metals, and in the rich domain of phenomena associated with animal and human physiology. He wrote of the stars that "since, by means of our senses, we perceive absolutely nothing in the fixed Stars except their light and apparent situation, we have no reason to attribute to them anything other than what we judge necessary to explain these two things" (*Principles*, III.68).[10] If he sometimes spoke of "deducing" his particular explanations from first principles (e.g., IV.187), he did not mean a rigorous deduction (Clarke, 1982, appendix one); rather, he claimed that he could construct, for any given natural phenomena, an explanation that appealed only to the basic properties accorded to matter in the first principles of his physics.

Taken under its own description, as a contextually placed handbook of Cartesian physics, Garber's book does not reach its mark. It is structured around a concept of physics that is not Descartes', it does not inquire into the origin of the concept of physics assumed in the book itself, and it does not adequately analyze the relation between the laws of motion and the body of Descartes' physics. These problems with Garber's analysis arise from instances in which he was not contextual enough. Closer attention to Descartes' own conception of physics and its similarities and differences from the Aristotelian conception might have brought the differences between Cartesian and Newtonian physics into stronger relief. In this way the range of justificatory relations between Descartes' metaphysics and physics would have been revealed: metaphysics does not provide certification for all of the physics; for much of it, it merely sets *a priori* constraints on hypothesis formation. In any case, if we do not take the book on its own terms, but see it as a survey of Descartes' attempts to ground and clarify the concepts of body, motion, and motion's laws through his metaphysics, a different picture emerges. There is a good deal of careful and insightful discussion of these topics, only some of which has been mentioned. Moreover, the book synthesizes many important passages drawn from Descartes' entire corpus. Its publication marks a genuine milestone in Cartesian scholarship. It will provide the starting point for additional work examining the place of natural philosophy in Descartes' philosophical enterprise, and the place of Descartes' philosophy in the larger development of early modern science and metaphysics.

2. FRIEDMAN: KANT'S PHILOSOPHY AND NEWTONIAN SCIENCE[11]

Friedman's remarkable book is distinctive among twentieth-century English-language Kant commentaries for giving pride of place to Kant's engagement with science. Aside from Buchdahl (1969), to whom Friedman is heavily indebted (p. xvi), major English-language commentators have assigned little or no importance to the content of Kant's Newtonian interests.[12] Friedman's central thesis is that Kant's philosophical achievement (in theoretical philosophy) "consists precisely in the depth and acuity of his insight into the state of the mathematical exact sciences as he found them" (p. xii). Friedman contends that this achievement retains value despite the fact that the Newtonian physics Kant analyzed has been replaced by a physics incompatible with Kant's philosophy, because Kant's work nonetheless provides an admirable exemplar of serious philosophical engagement with the sciences. Moreover, Friedman maintains that our present philosophical situation can best be understood by seeing how changes in science could undermine the Kantian philosophy (p. xii).

Friedman's Introduction portrays Kant's early attempts to bring "Leibnizean-Wolffian" metaphysics into harmony with Newtonian natural philosophy, while also sketching Kant's entire philosophical development. In his early works, Kant sought metaphysical foundations for Newtonian science in the concepts of *substance* and *force*, by altering the Leibnizean–Wolffian monadology – with its metaphysics of non-spatial, non-temporal, unextended, non-interacting simple substances – into a metaphysics accommodating real spatial and causal relations among substances. While seeking this accommodation, Friedman tells us, Kant also rethought the method of metaphysics. In particular, he saw that metaphysics should reject the (alleged) Wolffian method of grounding natural science on abstract general concepts and principles, and should instead take the findings of the exact sciences as "data" with which the basic propositions of metaphysics must be rendered consistent.

Part One of the book contains Friedman's central interpretive achievement, his fine-grained and insightful analysis of the central projects of the Aesthetic and Analytic of the first *Critique* in light of Kant's engagement with Euclid's geometry and Newton's mechanics. It begins with the doctrine of geometry, which readers of Kant earlier in this century considered an embarrassment, believing that Kant simply bet wrong on the structure of physical space. But Friedman maintains that, while lacking the logical resources to distinguish pure from applied geometry, Kant rightly perceived that the usual proof procedures of Euclid's geometry require constructive operations (with compass and straightedge) that themselves presuppose a spatial medium; in Kant's terms, Euclid's proofs require "construction in intuition" (pp. 56–66). But what precisely is Kant's conception of the function of intuition in geometry? On one view, Kant appeals to intuition in order to determine the structure of space by "reading off" its geometrical properties, thereby distinguishing Euclidean triangles (with angles summing to 180°) from logically possible but unintuited non-Euclidean triangles. Friedman argues instead that "construction in intuition" is Kant's reconstruction of argumentation or rational inference in geometry (pp. 80–95). In Friedman's view, Kant never doubted that the procedures could yield only Euclidean spatial structures (Euclid's geometry being *the* geometry). In chapter two, Friedman argues that pure intuition serves not to provide a model for geometrical and arithmetic objects, but to make possible the very thought of geometrical and arithmetic propositions; transcendental philosophy then shows the necessity of presupposing that empirical intuition will provide objects conforming to such propositions, thereby assuring the applicability of mathematics to the world (pp. 122–3; also, p. 46).

