

# Problems with the Deductivist Image of Scientific Reasoning

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There seem to be some very good reasons for a philosopher of science to be a deductivist about scientific reasoning. Deductivism is apparently connected with a demand for clarity and definiteness in the reconstruction of scientists' reasonings. And some philosophers even think that deductivism is the way round the problem of induction. But the deductivist image is challenged by cases of actual scientific reasoning, in which hard-to-state and thus discursively ill-defined elements of thought nonetheless significantly condition what practitioners accept as cogent argument. And arguably, these problem cases abound. For example, even geometry—for most of its history—was such a problem case, despite its exactness and rigor. It took a tremendous effort on the part of Hilbert and others, to make geometry fit the deductivist image. Looking to the empirical sciences, the problems seem worse. Even the most exact and rigorous of empirical sciences—mechanics—is still the kind of problem case which geometry once was. In order for the deductivist image to fit mechanics, Hilbert's sixth problem (for mechanics) would need to be solved. This is a difficult, and perhaps ultimately impossible task, in which the success so far achieved is very limited. I shall explore some consequences of this for realism as well as for deductivism. Through discussing links between non-monotonicity, skills, meaning, globality in cognition, models, scientific understanding, and the ideal of rational unification, I argue that deductivists can defend their image of scientific reasoning only by trivialising it, and that for the adequate illumination of science, insights from anti-deductivism are needed as much as those which come from deductivism.

**§1. Introduction.** Only in a logically valid argument is the conclusion fully warranted by the premisses. But the conclusions which scientists reach are often not fully warranted by the premisses they officially state.

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<sup>1</sup>I am grateful for earlier discussions with David Gunn, Alan Musgrave, and Robert Stoothoff, and for extensive critical comments from Robert Stoothoff on an earlier draft. On the present draft I have also received some excellent suggestions by Otavio Bueno and Steven French, in light of which I intend in some ways to revise it.

Alan Musgrave, whose deductivist image of scientific reasoning I will consider in this critical discussion, suggests that, when this occurs, the scientists' argument can be treated either as *ampliative*, i.e. non-deductive, or as an *enthymeme*, i.e. as an argument within which additional premisses sufficient to secure logical validity have been left merely tacit.

Musgrave's deductivism resides in his preferring the latter approach. On the former approach, as Musgrave characterises it, scientists' inferences are viewed as following some non-logical inference rule. Musgrave argues that this approach threatens a slide to logical relativism, psychologism, and obscurantism. The deductivist approach, on the other hand, he says is firmly objectivist and non-psychologistic, because it makes (he says) a sharp distinction between matters of logic and matters of fact and accords with a demand for logical perspicuity. The demand to construe scientific argumentation as logically valid deduction is, he contends, reasonable, for it is just the demand to lay out all the assumptions, tacit and otherwise, without which a given conclusion could not have been validly reached.<sup>1</sup>

My worry concerning the deductivist image is that actual sciences lack the perfection of logical organisation required for it to ring true. This is not a merely psychologistic point concerning the practising scientists, who will of course often fail to reason logically. Rather it concerns a reason why valid logical deduction is often out of the question in the sciences. To argue this point, I shall consider a science where support for it seems least likely—namely, the most exact and rigorous of empirical sciences, mechanics. My discussion of mechanics (§4, below) is facilitated by an interchange in *Philosophy of Science* between Musgrave

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(“Discussion: Realism about what?”, 1992, pp. 691-697) and Roger Jones (“Realism about what?”, 1991, pp. 185-202), and by some recent clarificatory work concerning the physics in these two articles by David Gunn (unpublished manuscript).

The points I make concerning mechanics emerge from consideration of a still more exact and rigorous, though less clearly empirical, science that is closely allied to mechanics, the science of geometry. Even so exact and rigorous a science as geometry lacked, through most of its history, the perfection of logical organisation required for the deductivist image to ring true of it. The historical facts concerning geometry are well known, and they directly imply a problem for the deductivist image of scientific inference. Yet, oddly enough, no-one, so far as I know, has used these facts to evaluate the deductivist image.

## **§2. Geometry before Hilbert a Problem Case for the Deductivist**

**Image.** We have a problem case for the deductivist image if, in a scientific discipline, hard-to-state and thus discursively ill-defined thought-elements nonetheless significantly condition what is accepted as cogent argumentation. That there are such elements in all empirical sciences, including even the most exact and rigorous of empirical sciences, mechanics, I shall argue later. The point will be made to seem less surprising, if we first recall that there are hard-to-state and thus discursively ill-defined thought-elements even in geometry, at least as Euclid formulated it. Not even geometry has long had the perfection of

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<sup>1</sup> Musgrave expresses these views among other places in his (1981) (see especially pp. 83-84); (1988) (see especially pp. 237-239); and (1989).

fully discursive expression and logical organisation required for the deductivist image to ring true of it.

That is to say, across all but the last ninety-eight years of Euclidean geometry's two-and-a-half-thousand year history, geometry was not really a logical system. The proof procedures Euclid and all his successors up to Hilbert regarded as rationally cogent, were not the proof procedures of any explicit or even possible discursive deductive logic. Rather, proofs depended crucially on diagrams. The diagrams introduced non-discursive elements, aptly called constructions in intuition, which are essential to the rational cogency of the proofs which Euclid offered us.

