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Why I Am Not a Nominalist

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Introduction The sum of the divisors of 220 is 284, and the sum of the divisors of 284 is 220. The Pythagoreans spoke of numbers so related as being *amicable.* I don't know how this ancient teaching should be taken, but surely nobody nowadays, except perhaps a stray numerologist or two, would imagine that numbers are literally capable of forming friendships. A number is just not the sort of thing that can enjoy a social life. And this is but the least of a number's lacks.

A number lacks a position in space, such as tables, chairs, and other material bodies possess. It lacks dates in time, such as dreams, headaches, and other contents of minds possess. It lacks all visible, tangible, audible properties. In a word, it is *abstract.*

Disbelievers in numbers and other abstract entities or "universals" have come to be called *nominalists.* Nominalism has always attracted philosophers of the hard-headed, no-nonsense type. But does it not conflict with modern science, which speaks the language of abstract mathematics?

1 Instrumentalist nominalism Some nominalists concede that their philosophy of mathematics conflicts with science by implying that science, when it speaks the language of mathematics, is not speaking truly. These nominalists adopt an *instrumentalist* philosophy of science, according to which science is just a useful mythology, and no sort of approximation to or idealization of the truth. Truth is to be sought, rather, in a philosophy prior and superior to science.

The position of the well-known nominalist Nelson Goodman is best understood as a subtle and sophisticated variation on instrumentalism. For Goodman, science is less a useful fiction than useful *nonsense.* But whereas a

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straightforward, simple-minded instrumentalist would be willing to label science as untrue and let it go at that, Goodman holds that the philosopher ought at least to attempt to give some sense to the scientist's otherwise senseless productions by reconstructing them nominalistically:

> The nominalist does not presume to restrict the scientist. The scientist may use platonistic class constructions, complex numbers, divination by inspection of entrails, or any claptrappery that he thinks may help him get the results he wants. But what he produces then becomes raw material for the philosopher, whose task is to make sense of all this: to clarify, simplify, explain, interpret in understandable terms. . . . Nominalism is a restraint the philosopher imposes on himself, just because he feels he cannot otherwise make real sense of what is put before him. ([4], Objection vii)

Goodman's own steps towards nominalistic reconstruction of science (taken jointly with W. V. Quine in [5]) never led very far. So presumably for Goodman the bulk of science remains nonsensical.

Most recent philosophers of science, even those nominalistically inclined, have been hostile toward instrumentalist philosophies like Goodman's for a couple of good reasons. For one thing, since science is just an outgrowth of common sense, there can be no sharp dividing line between them. The most abstruse theoretical physics is connected in a thousand ways through experimental and applied science, through engineering and technology, to everyday belief. And much of everyday belief is couched in the vocabulary of mathematics, albeit of a sort more elementary than that which figures in general relativity theory or quantum mechanics. The philosopher who begins by rejecting theoretical physics as fiction will find no logical place to stop, and in the end will be unable, without inconsistency and self-contradiction, to accept commonsense beliefs as fact.

For another thing, the behavior of instrumentalists when not consciously philosophizing strongly suggests that their professed disbelief in science is a sham. Catch them off guard, and you're likely to find them classing the Steady State theory as false, and the Big Bang theory as true, just like the rest of us. The instrumentalist seems to be "engaging in intellectual doublethink: taking back in [his] philosophical moments what [he] asserts in doing science" ([2], p. 2). He seems to be "an irrational person . . . who is unwilling to accept the consequences of his own theories" ([1], p. 63).

It is on account of such *slippery slope* and *insincerity* objections that instrumentalism is not a live option for most contemporary nominalists; and it is certainly not a live option for me.

2 ***Scientific dispensability and nonexistence*** Some antinominalists have argued that the conflict between nominalism and science is so strong that nothing like modern science as we know it could survive if the nominalist ban on mathematical abstractions were accepted. Such a position has been reluctantly maintained by the ex-nominalist Quine ever since the failure of his joint attempt with Goodman at nominalistic reconstruction. Such a position was also maintained, under Quine's influence, by Hilary Putnam, during his phase of enthusiastic realism.

> I have explained early and late that I see no way of meeting the needs of scientific theory . . . without admitting universals irreducibly into our ontology. . . . Nominalism . . . is evidently inadequate to a modern scientific system of the world. ([14], pp. 182-183)

> It has been repeatedly pointed out that such a [nominalistic] language is inadequate for the purposes of science. . . . The restrictions of nominalism are devastating. . . . It is not just "mathematics" but physics as well that we would have to give up. ([12], p. 35)

In short, Quine and Putnam have maintained that mathematical objects are *scientifically indispensable.*

The refutation of this thesis has been the first aim of the most prominent recent nominalist writers, Charles Chihara and Hartry Field. The programs of nominalistic reconstruction developed in their books [1], [3] are reviewed in outline in the Appendix to the present paper. Suffice it to say here the Chihara and Field draw on results from advanced research in the foundations of mathematics (predicative analysis, measurement theory, proof theory), and that Chihara assigns the work normally done by mathematical abstractions to certain *modal* notions (including that of the possibility-in-principle of inscribing tokens of symbols of a certain formal language), while Field assigns it to certain *spatiotemporal* objects (admitting as concrete entities regions of space-time that are irregular, disconnected, and of heterogeneous material content). Their books cast considerable doubt on the thesis of the scientific indispensability of mathematical objects.