As striking as these reflections on Kantian mathematics are, Friedman's most original and penetrating analysis comes in his treatment of Kant's reconstruction of Newtonian mechanics in the *Metaphysical Foundations of Natural Science* and related texts (Part One, Chapters 3–4). Kant could not accept the existence (whether interpreted as appearance or as thing in itself) of Newtonian absolute space; nonetheless, he was committed to the notion of true as opposed to apparent motion. Through careful exposition of the *Metaphysical Foundations* (Chapter 3), Friedman shows how Kant treated the determination of true motions as an "idea" that can only be sought but not achieved. Kant used the determination of the operation of gravitational force within ever wider systems of bodies as the framework for determining true motion (with respect to the stationary center of mass of the system), or at least for determining what we would call a privileged inertial frame. In chapter four Friedman gives a careful reading of a passage from the *Prolegomena*, one that at first seems to suggest Kant believed he could derive the law of gravity from the geometrical structure of space alone, but which, on further consideration, supports the point that only by means of unification through the concepts of the understanding can laws of nature be cognized. He thereby extends Buchdahl's (1969, Chapter 8, Section 4) important discussion of the sense in which the understanding could be said to "prescribe" laws to nature, according to which the understanding, without being able to cognize particular laws independently of experience, nonetheless demands law-governedness in experience, and so requires the construction of empirically-derived but necessary and universal natural laws (pp. 177, 203).

The *Metaphysical Foundations* provides an *a priori* basis for cognition of general laws of nature. Such laws do not specify a determinate order in nature, nor do they require or imply that nature must be cognizable as an integral whole. In the first and third *Critiques*, such a cognitition of the whole was left as a regulative ideal, to be guided by teleological ideas. Beginning in the 1790s, however, Kant held that his failure to show the necessity for such overarching cognitive unity revealed a "gap" in his system, between metaphysics and physics: the metaphysical foundations of natural science failed to show that a physics of material nature, including the phenomena of heat, light, electricity, magnetism, and chemistry, was possible. The *Opus Postumum* writings on physics, which Friedman examines in detail in Part Two, contain Kant's attempt to fill this gap.

Kant's hope that he could fill this gap arose from the chemical revolution of the late 1780s and early 1790s. In the *Metaphysical Foundations* Kant despaired that chemistry could become a science, doubting that chemists would ever know the mathematical forms of the "action at a distance" forces he believed to govern chemical interactions (Ak 4: 468–71). He gained new hope from Lavoisier's transformation of chemistry into a science of chemical combination based on the theory of caloric. This hope gave birth to the so-called "aether deduction" (or "aether proof", in Kant's words), aimed at showing that the postulation of an all pervasive heat-aether was not a mere physical hypothesis, but a necessary condition for "the unity of the *whole* of possible experience" (Ak 22: 550). In this connection, Kant distinguished between the unity in experience of individuals brought under concepts and the collective unity of individuals cognized as parts of one, all-embracing totality, whole, or "All". Ultimately, Friedman plausibly argues, the aether deduction failed. The compelling reason Kant offered for demanding a unifying aether to permit collective cognitive unity was the necessity of a medium permitting observers to perceive far-away stars and planets, which might show the necessity of a light-aether. But Kant's *a priori* argument for the necessity of a genuine unity in the cognition of nature – a cognition that could be extended to heat and to chemical combination – required a deduction of a heat-aether, which Kant could not provide; the heat-aether remained a hypothesis rather than a necessary condition for experience. This episode shows Kant, to the end, seeking to use the physical sciences to grasp the transcendental structure of human cognition.

While admiring Friedman's book and the high achievement it represents, I would like to observe some limitations of its method and results. Friedman's book admirably relates Kant's theoretical philosophy to its intellectual context. He quite naturally uses Kant's own writings as a guide

to this context; e.g., by giving weight to the fact that Kant wrote extensively on foundational projects in physics throughout his life. But the use of intellectual context to achieve a deeper philosophical understanding of an author cannot remain primarily within the author's own texts, or even those of distant and "major" predecessors. Although it is important to know how Kant saw his own intellectual context, this knowledge will of necessity involve reconstruction from Kant's incomplete allusions or from interpretive conjectures about the targets of Kant's arguments. In order to read Kant's texts with simple comprehension, one must be able to identify Kant's intended contrasts with positions well-known to his audience (which may be now forgotten or known by caricature), to fill out allusions such as those to "recent metaphysicians", and, in short, to understand how his perceptions of the problems worth addressing and the solutions worth considering were shaped by the contemporary philosophical scene in Prussia and beyond.