It is a common misconception at least of the present day that the system of geometry given us by Euclid is a logical system. I think this misconception has arisen because of advances in logic at the end of the last century. For a while these advances made newly tenable the conception that reason is logic, a conception that had not been ascendant since the heyday of Aristotelianism. Because Euclid's geometry is undoubtedly a rational system, people will mistakenly think it is a logical system if they also think that reason is logic. I am unsure how much confusion there was on this point prior to recent times. I suspect not much. Although the rationalists, it is true, treated Euclid's system as their very ideal for knowledge, in general they did not identify reason with logic. It was the Aristotelians, philosophically more empiricist than the rationalists, who typically equated reason and logic. Aristotelians, unlike rationalists, laid no great store by mathematical knowledge. It was in a perfected system of, say, qualitative zoological knowledge rather than geometrical knowledge that Aristotelians expected to find the logical

systematicity which was their ideal for knowledge. NeoPlatonists, who by contrast to Aristotelians worshipped mathematics, and who drew from Euclid inspiration for their radical rationalism, tended to equate reason not with logic but rather with mathematical insight and argumentation. In the fifteenth, sixteenth and seventeenth centuries, neoPlatonism helped urge the progenitors of modern science to replace discursive logical modes of reasoning and analysis by intuitive mathematical modes of reasoning and analysis, and thereby replace the qualitative worldpicture of Aristotle by a mathematised worldpicture. Both Galileo and Descartes expressly held that reason is more than logic, and they had mathematics, and in particular geometry, in mind, as the paradigm of rational thought. In his early years Leibniz had attempted to reduce mathematics to logic, just as he had, following Descartes, attempted to reduce physics to mathematics; but he came to view both these attempts as unsuccessful, and to regard mathematics as irreducible to logic from any vantage point but that of the divine mind. Leibniz attempted in his monadology a reconciliation of Aristotelianism and neoPlatonism, and held in effect that reason is the same as logic only for the monad which is highest of all on the scale of apperceptiveness. For all other monads—e.g. for you and me—the rational cogency of geometrical argumentation is not the same as logical cogency. Kant held that reason is not the same as logic, and his philosophy of mathematics was one foundation for this view. Thus it seems that in earlier times thinkers were not apt mistakenly to view Euclid's geometry as a logical system.

Why does Euclid's geometry resist formulation as a logical system? Most importantly, because the cogency of Euclidean argumentation is conditioned by the notion of continuity, which resists formal or conceptual

definition. The question, “What is a continuum?” in fact is very difficult to answer formally or conceptually. Whatever could the relation of composition be, which holds between a collection of *unextended* points and an *extended*, continuous line? Genuine and appropriate puzzlement concerning this question goes back to the Greeks. There is a thickness to the continuum which Zeno helped show is important but which is difficult to lay hold of formally. To reckon to ourselves conceptually the nature of this thickness requires us either (1) to abandon the conception that a continuous extended line is composed of unextended points, or (2) to countenance an infinity higher than that of the natural numbers.

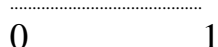
Consider some dense but denumerably infinite collection of points between 0 and 1—for example, the collection comprising 0 and 1 plus their midpoint, the midpoints of the subintervals thus defined, the midpoints of those subintervals and so on. Call this collection  $\mathcal{M}_{[0,1]}$ . Although dense, this collection of points has gaps—a remarkable fact. Thus the point  $1/3$  lies on the interval from 0 to 1 but is not a member of  $\mathcal{M}_{[0,1]}$ .  $\mathcal{M}_{[0,1]}$  clearly comprises only rational points, but equally clearly comprises not all rational points. The larger collection  $\mathcal{Q}_{[0,1]}$  of all the rational points along the interval from 0 to 1, is, as Cantor showed, itself still denumerable. And  $\mathcal{Q}_{[0,1]}$ , too, has gaps, failing to contain, for example,  $1/\pi$ .

A still more remarkable fact than that dense collections *can* have gaps, is that *any* denumerable collection *necessarily* has gaps. Thus, one cannot take a continuum to be comprised of unextended points without countenancing an infinity which is somehow higher than that of the natural

numbers. To prove this, one does not need a fully formed theory of the real numbers coupled with the well-known diagonalisation argument of Cantor's. There is a shorter proof: namely, one based on the fact that a denumerable collection of points has measure zero on the continuum. Suppose that (in Figure 1) I have a section of the continuum, and as well (in Figure 2) a dense but denumerable collection of points between 0 and 1 (say, all the rational points). And suppose I want to cut



**Figure 1**



**Figure 2**

off a separate finite bit of the continuum in Figure 1 to cover each point in Figure 2. That is, each point among the infinite collection of points in Figure 2 is to have its own “hat” (let us call it)—a unique finite interval cut from the continuum in Figure 1. To supply a hat for each and every point in Figure 2, what total length from the continuum in Figure 1 is it necessary to use? Since in Figure 2 there are infinitely many points to cover, each with a finite hat (which will cover in addition infinitely many points to either side, points which however are themselves to have their own unique finite hat assigned to them as well), it may seem that an infinite total length will be needed. Or, since the points in Figure 2 run densely from 0 to 1, and each is to be covered by its own finite hat, it may seem that at any rate a total length for hats will be needed which is at least somewhat greater than one. The fact is, however, that the total length I need to use for hats is less than any assignable length! For, for any  $\epsilon > 0$ , no matter how small, I may, for arbitrary  $n$ , cut from the continuum in