Friedman adopts this method in his examination of Kant's relation to Newton, coordinating Kant's discussion with his own readings of Newton's works and those of important followers. In the case of Kant's philosophical relation to Christian Wolff, Friedman's reconstructive interpretations are less successful. He interprets the content and method of Kant's pre-critical writings in relation to the "Leibnizean-Wolffian" philosophy, and to the Wolff-Euler dispute over monadology (pp. 1–4). In particular, he ascribes to the "Leibnizean-Wolffian" monadological metaphysics the doctrine that simple substances are completely determined by their own inner, active force, and a denial of real relations or real causal interactions (pp. 1–10); he portrays Kant as departing from Wolff in adopting Newton's second law of motion (p. 5); he describes Kant as understanding Wolff to be "suspicious of the content of mathematics itself", merely adopting the form of mathematical argument (p. 15); and he sees Kant as ascribing to Wolff an *a priori* rationalist method in metaphysics, which Kant rejects in favor of an "analytic, regressive" method that treats empirical science as providing "data" to which metaphysical systems must conform (pp. 15–24).

Each of these assertions about Wolff misses the mark. As L. W. Beck and others have noted, the philosophies of Leibniz and Wolff differ too greatly to justify the summary use of the moniker "Leibnizean-Wolffian"; by contrast with Leibniz, Wolff affirmed real causal relations among (at least physical) simples, and he adopted an empirically-based methodology for all of philosophy, including metaphysics.[13] Although Wolff held that the world consists only in simple substances and combinations of such substances, he denied that the states of such substances can be explained through inner principles of activity alone, affirming the need to appeal to external causes to explain the states of simple substances and the composites they form.[14] Rejecting the term "monad" for his simple substances, he ascribed real relations of being "outside of" to them – relations that we perceive (confusedly) as extension and space, the latter (as with early Kant) being "phenomenal" but not "ideal" – and he did not attribute perception and appetite to his physical simples (contra Leibniz).[15] Although Wolff did not prominently discuss Newton's second law, he accepted both the first and third laws, and he adopted the parallelogram of forces, using the terminology of "impressed forces"; he rejected gravitational forces acting at a distance but affirmed impact forces.[16] Wolff was not "suspicious" of the contents of mathematics: his first post was teaching mathematics at Leipzig, and in 1706 he was appointed professor of mathematics and natural science at Halle with a recommendation from Leibniz (Beck, 1969, p. 257). Although not an original mathematician, he produced a full range of mathematics textbooks complete with sections on "applied" mathematics such as mechanics (1733–42; 1750–57), he surveyed the extensive literature in mathematics and mathematical science (1750), and he produced a detailed lexicon of terms from the mathematical sciences (1716).[17] Kant presumably did not regard Wolff as untrustworthy on mathematical content, for he used Wolff's textbooks for his courses on "pure mathematics" and on "mechanical sciences", announced in 1759 (Ak 2: 35).[18] And finally, Wolff affirmed that all philosophical cognition must begin from experience, citing as an example that in the analysis of nature one must begin from empirically-given regularities (1740a, §§4–12; 1737, §3). He did not claim that his "rational" reconstructions of disciplines were grounded independently of experience; rather, he conceived them as applications of reason to empirical representations in order to clarify concepts through analysis and to abstract (or extract) first principles. These concepts and principles could then be used synthetically to infer the structure of the world as it should be given in experience, allowing an empirical check on the "rational" disciplines.[19] In pronouncement, and at least sometimes in practice, Wolff was an adherent of the "analytic-synthetic" approach that Friedman believes was used by Kant in contrast to Wolff.

Friedman is correct that there is much to be learned by analyzing Kant's early Wolffian training and tendencies. Such results will require a fundamental engagement with Wolff's philosophy and its relation to Newtonian science. They will require a close study of the relation between his metaphysics and his writings on natural science, including an analysis of his concept of space and the distinctions among real, abstract, and imaginary space, an analysis of his conception of motive force as "phenomenal" and

of its metaphysical basis in simple substances possessed of "active force", and acknowledgement of his genuine differences from Leibniz.[20] They will not be achieved by filling out Kant's telegraphic allusions to "metaphysicians" with caricatures of Wolff's position. Moreover, they will require sensitivity to both Wolff's actual position and Kant's polemical (or ignorant) presentations of it, and to similar relations between Wolff and Leibniz and Kant and Leibniz. Only in this manner will we gain genuine understanding of the early development of Kant's theoretical philosophy and hence of its role in shaping his critical work.

Although the philosophical interpretation of past texts usually benefits more from examining predecessors than successors, there is nonetheless much to be gained by considering subsequent responses. Friedman interprets Kant's discussions of mathematics in light of Russell's dismissal of Kant for lacking the logical resources to make the distinction between pure and applied geometry (with the consequence that he held physical space necessarily to be Euclidean). Friedman agrees with Russell's assessment of Kant's "failings", maintaining that Kant was unable to make the pure/applied distinction because he had only monadic logic at his disposal (pp. 55–6), rather than the polyadic logic needed for adequate treatments of infinity and continuity in geometry (pp. 56–71) and the calculus (pp. 71–80). Taking an historicist tack, Friedman turns the assessment into a diagnostic tool for understanding Kant's relation to the mathematics of his day. In particular, he claims that given the inadequacy of monadic logic, Kant showed deep insight in turning away from the "Leibnizean-Wolffian" program of treating mathematical inference as resting wholly on the principle of contradiction (p. 98). He contends that Kant turned from concepts to intuitions in treating the infinitely divisible space because he saw no way to give an adequate representation of the infinite via (monadic) logical connections among general concepts as he conceived them (pp. 67–71). He shows that in geometry Kant recognized the central role played by constructive procedures (with compass and straightedge) in demonstrations, and that in arithmetic he recognized the importance of iterative operations (pp. 122–9).

Friedman's analysis of Kant stresses that as geometry was understood in the eighteenth century space is ineliminably its object, and that its demonstrations rely on spatial constructions. So far so good. But the philosophical and historical perspective from which Friedman carries out his analysis is narrow and limiting. Friedman says that Kant could not make the distinction between pure and applied geometry because he lacked our modern understanding of logic, "which did not exist before 1879, when Frege's *Begriffsschrift* appeared" (p. 56). In general, this claim is curious as a statement about the development of mathematical thought in relation to Frege; for although Frege made predicate logic into a field of study in its own right, mathematicians could and did *reason* polyadically prior to his work, by use of "for all" and "there exists" on suitably complex domains and relations thereon. Friedman articulates his claim about the need for Fregean logic through an analysis of the logical resources needed to handle continuity (a) in the Euclidean context of proving the existence of points of intersection between two circles and (b) in the case of continuous but nowhere differentiable curves. Allegedly, Kant needed to appeal to intuition in geometry and calculus because he could not logically "force" the existence of an infinity of points, lacking the resources provided by quantifier dependence in polyadic logic; he needed spatial intuition in order to "draw" continuous lines and to carry out geometrical demonstrations by constructing figures. Friedman's implication that non-intuitional means for handling continuity depended on Frege's work and that Kant could not make "our" pure/applied distinction because he antedated Frege are especially curious. The mathematical results that divorced geometry from intuition, distinguished pure geometries from the theory of physical space, and provided a successful mathematical treatment of continuity all were achieved and published prior to 1879 and independently of Frege.[21] In particular, the work of Grassmann (1844) and Riemann (1867) on *n*-dimensional manifolds formulated geometry (or the theory of continuous manifolds) independently of spatial intuition, and indicated the possibility of non-spatial interpretations of the metric "distances" in geometry (e.g., in terms of the "space" of color similarities); Bolzano produced important results on continuity (1817) and on the distinction between the formal structure of logic and its interpretation (1837; see Ewald, forthcoming, Chapter 6, introduction, and Wang, 1974, pp. 145–50); Dedekind (1872) produced his mathematical theory of continuity (as well as his treatment of numbers, 1888) independently of Frege; and in any case mathematical continuity on the real line was understood with the concept of function, as made rigorous by Weierstrass in the 1870s (see Bottanzini, 1986, Chapter 7), rather than through a new understanding of logic.

Friedman maintains that Kant's appeal to construction in intuition as an analysis of geometrical reasoning showed deep insight into the "logical and mathematical" practice of his day (p. 95). Kant's point that construction was essential to geometrical demonstration was a commonplace;[22] what Kant did was to interpret this practice by appeal to his distinctions between intuitions and concepts, so as to provide his particular explanation of the *a priori* status of geometrical demonstration in service of his arguments for transcendental idealism (see Webb 1987, esp. Section 2). By Friedman's

accounting, Kant's emphasis on spatial construction shows deep insight because of the mentioned inadequacies of monadic logic. However, in framing his analysis in terms of the *logical* resources available to Kant, Friedman fails to ask what *mathematical* resources might or might not have been available in the time of Kant, and to relate Kant's account of geometry and arithmetic to the mathematics of his day. While geometry had long been dominated by techniques of demonstration that relied on compass and straightedge, results by Descartes (1637) and Fermat (1679) had shown the way to describing curves by equations in "analytic" or algebraic geometry. Indeed, Newton (1769, p. 468) complained that "the Moderns ... have received into Geometry all Lines that can be expressed by Equations", a practice of which he disapproved, maintaining that geometrical objects are defined by the process of "description" or construction, not by their equations (though Newton himself contributed to the new algebraic techniques).[23] The course of the eighteenth century saw a conflict, or at least a contrast, between those who viewed the new algebraic techniques simply as aids to solving geometrical problems, and those who came to see the algebraic treatment as primary. Kant's admired contemporary, Euler, made important contributions to the algebraic treatment of higher order curves and to the development of the concept of function (1748), but did not algebraicize all of elementary geometry. The program of fully algebraicizing geometry – which meant replacing rule and compass constructions with the equations for the line and circle so that solutions to problems are "purely analytic and can even be understood without figures" (Lagrange, quoted in Boyer, 1956, p. 201) – was not realized until the work of Lagrange, Monge, and Lacroix (from the 1770s to ca. 1800), and the full arithmetization of the coordinates so that they were conceived as expressing numerical quantities rather than lengths of line segments – an achievement often now attributed to Descartes – apparently took place only in the first third of the nineteenth century (Boyer, 1956, Chapters 8–9).