Figure 1 a hat of length  $\varepsilon/2^{n+1}$ , to cover therewith the  $n^{\text{th}}$  member of the enumerable collection of points in Figure 2. If I do this for all  $n$  then the total length used from the continuum in Figure 1 is  $\sum_{\text{all } n} \varepsilon/2^{n+1} = \varepsilon/2 < \varepsilon$ . Thus the mere denumerability of the collection of points in figure 2 implies that its thickness is as nothing compared to that of the continuum. Thus the continuum itself clearly has more than a denumerable infinity of points.

Until relatively recent times no-one possessed the concept of an infinity higher than that of the natural numbers. How then did Euclid express the relation of composition which he held to exist between an extended, continuous line, and unextended points? The answer is that he did not. It was beyond Euclid to define this relation discursively. Fortunately for Euclid, however, a more-or-less definite notion of continuity arises in us intuitively. Following Kant, it can be argued that a basic condition of the possibility of our cognizing at all is that, as active subjects of cognition, we possess an intuitive notion of continuity. Thus I know intuitively (from my synthetic apprehension of my own agency, which requires that I endure through time) what it is for an extension, viz., my own temporal extension, to be gapless. Perhaps this rudimentary intuition of continuity is all I need if also I am to know intuitively what it would be for a point to move continuously in a space, describing a gapless line.

Euclid starts Book I of the *Elements* by giving definitions for the geometrical notions of point, line, surface, straight, plane, circle, etc. which he will then employ in stating his postulates or axioms and in articulating his preliminary propositions and proofs. Euclid then states five



postulates and, in addition, five very general principles which he calls “common notions” (see Euclid 1956, pp. 153-155). Naturally, in order to give the definitions of point, line, surface, straight, plane, circle, etc., Euclid must make use of some further notions which he leaves merely intuitive and undefined—notions such as length, breadth, extremity, evenly, and inclination. The intuitions which Euclid mobilises in fact call *into his reasoning* a number of ideas—among them continuity, metric and conformal definiteness, flatness—which for all the centuries until just before Hilbert’s day would remain ill-defined because ever-so-difficult to formalise, that is, ever-so-difficult to capture discursively. Hilbert worked of course within a context of thought much changed by the development of non-Euclidean geometries and of more abstract approaches than Euclid’s to geometry, and still more significantly, by key nineteenth-century work to rigourise analysis and thus to render discursively clearly, that is, formally rather than intuitively, the idea of continuity.

As is well known,<sup>2</sup> the notion of continuity had long resisted formalisation because it has a logical richness far outstripping the expressive power of Aristotelian logic. From Kant’s point of view the project would have seemed an impossible one—to conceptualise something ineluctably intuitive. In hindsight we can see that the project was possible after all, but it was at the same time certainly not easy. To approach the notion of continuity purely formally, i.e. with reasoning that is altogether discursive, we have learned to use such notions as that of the *convergence of a sequence* and the *convergence of a sequence to a limit*. A sequence of numbers  $s_1, s_2, \dots$  converges if for every  $\varepsilon > 0$  there is a

natural number  $N$  such that for all natural numbers  $m, n$  which are greater than  $N$ ,  $|s_m - s_n| < \epsilon$ . A sequence of numbers  $s_1, s_2, \dots$  converges to a limit  $r$  if for every  $\epsilon > 0$  there is a natural number  $N$  such that for all natural numbers  $m$  greater than  $N$ ,  $|s_m - r| < \epsilon$ . One can use the notion of *Cauchy completeness*—whenever a sequence converges, it converges to a limit  $r$ —formally to introduce the notion of continuity. But the expression of these ideas lies way beyond the power of Aristotle’s logic. For their expression requires a logic of quantifiers ( $\forall, \exists$ ) sufficiently strong to allow *nested quantification*. (The Cauchy completeness condition has logical form  $\forall \epsilon \forall \delta \exists N \rightarrow \exists N \forall \delta \forall n$ , so it involves *lots* of nesting of quantifiers.) The great step beyond Aristotle’s logic to a proper logic of quantifiers was given us by Gottlob Frege. Frege effectively codified logical conceptions and methods already pretty fully developed in an informal way by mathematicians seeking surer rational foundations for the mathematics of the continuum.

Without the emergence of a logic of quantifiers—that is, with only Aristotelian logic—no axiomatisation of Euclidean geometry is possible such that the connection of theorems to axioms is altogether discursive-deductive-logical. Thus from the definitions, postulates and common notions of Book I of the *Elements*, one moves by logic not even to the very first proposition that is proved. Euclid’s very first inference is not logical inference. This is amply discussed by the authors whose writings I have been following in my last two paragraphs.<sup>3</sup> Proposition One in Book I of

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<sup>2</sup>My discussion in this and the next paragraph follows that in Michael Friedman’s (1992), ch. 1. Friedman himself draws on discussions by Eves, Hintikka, Parsons, Heath, and Pasch.

<sup>3</sup>See note 3. Another good discussion is in Clark Glymour’s (1992.) See Ch. 2.

the *Elements* states that on a given line segment, an equilateral triangle can be constructed. Diagrammatically, the proof involves describing circles about the endpoints each with radius equal to the length of the line segment, and taking the point of intersection as vertex for constructing a triangle. Then various considerations are used to prove that this triangle is equilateral. As logical demonstration, Euclid's argument is not cogent. With ingenuity we could assign to the terms which Euclid leaves undefined, rather than the standard intuitive meanings, sufficiently non-standard intuitive meanings (*is red* for “has breadth”, say, and likewise for other undefined terms) that the definitions all come out sensible and postulates and common notions all come out (in some model) true, yet Proposition 1 comes out (in that model) false. Thus Euclid's argument to Proposition 1 is not *logically* valid. Another, particularly pretty way to see the logical invalidity of the argument to Proposition 1, is to consider the rational plane— $\mathbb{Q}^2$ —and understand Euclid's definitions as introducing objects in this plane. This thwarts Euclid's tacit intuitive requirements of continuity, and introduces a model in which Euclid's definitions make sense and postulates and common notions hold true, but which falsifies Euclid's Proposition 1.<sup>4</sup>

The advent of a discursive reckoning of the concept of continuity, together with the development of a powerful logic of quantifiers, quite changed matters. With quantifier logic, an axiomatisation utterly different from Euclid's of Euclidean geometry becomes possible, such that the connection of theorems to axioms is altogether discursive-deductive-logical. This was not discovered until 1899, in which year Hilbert

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<sup>4</sup>See Friedman, *op. cit.*

published his *Foundations of Geometry*. Without the prior development in the practice of mathematicians of the logic which Frege was to codify, Hilbert could not possibly have given his new axiomatisation of geometry.

Frege's own work in the foundations of mathematics was logicistic. Logicisation (in Frege's case, of arithmetic) is work towards identifying what is reasonable with what is logical. Note that a general conception that reason = logic just *is* deductivism. Ironically, Frege was no deductivist, for, over against Hilbert, he shared with Kant the view that geometry is an intuitive science. Nonetheless Frege's logicising work in the foundations of mathematics for a while revitalised in philosophy the conception that reason = logic. Before Hilbert and Frege it had seemed that mathematics provides powerful reasons to deny this identity, i.e. powerful reasons not to be a deductivist. After their work it began to seem to many philosophers that the identity holds. The new confidence in deductivism was arguably misplaced even within the narrow purview of mathematics. It turns out that, even within the narrow purview of mathematics, Frege's logic is itself in important ways limited. For example, it is impossible within the context of Frege's logic to axiomatise the theory of arithmetic completely. This result, due to Gödel, arguably returns us, despite all Frege's work, even within the narrow purview of mathematics, to the conception that reason  $\neq$  logic.

In fact, however, Gödel's proof is but one ground in mathematics for the anti-deductivist conviction that reason  $\neq$  logic. The example of Euclidean geometry itself supports this conviction, in quite another way.

### **§3. A Latent Non-monotonicity in Intuitive Geometric Reasoning.**

According to deductivism, “non-monotonic reasoning” is a contradiction in terms. What ‘monotonicity’ means is that if, given the premisses of an argument, it is reasonable to infer that the conclusion is true, then the addition of more premisses cannot make it unreasonable to infer that the conclusion is true. And so long as we take reason to be deductive-logical in character, the monotonicity of reasonable inference is guaranteed. Adding premisses to an already reasonable (i.e. logically valid) argument cannot make the argument logically invalid (i.e. unreasonable). For an argument is logically valid if there is no way that the premisses can all be true and yet the conclusion be false. And if there is no way an initial set of premisses can all be true and yet the conclusion be false, then there is no way that an extended set of premises can all be true and yet the conclusion be false.

The example of geometry directly challenges, if I am not mistaken, the deductivist’s refusal to countenance non-monotonic reasoning. For there was a latent non-monotonicity in Euclid’s reasonings which came to light only in the last two centuries or so and was fully disclosed only by Hilbert’s work. This is a point I want briefly to explore before I move on from geometry to mechanics, and thus from mathematics to an empirical science.

It seems to most philosophers, except the most ardent deductivists, that *for the most part* humans reason non-monotonically, and thus in some way other than logically. Of course there are sceptics (David Hume, for example) who deny that non-monotonic inference can be reasonable, and who consequently reject human claims to knowledge. And the sceptics who go this way are not silly or unserious in the things they say. They

have genuine doubts as to whether the idea of non-monotonic reasoning makes coherent sense. Bertrand Russell once remarked in effect that unless he could give some reason for trusting non-monotonic inference, which he found it impossible to do, he could not see how to argue against the convictions of a man who thought himself a poached egg. That is, without first discovering, as he found it impossible to do, an argument in support of non-monotonic reasoning, Russell thought he was unable to argue anything at all. It mystified Russell how we can have our capacity to reason non-monotonically. Of course Russell was early to seize upon the nineteenth-century development of a powerful new logic, and was pressing in his work on foundations of mathematics for an understanding that reason = logic. I think that this deductivist conception was the source of defects in his philosophy.