I am not suggesting that Kant should be chastized for failing to embrace the algebraic understanding of geometry that was developing around him. At the same time, the Wolff–Kästner admiration of the "analytic" method in mathematics (and the "Leibnizean-Wolffian" program that allegedly was Kant's target) should not be dismissed without investigating their mathematical writings and their uses of algebra; for a prominent meaning of the term "analytic" in eighteenth century mathematics was "the method of solving problems, of demonstrating the theorems of geometry, by employing analysis or algebra" (D'Alembert, 1751b),[24] a method that Wolff (1733–42, 1750–57) fixed within the German mathematical textbook tradition, and that Kästner praised as less elegant but more powerful than the traditional synthetic (constructive) mode of demonstration.[25] Further, eighteenth century discussions of the relation between geometrical space and physical space should not simply be assimilated to Friedman's Russellian understanding of the pure/applied distinction. The conception that space is the object of geometry is not equivalent to an endorsement of intuitional techniques, as the development of algebraic geometry (conceived as applying to extended magnitude) attests.[26] In order to assess the relation of geometrical space to physical space in Kant's time, one would like to know more about how contemporary mathematicians conceived the object of geometry in relation to physical space, how they understood the relations among geometrical extension, magnitude, and number, and how they viewed the possibility of a purely algebraic geometry. A genuine appreciation of Kant's relation to the mathematics of his time cannot be achieved by the *a priori* use of results about the comparative strengths of monadic and polyadic logics to determine the state of mathematics a century earlier; engagement with the history of mathematics itself is needed.

More generally, an evaluation of Kant's discussion of mathematics in the *Critique* should not lose sight of the fact that this discussion served a critical aim: that of deciding whether metaphysics could hope to achieve the sort of synthetic *a priori* knowledge that Kant believed was exhibited in mathematics (B14–24). Kant presented his analysis of geometrical demonstration as a description of mathematical practice (which it was); he need not be faulted if that practice was changing even as he wrote, for in this instance his aim was not to reshape the practice but to describe it as he found it. He wanted to use this description to reshape metaphysics, and especially to support his doctrine of transcendental idealism.[27] In this instance, Kant was not well served by a pat reliance on the "best science" of the day. He might have been better served by a direct engagement with ongoing developments in mathematics – in the way, as Friedman shows, he directly engaged Newtonian developments in mechanics – rather than reliance on textbook wisdom. For our part, we can assess his relation to the mathematics and science of his time only through a properly historical investigation that engages that body of thought in detail. The history of science and mathematics, or rather application of the research methods of the history of science to past mathematical and philosophical writings, therefore is essential to a sound assessment of Kant's theoretical philosophy, and so to an assessment of our relation to it.

3. PHILOSOPHY AND HISTORICISM

Garber's and Friedman's books represent high achievements in the interpretation of the scientific content and context of the work of two major early modern philosophers. They provide compelling evidence that early modern philosophy was "culturally engaged", that is, involved in the central intellectual enterprises of its time (here, mathematics and physics). Applying the same spirit of investigation to the whole of metaphysics and epistemology in the early modern period would involve attending to biology (under the titles of natural history and physiology), psychology, and natural theology, as well. The resultant image of philosophy is refreshingly different from the portrayal of early modern philosophers – by Rorty (1979) and other would-be undertakers of the philosophical enterprise – as culturally imperialist, intellectually isolated, and skeptical (or anti-skeptical!) judges of all cognitive claims in the abstract. Garber's and Friedman's portrayals of the high level of intellectual and cultural engagement shown by previous philosophy also provide a model for philosophy today, a model of engagement with the intellectual pursuits of our own time. In this way, their attention to the relation between the history of science and philosophy can help us not only to understand philosophy's past, but also to form its future.

My criticisms of these excellent books have focused on the manner in which the authors carried out their contextualizing programs. In some areas they did not engage the context as fully as needed; their interpretations of particular arguments and indeed of whole philosophical programs suffered thereby. My qualms do not stem from an antiquarian delight in historical accuracy for its own sake. I believe I have shown that even to read the words on the pages of early modern texts, including words as familiar-seeming as "physics" and "analysis", one must be deeply steeped in the intellectual context. But reading the words on the page is a necessary condition for understanding a philosopher. Hence, deep engagement with intellectual history and the history of science is a necessary condition for reading the texts of theoretical philosophy. Here, the history of science as a discipline can guide us methodologically, for we can learn to emulate the best history of science by looking to predecessors first in establishing textual contexts, by relying on primary sources to establish the most important contexts, and by reading widely, as opposed to jumping from "great" to "great".

Our present philosophical orientation is set by the stories we tell about our past. Consequently, historical engagement can be a source of new philosophical inspiration. As Garber and Friedman remind us, philosophy traditionally has been engaged with the sciences. The history of the sciences, including mathematics, physics, biology, chemistry, and psychology, is needed to understand past philosophy, and therefore is of direct relevance for philosophy in general today. Moreover, given what Garber and Friedman have shown us about Descartes' and Kant's engagement with the science of their times, it should not be surprising that a currently promising direction in the area of metaphysics kown as "philosophy of science" lies in the philosophies of the special sciences, including the philosophies of physics, biology, and psychology. These areas provide examples of philosophical engagement with living bodies of thought similar to that found in the best theoretical philosophy from the early modern period. Contextually sensitive reflection on early modern philosophy can thus help us to acknowledge the centrally philosophical character of philosophy that engages the ongoing intellectual work of its own time, and that does so from the stance of an informed participant. No more than did Descartes or Kant, we today should not hope to do philosophy without knowing anything else.