Any reasoning that depends on discrimination of relevance relationships is non-monotonic. A prime example is reasoning which employs *ceteris paribus* or “other things being equal” statements, and thus depends on judgments concerning what is relevantly similar (equal) or relevantly dissimilar (unequal) to what. Discerning relevance seems not a mechanical process: the “frame problem of AI” is, I believe, a significant problem. At least, it is notoriously problematic whether discrimination of relevance relationships could ever take the form of formal manipulations on symbol strings. Since in principle, anything could change as the result of any other change, discriminating when “other things are equal” from when they are not seems to call all at once upon total knowledge. Our way of discerning relevance relationships apparently links to a holism or globality in cognition, a feature of cognition which we have good reason to suspect could not possibly be mechanical or thus merely logical. Our way

of discerning relevance relationships seems to involve a generalised “feeling” we have for the kind of world we’re set into. Such feelings involve intuitions which gather together into a unitary certain features of ourselves and of our world—features which resist discursive codification.

For example, such undefined intuitive notions as “length”, “breadth” and “evenly” get their content from gestures we make with our hands, things we can do with blackboard diagrams, calculations we can make, and many other skills. A remarkable fact concerning our cognition is that these diverse and multifaceted skills, these varied features of our way of being in the world, can be gathered together in a unitary cognition. The connection between my having certain diverse skills and my having an intuitive content for the notion of “length” illustrates a feature of cognition—a globality or kind of transcendental unity—which arguably (here I nod to Kant) is essential to cognition as such, and which makes non-monotonic reasoning possible. There is meaning “in my head” for ‘length’ etc., only because in some remarkable way I gather into a unitary “feel” what are complex patterns in my way of being in the world. Although innate endowment may partly explain my readiness to enter upon these patterns of activity and so to possess in my head the classical concept of length, nevertheless the patterns are (and here I correct Kant) somewhat open-textured, as is the corresponding concept of length. For example modern geometry concerns itself with lengths that are *negative*. Now in the light of intuitions we acquire early in life, it seems impossible for the magnitude of the separation of two points ever to be negative. But conservative extensions are possible of our childhood practices, through which ideas of negative lengths do make sense. The enriched practices provide a new and palpably better vantage point (for example, for the

consideration of kinematical questions using Minkowski geometry), so on learning modern geometry we may even say that in a sense we've only now learned what length is. So although we have an innate readiness to acquire the classical concept of length, the concept acquired nonetheless is open-textured and can be changed.

I claim that before Hilbert, there was in the reasoning of geometers a latent non-monotonicity. Geometrical reasoning proceeded in a way which gave a direct role to intuition, intuition which later knowledge would help to reshape. With the addition of new things to think about—non-Euclidean geometries, more powerful conceptions of the continuum -- the cogency of this reasoning was called into question. In effect, the addition of new premisses undermined the inferences. Thus the reasoning was non-monotonic.

Now whenever reasoning is non-monotonic I think it depends on a feeling for the kind of world one is set into: it depends on discriminations of relevance. Euclid presented geometry as a closed rational system, but the reasoning it involves makes use of intuition. Euclid depended on a common feeling we all have for our world's spatial form. This dependency is present in virtually all the reasoning he takes us through. As it happens, the feeling we have for our world's spatial form is less fixed than Euclid imagined. This points to something unsatisfactory in Euclid's approach. Hilbert as I have mentioned made out how Euclidean geometry could be captured as a rational system in a sense involving rationality or reasoning of a wholly monotonic sort. In consequence, Hilbert's theory of geometry has no necessary link to intuition. Hilbert said that if you like you can take beer mugs or coffee tables as points or lines: geometry



doesn't depend on there being any particular meanings for its terms, for it is a set of formal operations on symbol strings. Intuitive meaning is out of the window. That is, the complete formalisation of formerly intuitive notions allows their entire content to be carried syntactically, in the logical import of appropriate, fully discursive, axioms.

Thinking which has not reached such a state of formalisation in general will inevitably involve those inference moves which traditionally have been counted contrary to the deductivist image: analogy, induction, *ceteris paribus* deduction. Inside geometry as Euclid had fashioned it, only commonalities in our feeling for the world's spatial form enter our reasonings. The reasonings seem timeless, and Euclid makes them seem as cogent as any other reasonings from mathematics. This is why the non-monotonic character of Euclid's reasonings was hard to discern, and his science was regarded as at the furthest remove from inductive or empirical science. When we consider empirical science, we concern ourselves with relations and affections among enduring but changing things. Here again there is much that is common in our feeling for relevant similarity and dissimilarity. A proponent of the deductivist image of scientific inference will seek to reconstruct any reasonings as deductively valid in form: for example, a deductivist will take the inference to 'B' from 'if A, then (*ceteris paribus*) B' and 'A', to involve the additional premiss *ceteris paribus* — other things are equal. Likewise any apparently ampliative (inductive) inference or apparently analogical inference will be reconstructed as logically valid. The deductivist would presumably reconstruct Euclid's reasonings to his Proposition 1 as involving tacit assumptions, about what is true in the associated diagram, and about what is true in this diagram being somehow generally true. For, only by reconstructing inferences in

this way, it will be claimed, can the assumptions be laid out fully, without which the conclusion cannot be validly inferred. I think that by now we can see many reasons to doubt the worth of this prescription. The inferences are accepted as cogent because of a tacit reliance on intuition, the content of which cannot be adequately rendered in words. What is true in the diagram, which we must assume generally is true, cannot be adequately related until we have discursively delineated such ever-so-hard-to-formalise notions as that of continuity. What it means to say that other things are equal, is for the present at least quite beyond us adequately to say. The whole question concerns how we reason not logically, but intuitively— geometrically, or analogically, or inductively, or *ceteris paribus*. It is not illuminating to answer this question with the suggestion that we simply build intuitive premisses (like “other things are equal”) in reasonings which are deductively valid. This is not illuminating because it is quite unclear what these premises mean. The wrongness of the deductivist reflex is I think quite clear in the case of Euclid’s reasonings to his Proposition 1. We gain understanding of these reasonings precisely by realising that they are *not* discursive-deductive-logical, but intuitive.

Deductivists reconstruct all inferences in the sciences as logically valid. To accomplish this, they will often stray so far into artifice as to lose proper contact with the thinking of scientists. It is always possible to insist upon the deductivists’ reconstruction, but as the foregoing discussion shows, this insistence can be forced and so both unhelpful and implausible. To sustain the doctrine that rational inferences are always logically valid discursive inferences requires such stubborn adherence to it that the doctrine is rendered analytic. In short, deductivists can maintain their position only by trivialising it.

It is instructive to compare the work through which Hilbert and his predecessors perfected the discursive expression and logical organisation of geometry, with what positivists called “rational reconstruction” of an empirical science. Notwithstanding the fairly definite intuitive meanings of ‘continuity’, ‘line’, ‘point’, etc., Hilbert and his predecessors rightly held that people did not properly understand the meanings of these terms. They thought that only through painstaking logical reformulation of geometrical theory could one render clear the meaning of these terms. When they had succeeded in this rational reconstruction of geometry, the former intuitive meanings of these terms had been replaced by something purely formal.

Correlatively, positivists have always held that one cannot take for granted the meanings of theoretical terms in empirical sciences. Only through painstaking logical reformulation, they say, can one hope to render clear the meanings of these terms. Once one succeeds in such rational reconstruction of an empirical science, the intuitive meanings of various theoretical terms will have been replaced by something purely formal. I do not agree with the positivists’ conception of the form which such logical reformulation should take. Through rational reconstruction, positivists hoped to analyse the relation of theoretical terms to what they called “observation” terms, and their various ideas as to what this should come to were wrong and notably philosophically harmful. I *do* agree, however, with the positivists’ conviction that one cannot take for granted the meanings of theoretical terms in empirical sciences. And I believe that painstaking logical reformulation of some sort is often necessary, in order to render clear the meaning of these terms. Some of the issues are I think well illuminated by David Gunn in an above-cited unpublished discussion

of Jones and Musgrave. My own briefer discussion of mechanics, to which I now turn, I believe also illustrates these points.

**§4. Mechanics also a Problem Case for the Deductivist Image.** We have as I have said a problem case for the deductivist image if, in a scientific discipline, hard-to-state and thus discursively ill-defined ideas nonetheless significantly condition what is accepted as cogent argumentation. That the theory of Newtonian gravitational mechanics involves such ideas is indicated by the susceptibility of this theory to *alternative formulations*, or rather, more particularly, by problems there are at present for making out these alternative formulations as *alike in meaning*. The difficulty of determining whether or to what extent the alternative formulations are alike in meaning is precisely that, because the logical organisation of the science of mechanics is as yet imperfect, and the formalisation of some of its basic ideas is as yet far from complete, hard-to-state and thus discursively ill-defined ideas within each of the formulations of mechanics at present significantly condition what is accepted as cogent argumentation within that formulation.

I see the state of the science of mechanics today as akin to the state of Euclidean geometry before Hilbert's formalisation of it. Hilbert himself also held this view. Hilbert's sixth problem challenged mathematicians to do for physics what he had done for the science of geometry. Success so far in answering this challenge has been *very* limited. As a consequence, it is not altogether clear what we are talking about in physics. It is difficult even to determine whether we are saying the same thing a different way when we go from one formulation of a physical theory to an alternative one. The complexities are I believe well canvassed by Gunn.

A work such as Newton's *Principia* sets out a theory of mechanics much as Euclid set out his theory of geometry. The use of various key terms is regularised by means of definitions, which connect these key terms in some precise way both with one another and with further terms left intuitive and undefined. The meaning of these intuitive terms plays a significant role in shaping what reasoning is regarded as cogent. And so cogency (or what passes for such) does not amount simply to discursive deductive validity. To bring *this* about, I believe one would need to formalise, or discursively delimit, ideas left merely intuitive and informal in present formulations of the theory. If this proves possible (which it might not if, say, as in the case of arithmetic, mechanics is not first-order axiomatisable) such a formalisation would be at least as significant a reworking of the theory as was Hilbert's of Euclid's.

Present-day formulations of the theory trade undeniably in various such intuitive notions. The discussion by Gunn carefully distinguishes the formulations which ensue. These formulations involve very different commitments concerning what, according to physics, there fundamentally is, and concerning the most general forms of understanding presupposed in physics. That is, each formulation involves an apparently very different "categorical framework" for fathoming the physical world.