NOTES

[1] In this section references to Garber (1992) are given parenthetically as page numbers in the text and notes; references to Descartes' major works are given by title and major division (Pt., etc.,), these works being available in translation in Descartes (1984–91). The section derives from a paper given at a meeting of the Pacific Division of the American Philosophical Association, March, 1993. I am grateful to Lanier Anderson for helpful remarks on this section and other parts of the paper.

[2] Kenny (1968) covered these topics and more, with only a dismissive summary of Descartes' scientific projects (Chapter 9). Beck (1965) was mute on the foundations of physics, and his (1952) related the *Rules* to science only through isolated examples of their "applications". Frankfurt (1970) hardly mentioned the program in physics. Of the three 1978 books, Wilson's nodded in the direction of the this program (pp. 2–3), then said barely a word more about it. Curley devoted a chapter to it (Chapter 8), gleaning from each Meditation points relevant to founding a physics, mainly the primary/secondary quality distinction and the immutability of God as a foundation for the laws of motion. Williams did better, devoting one chapter to physical objects (Chapter 8) and another to "Science and Experiment" (Chapter 9). Save Curley and Williams, these authors treated Descartes' interest in science as at best a source of examples, at worst an inconvenience and embarrassment.

[3] Serrus (1933) as discussed by Beck (1952, pp. 279–95); Clarke (1982, Section 22); Hatfield (1985, 1989). Beck himself (1952, pp. 295–307; 1965, II.13) argues that Descartes portrayed a general method of responding to *quaestio* in the *Rules* and used it in the *Meditations*.

[4] Descartes' acknowledged need to rely on experience in deciding among mechanistic hypotheses has been studied by many scholars, including Blake, Gewirth, Buchdahl, Denissoff, Laudan (cited in Clarke, 1982, p. 16, n. 15), Williams, Clarke, and myself; Garber cites only the latter two (Clarke, 1982, Hatfield, 1985), and only in other contexts. See also Hatfield (1989).

[5] The distinction between real relative motion and rest cannot solve, as Garber suggests (p. 241), a problem for Descartes' fourth rule of impact. According to that rule, a larger body at rest remains at rest when hit by a smaller body, no matter what the speed. As commentators have observed (e.g., Blackwell, 1966), if motion and rest can be differently assigned depending on the frame of reference, it is difficult to interpret the claim that the larger body is genuinely at rest (relative motion shows the absence of real relative rest, but does not permit further determination). Indeed, in Chapter 6 Garber effectively allows that any case of motion admits of proper descriptions assigning motions to either the body, or its reference frame, or both (p. 169). In further defense of the rule, Garber (pp. 247–8) cites Descartes' letter to Clerselier, which claims that by a (larger) body at rest he meant one embedded in surrounding matter to form one large, hard body. But this qualification contradicts the (counter-factual) condition Descartes said (*Principles*, II.45) that he assumed to hold for the seven rules of impact: "if only two bodies were to come in contact, and if they were perfectly solid, and separated from all others both solid and fluid in such a way that their movements would be neither impeded nor aided by any other surrounding bodies" (Descartes, 1983, p. 64), a condition he restated at II.53, noting that it cannot be met in a plenary world.

[6] Descartes wished to banish real qualities from his *World* (Chapters 5, 6), a desire he thrice repeated in the *Discourse* and *Essays* (pace Garber, who says Descartes was not very interested in the question prior to the 1640s, p. 97); he saw to the banishment at several places in the *Principles* (I.66–74, II.3–4, IV.197–9). The following discussion does not address Garber's "little souls" reading of real qualities (pp. 97–102), according to which Descartes first attributes to the schoolmen the doctrine that sensory qualities are like little souls attached to bodies, and then cannot rule out "soul-body" unions between sensory qualities and extended and matter. In my view this reading (a) does not give adequate weight to Descartes' "resemblance" interpretation of scholastic sensory qualities, and (b) does not show that Descartes could not appeal to intellectual intuition to banish the little souls.

[7] Descartes gave a similar argument in the second paragraph of the Fifth Meditation, and referred back to it at the end of the Fifth and beginning of the Sixth Meditations; however, for the reason he later revealed to Mersenne (CSM 3: 173) – namely, that he wanted to destroy the principles of Aristotle without his readers at first noticing it – he did not call attention to his attempted replacement of Aristotelian matter theory.

[8] The following description relies on Eustace of St. Paul (1638), Part 3, and Tittelmans (1596); it applies to many other textbooks.

[9] The major textbooks of Cartesian philosophy (Le Grand, 1678; Regis, 1691; and Rohault, 1676) embodied this broad conception of physics, so as to include everything up to physiology and sensory psychology, and, in some cases, the immaterial mind.

[10] In the *World* Descartes implied he could explain the formation of the three differently sized elements from an assumed primeval chaotic soup of matter (Chapter 8), but in the *Principles* he has given this up, adopting the (hypothesis-forming) strategy of attributing to the initial motions conferred on matter by God such properties as would lead to the configurations that "experience" teaches us matter has (III.46).

[11] Citations to Friedman (1992) in the text and notes of this section will be given by page numbers in parentheses, as will citations to Kant's *Critique of Pure Reason*, using the usual "A" and "B" to denote the first (1781) and second (1787) editions, and to Kant's other writings as in Kant (1902–), cited as "Ak" followed by volume and page numbers. I am grateful to Scott Weinstein, Zoltan Domotor, and Bill Ewald for conversations on the history of logic and mathematics, and to Jamie Tappenden for helpful remarks on Sections

2 and 3 of the penultimate version.

[12] E.g.: Bennett (1966), "if we are looking for the core of philosophical interest in Kant, then his Newtonian preoccupations are peripheral" (p. 200); Strawson (1966), sees Kant's engagement with the science of his time as an "obstacle" to sympathetic understanding (p. 23).

[13] Beck (1969), pp. 257, 262, 270–271; and on real causal relations, Burns (1966), pp. 98–99.

[14] Wolff: "Mutationes elementorum omnium inter se & omnes in eodem elemento connexae sunt" (1737, §210; also, §§176–7, 197, 204); "Vis continuo tendit ad mutationem status subjecti sive externi, sive interni" (1736, §725 also §§882, 887). Physical interactions occur through impact: "Corpus non agit in alterum, nisi dum in ipsum impingit" (1737, §320); he did not pretend to explain metaphysically how simple elements interact (§294, note).

[15] Wolff explicitly rejected the term "Monades" (1737, §182, note); he affirmed the phenomenal status of extension but denied idealism (1737, §§221–6; also 1736, §§589–91); he explicitly affirmed external relations: "Si *status rei* constituitur mutabilibus intrinsecis, nempe modis, *internus* dicitur; se vero extrinsecis, quales sunt relationes rei ad alias, *externus*" (1736, §706; also §710); and contrasted the simple substances that compose body with those that are minds (1740b, §644). More generally, it is not clear that the eighteenth century was well acquainted with Leibniz's denial of real external relations and doctrine that space is merely a "well-founded phenomenon". Friedman (p. 8, n. 10) refers to Leibniz and Clarke (1956, 3.4) for Leibniz's doctrine that space is merely ideal; this work (1720) was available to Kant; Wolff (1736, §589, note) cited a related passage from it in his discussion of space. There is trouble for Friedman's exposition because a) Wolff did not conclude from the passage that space is ideal, and 2) a good case can be made that in the correspondence with Clarke, Leibniz (1956, 5.47) treated space as being ideal only in that the idea of homogeneous space abstracts from particular real relations among co-existing things (which he affirmed in the correspondence, by contrast with the position in the *Discourse on Metaphysics* and *Monadology* that there are no real relations among monads).

[16] Wolff (1737), §§309, 315, 320–3; (1733–42), vol. 2, "Elementa mechanicae et staticae", Chapter 5, "De motu rectilineo composito".

[17] In addition, Wolff produced extensive textbooks in "Experimental-Physik". All are in the *Gesammelte Werke* from Olms.

[18] Warda (1922, p. 40) reports that Kant had in his library copies of Wolff's *Elementa matheseos universae*, 2 vols. (Halle, 1713–15), *Auszug aus den Anfangs-Gründen aller mathematischen Wissenschaften*, new ed. (Frankfurt and Leipzig, 1749), and *Anfangs-Gründe aller mathematischen Wissenschaften*, new ed., 4 vols. (Frankfurt, Leipzig, Halle, 1750); the first and last works contained Wolff's exposition of analysis and of algebra, with applications to geometry.

[19] On the relation between rational and empirical psychology, see Richards (1980), and Hatfield (1990), pp. 72–4.

[20] Wolff (1736), §§588–618, on space; (1737), §§135, 137, 295–300, on force. Some help in interpreting these passages and others is available in Burns (1966), Chapters 2–3, and Polonoff (1973), Part 1, Sections 4–6, 10, Part 2, Sections 1–5. Wolff not only treated the system of pre-established harmony solely as an explanation of mind-body interaction (as opposed to a basis for apparent body-body interactions), but he also regarded it as only "probable" and "preferable" to the systems of physical influx and occasionalism, (1740b), §§6 [38]-9.

[21] At the same time, it has been claimed that the precise result that Friedman attributes to Frege – the formulation of "our" distinction between a formal system and its interpretation, or between a "pure" formal system and its "application" via a real-world model – was only achieved half a century later by Gödel (1930); see Goldfarb (1979), and Wang (1974), pp. 145–52.

[22] D'Alembert, 1754, Newton, 1729, vii–viii; also, Lamy, 1704, pp. xv–xvi; Lamy, 1758, pp. 2, 6–7.