Perhaps the lesson we learn from studying these alternative formulations of Newtonian mechanics is that the real character of this theory is more exquisite than that of any one such "framework" which we can fathom. That is, there is a difficulty of categorization, so that we are faced with a considerable gulf between the physical laws we may actually

establish and the intuitive physical conceptions in terms of which we reckon to ourselves the significance of these laws. As we bring the theory into logically better perfected form, we may expect to learn that none of the categories thus far invoked are essential to it. Physicists themselves seem to adopt this attitude, for example, when they make out the commitments they assume on accepting and using a given theory, as pertaining just to the symmetries the theory has. This betokens a realism about structure, which is however aloof from ontology. It is a refusal to be drawn on questions which Jones and Musgrave consider important.

On the other hand, it is perhaps too early to tell whether the frameworks which seem so different from one another are or are not incompatible. Because we are faced with the alternative formulations, which seem to us quite different in meaning, we are at present left wondering what our laws really do mean. (Similarly, it has been appropriate for geometers to wonder what really is meant by ‘continuity’.) What is definite in the “intuitive content” of a concept concerns constancies in *practices*, practices which are far too rich to be discursively delineated. Constancies of practice can however be brought into connection with one another, and this can serve to give the terms we use a fuller and more coherent sense. It is appropriate to employ a principle of charity in interpreting our earlier meaning from a later vantage point. By increasing the logical perfection of the formal presentation of a theory we can improve our understanding of what we meant all along by certain (formerly intuitive) concepts. Successful formalisation of a theory may even bring us to see the former alternative formulations of the theory as after all alike in meaning. This would follow in the case of mechanics if the work towards logically perfecting the formal presentation of the theory

made completely firm and clear the logical connections that there are between the various alternative formulations of Newtonian mechanical theory.

It seems a sure thing that there *are* strong connections between these alternative formulations, since in the case of some systems (point-particle systems), it is possible to make out a mathematical link which is so tight that the difference between one and the other seems merely a difference of terminology. A problem, however, is that this class of systems is special and in fact especially unrealistic. In reality there are no systems of point particles. Realistically, every mechanical system comprises matter densities over continuous regions rather than discrete assignments of finite masses to (merely denumerably many) points, and every system involves dissipation of mechanical energy into heat. In the case of dissipative continuous systems it seems impossible to follow similar considerations to the conclusion that the alternative formulations are alike in meaning (for the terms of one of the formulations simply cannot be used). Consequently we have inadequate grounds at present to call the alternative formulations mere terminological variants of one another.

To demonstrate that two formulations of the intuitive theory of mechanics are mere terminological variants of one another, we need (on a syntactical approach) to show that merely by supplementing the sentences of one formulation with some terminological definitions (sentences which carry no content), and thus appropriately extending the language in which this first formulation is couched, we can deduce all the sentences comprising the second formulation, and conversely. Alternatively (on a

semantical approach) we would need to prove in terms of the models of the theory that the choice of formulation is a matter of indifference. Here, by ‘model’ I mean the sort of thing that Tarski talks about, not the sort of thing that a scientist would mean by ‘model’. Models in this sense are brought into view only through careful axiomatisation of the theory. This is precisely the work that has not yet been done. The two formulations are in distinct languages, so the semantical structures in Tarski’s sense for the language of one formulation are distinct from those for the language of the other formulation. Within each of the two classes of structures, a certain subclass of structures comprises all and only those that are models of the theory according to the corresponding formulation of the theory. To consider them we would be bound to face twice over, in each separate language, the daunting task of adequately axiomatising the theory. In order by semantical considerations to show the equivalence of the two formulations, we would need to show that each model in the one subclass can be linked in a natural way by an isomorphism with some model in the other subclass, and conversely.

Neither the former, syntactical task nor the latter, semantical task will be successfully carried out without there first occurring some great advances in the formalisation of the theory of mechanics. For either task to be completed successfully, we first need someone to solve Hilbert’s sixth problem (for mechanics). Only then could the present formulations of the theory of mechanics, be rigorously compared as to likeness of meaning. As is well known, very limited success has so far been achieved



in efforts to solve Hilbert's sixth problem.<sup>5</sup> So we are at present in no good position rigorously to compare the alternative formulations of Newtonian mechanics as to likeness of meaning.

A symptom of the poverty of our present-day understanding of mechanics is that we are unable to view the theory as Hilbert could geometry, as just a logically articulated set of sentences. In the present state of our knowledge, it seems better to adopt some other, non-syntactic, view, of what the theory of mechanics is. Many philosophers prefer the so-called "semantic view", which identifies the theory instead with a class of models. But this seems unsatisfactory, for surely we remain in a poor position clearly to delineate this class so long as we are hazy about the true meanings of the theory's terms. In my view we would be in a position to delineate such a class of models precisely only when we had adequately formalised the theory. But were we in that happy position, it might be tempting after all to conceive the theory as a logically articulated set of sentences. Thus I do not see, in the case of mechanics, that the so-called semantic view of theories can resolve the difficulties there are at present for the syntactic view. I think therefore that the view of theories (or anti-theory view of thought in science) which anti-realists adopt—a view which looks beyond what is formal to skills, paradigm problem solutions, informal habits of mind, language-community-delimited inference patterns, merely agreed-upon stopping points for pressing the question 'why?', and so on—deserves at least some credence, for as long as a theory is (like mechanics in the present day) in a logically unperfected state.