[23] Newton's remarks came in a discussion of whether geometry proper should be limited to constructions with line and circle alone, or should admit other curves, including "mechanical" ones (1769, appendix, pp. 467–504). Newton was defending his use of the conchoid, which he held should be admitted into plane geometry before or with the conic sections (pp. 492–5). He argued that the relative simplicity of the equations of curves is irrelevant to their use in geometry, where simplicity of construction is determining (pp. 467–70, 495–6); he offered as a *reductio* the argument that if equations are followed, the circle will be classed with the conics, whereas in his (classically-guided) estimation it belongs with the line (pp. 468–9, 493).

[24] D'Alembert defined "analysis" in mathematics as "the method of solving mathematical problems by reducing them to equations" (1751a); he reported that in this context the terms "analysis, algebra, are often regarded as synonyms". He maintained that algebraic techniques should not be applied to elementary geometry, because such techniques are not needed to "facilitate the demonstrations", though he excepted the solution of problems of second degree with line and circle, which are included in elementary geometry but for which algebraic techniques do help (1752, p. 637a).

[25] Cantor (1880–1908, 4: 453–5) quotes portions of Kästner's remarks on analysis and synthesis from the latter's preface to Hube (1759). Kästner (1790a, c) intervened in the Kant-Eberhard dispute to support the Wolffian conception that mathematical cognition is via concepts (e.g., of space), though he criticized Wolff for begging certain questions in his definition of parallel lines (1790b); he was Gauss' teacher and an inspiration to Bolzano. Wolff (1733–42, 1: 295) placed "analysis" (of which he held algebra to be one part, calculus the other) at the pinnacle of human learning, and devoted a significant portion of his algebra ("Elementa analyseos mathematicae tam finitorum quam infinitorum", Part 1, 1733–42, vol. 1) to the solution of geometric problems. Boyer (1956, pp. 155–6) cites Wolff for providing an important and early German textbook treatment of "plane analytic geometry, in the sense both of Descartes [geometrical emphasis] and Fermat [algebraic emphasis]"; Kästner (1800, preface to the first edition of 1758) confirms the importance of Wolff's mathematical textbook in Germany. These facts lead me to disagree with Beck's statements that "Wolff's only contribution to mathematics was lexicographical", and that he "considered mathematical knowledge to be very different from what Descartes and Tschirnhaus thought it to be" (1969, p. 262).

[26] Kant held that algebra, too, requires appeal to constructions in intuition, but he distinguished such constructions as "symbolic" by comparison with the "ostensive" spatial constructions of geometry; see Brittan (1992), p. 320.

[27] Kant's most extensive analysis of mathematics, in the Doctrine of Method (A712/B741–A738/B766), was intended to show that philosophy cannot expect to achieve demonstrative proofs with the rigor of mathematics, by showing that philosophy is limited to "discursive" as opposed to "constructive" procedures; the latter supported his claim in the Aesthetic that geometrical judgments cannot rest on the analysis of concepts but require intuition (B39–41), which in turn supported transcendental idealism (A26/B42-A30/B45).

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ANTI-REALISM AND SPEAKER KNOWLEDGE

ABSTRACT. Dummettian anti-realism repudiates the realist's notion of 'verification-transcendent' truth. Perhaps the most crucial element in the Dummettian attack on realist truth is the critique of so-called "realist semantics", which assigns verification-transcendent truth-conditions as the meanings of (some) sentences. The Dummettian critique charges that realist semantics cannot serve as an adequate theory of meaning for a natural language, and that, consequently, the realist conception of truth must be rejected as well. In arguing for this, Dummett and his followers have appealed to a certain conception of linguistic knowledge. This paper examines closely the appeal to speakers' knowledge of linguistic meaning, its force and limitations.

1. INTRODUCTION

Dummettian anti-realism is first and foremost a negative position. It is a repudiation of the realist's notion of verification-transcendent truth, truth that lies beyond all possibility of ascertaining.[1] A crucial – perhaps the most crucial – element in the Dummettian attack on realist truth is the critique of so-called "realist semantics", which purports to specify the meanings of (at least some) sentences in terms of verification-transcendent truth-conditions. The Dummettian critique charges that realist semantics cannot serve as an adequate theory of meaning for a natural language, and that, consequently, the realist conception of truth must be rejected as well. In arguing for this, Dummett and his followers have appealed to a certain conception of linguistic knowledge. I believe the appeal is questionable, but deserving of close examination. The appeal to speakers' knowledge of linguistic meaning, its force and limitations, are the subject of this paper. However, I begin with a bit of selective recent history of ideas going back to logical positivism. This, by way of background to the Dummettian move from a rejection of realist semantics to a rejection of realist metaphysics.

The following quotation is from Moritz Schlick:

> Here then we find a definite contrast between the philosophic method, which has for its object the discovery of *meaning*, and the methods of the sciences, which have for their object the discovery of *truth* ... I believe Science should be defined as the "*pursuit of truth*", and Philosophy as the "*pursuit of meaning*". (cf. Rorty, 1967, p. 48)

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