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<sup>5</sup>One good way to gauge this is by studying the essays and works of Clifford Truesdell, a proponent

What models or metaphysics we must associate with Newtonian mechanics is by our present lights more than somewhat hazy. The realist practitioner of mechanics does not at present quite know what to be realist about. There are problems concerning the conceptual foundations of the theory. However, these problems are in their way esoteric. Philosophical reflection is required even to register them. Few physicists complain that their reasonings lack cogency. They think that their reasonings are perfectly cogent. If in fact it cannot be claimed that these reasonings are discursive-deductive-logically valid, this is a fact that escapes almost everyone's notice, including that of the practitioners themselves. Discursively ill-defined ideas within mechanics at present significantly condition what is accepted as cogent mechanical argumentation, but this problem will be felt only by workers whose concerns seem to most practitioners pedantic, or outright philosophical.<sup>6</sup>

**§5. Understanding and Non-Monotonicity.** There have been some interesting attempts to explicate the notion of scientific understanding in terms of the unification of knowledge. On this conception, the goal of comprehending formerly disparate elements of thought together in a logically unitary way is a realist goal, for it is the goal of achieving understanding. Whether there really is any good license to expect that the world is rationally comprehensible, and whether we are licensed in any way to build this expectation into our very notion of understanding, are questions I shall not discuss here. The foregoing discussion bears rather

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and key player in the program of rational continuum mechanics. See his (1966). (1984).

<sup>6</sup>For example, by the workers on rational continuum mechanics, whose efforts are so far off the beaten track, as to attract polemical defence by some among the tiny handful of adherents to the programme. Truesdell (op. cit) is a well known such adherent.

on another problem which this view is sometimes said to have. It seems, on the view that understanding is unification, that we should be able to make out in clear formal terms what constitutes an advance in scientific understanding and why. We should be able to say in formal terms what unification *is*. Proponents of the view have in fact attempted just this. It is, however, notoriously difficult to give an adequate formal treatment of understanding as unification.<sup>7</sup>

I believe that the problem here is not with the ideal of logical comprehension, nor with the underlying realist view that understanding is unification, but rather with the present state of theories in the sciences. The theories themselves are insufficiently formalised for their unificatory merits to be made out in formal terms. It is easy enough to see, as it were, the unification achieved when Kepler's and Galileo's ideas are carried over in modified form into Newtonian mechanics, if one knows already how to set oneself into the three ways of thinking—Kepler's, Galileo's, and Newton's. In analysing physical problems in Newtonian terms it is intuitively clear how the practices one would have fallen into from following Kepler connect with those practices one would have fallen into from following Galileo. The new relatedness of two separate sets of old skills to one another, and their comprehension within the more encompassing set of new skills one acquires by becoming a Newtonian, are all easy to make out. It is only when one tries to make precise formal sense of the comprehension of Galileo's and Kepler's ideas by Newton's that one encounters problems. The problems here in my view lie not with the thesis that Newton deepened our comprehension of physical reality,

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<sup>7</sup>See Friedman (1974), Kitcher (1976), and the contributions of Salmon and Kitcher in Salmon and Kitcher (eds.) (1989).

and so not with the realist inclination to hold that understanding is unification. The fault rather lies with the thoroughly intuitive character of most theories, including the three in question. It is perfectly easy to appreciate the comprehension intuitively, but there is, because of the present state of the theories, no way at present to explicate this comprehension perfectly in formal terms.

Perhaps we could smooth the way for formalised treatments of understanding as unification by bringing the presentation of some physical theories into logically more perfected form. In the foregoing I have not disparaged such work, but simply noted that it is hard (and that even the bare possibility of eventual success is not guaranteed). That there is a place for such work seems to me, for reasons I have briefly canvassed, to validate the positivist program in the philosophy of contemporary science, for their sense of the importance of such work was I believe for the most part accurate. Realism, too, has value in so far as we seek discursive clarity because we want to understand as best we can the way things really are, and this aim importantly conditions our practices even if it is painfully hard to achieve. Constructivism with its emphasis on paradigms, skills, non-discursive elements of thought in science, and so on, also has value, as shown. Deductivism attracts allegiance in the first two camps, and antipathy from the last one. In my view the question about the camps is not which is right, for in order adequately to illuminate science one evidently must draw insights from each of them. Likewise in my view the question about deductivism is neither whether we are to build upon it an entire adequate self-consistent view of science, nor whether to build such an encompassing view of science requires its rejection. Deductivism is a philosophical thesis, which stresses ideals of discursive clarity and logical

comprehension. These ideals are operative in the sciences, but only imperfectly so. Deductivists provide insights without which I expect we would not illuminate actual science adequately. I believe also, however, that the insights of some anti-deductivists are likewise indispensable.

**§6. Conclusion.** I have argued that there are problems even in the most rigorous and exact of sciences for the deductivist image of scientific reasoning. The general lesson which I believe should be drawn, is that the reconstruction of science should not follow upon a philosophical decision to be a deductivist or an anti-deductivist (or indeed a realist or an anti-realist). It is better to approach science armed with the insights of deductivists and anti-deductivists, realists and anti-realists. Adequate illumination of actual science seems to require all these insights.

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