Does that suffice to establish nominalism? Chihara and Field seem to think so. While for many readers the most valuable parts of Chihara's book [1] will be the chapters on Russell and Poincaré, for the author himself, to judge by his Introduction, what is most important is the attempt to refute the antinominalist arguments of Quine, and some not dissimilar arguments of Kurt Gödel. Chihara implicitly presumes that a refutation of these arguments is tantamount to a proof of nominalism.

As for Field, his book [3] bears the subtitle "A Defence of Nominalism", but includes (on p. 4) the disclaimer that "nothing in this monograph purports to be a positive argument for nominalism". The resolution of the paradox lies in Field's presumption that nominalism does not need to be defended by positive arguments. He explicitly says that if he can accomplish the negative aim of undercutting the arguments of Quine and Putnam, then he will have reduced belief in mathematical objects to the status of "unjustifiable dogma". Thus Field, like Chihara, presumes the burden of proof to be on his lotos-eating "Platonist" opponent.

I disagree. Chihara and Field may have gone a long way toward showing that science *could* be done without numbers. I maintain, however, that science at present *is* done with numbers, and that there is no scientific reason why in future science *should* be done without them. And thus it is not the (continued) acceptance of mathematical objects, but rather the nominalist's insistence on their rejection, that constitutes an unjustified and antiscientific philosophical dogmatism.

Quine and Putnam have been false friends of numbers in making the case, for their acceptance seems to depend on a claim of indispensability. Actually, the burden of proof is on such enemies of numbers as Chihara and Field, to show either: (a) that science, properly interpreted, *already does* dispense with mathematical objects, or (b) that there are scientific reasons why current scientific theories *should be* replaced by alternatives dispensing with mathematical objects. I will call the claim (a) about the proper interpretation of current science *hermeneutic* nominalism, and the proposal (b) to replace current science by an alternative *revolutionary* nominalism.

I have argued that any antiinstrumentalist nominalism must be either hermeneutic or revolutionary. I will argue that hermeneutic nominalism, judged by the standards of linguistics, is an implausible hypothesis thus far unsupported by evidence; and that revolutionary nominalism, judged by the standards of physics, is a costly proposal thus far without scientific motivation.

3 Hermeneutic nominalism If we take everyday beliefs at face value, then we must conclude that natural numbers are posits of common sense dating from prehistoric times. If we take physics even halfway literally, then we must conclude that science has been committed to complex numbers for well over a century. According to hermeneutic nominalism, this is all illusion. General relativity theory may seem to make statements about vector-valued functions. Quantum mechanics may seem to make statements about linear operators. But in fact, no physical theory asserts or presupposes the existence of such mathematical objects; no branch of science actually posits or commits itself to the existence of abstract entities.

Hermeneutic nominalism is thus a thesis of a type that has recently been described by Saul Kripke:

> The philosopher advocates a view in patent contradiction to common sense. Rather than repudiating common sense, he asserts that the conflict comes from a philosophical misinterpretation of common language—sometimes he adds that the misinterpretation is encouraged by the 'superficial form' of ordinary speech. He offers his own analysis of the relevant common assertions, one that shows that they do not really say what they seem to say. ([7], p. 269)

Let us imagine a laboratory assistant to Lord Kelvin reporting the data in some experiments on the conversion of mechanical into thermal energy. It sounds as if he's speaking of energy-in-joules and temperature-in-degrees-Kelvin and other such numerical and abstract entities. According to hermeneutic nominalism, he's actually speaking of something completely different: Perhaps of possible chalk marks on possible blackboards (following [1]). Maybe of so-called basic regions scattered through the vastness of space-time (following [3]). Or perhaps of something still less expected and still more surprising (following some as yet unwritten rival to [1] and [3]).

Now this claim is in itself not very plausible, and it becomes even less so when we reflect that to take anything like what we find in Chihara's book or Field's as an account of what the laboratory technician is saying is to attribute to that technician a tacit knowledge of such topics in foundations of mathematics as predicative analysis and measurement theory. These subjects did not even exist in Lord Kelvin's day, and even now they are studied by few pure mathematicians, let alone working physical scientists and their technical assistants.

Kripke's words (not specifically directed against nominalism by their author) seem appropriate here:

> Personally I think such philosophical claims are almost always suspect. What the claimant calls a 'misleading philosophical misconstrual' of the ordinary statement is probably the natural and correct understanding. The real misconstrual comes when the claimaint continues, "All the ordinary man really means is . . ." and gives a sophisticated analysis compatible with his philosophy.

Certainly the burden of proof is on the proponents of hermeneutic nominalism, who claim to have discovered a radical difference between appearance and reality in scientific discourse.

As a thesis about the language of science, hermeneutic nominalism is, I presume, subject to evaluation by the science of language, linguistics. For I am prepared to dismiss those who

> . . . write as if, in addition to . . . everyday or 'garden variety' rules of English, capable of being discovered by responsible linguistic investigation carried on by trained students of language, there were also . . . 'rules' capable of being discovered only by *philosophers.* ([13], p. 5)

In the current technical jargon of linguistics, the hermeneutic nominalist's thesis that scientific statements don't really say what they appear to say becomes the hypothesis that their *deep structure* differs from their *surface structure,* while the thesis that such statements aren't really about what they appear to be about becomes the hypothesis that certain *noun phrases* in the surface structure are without counterpart in the deep structure.

Now readers of professional linguistics journals will recognize that hypotheses of this general type (though normally less radical than those of hermeneutic nominalism) are not seldom entertained by trained students of language. Such readers will also be familiar with the kinds of evidence cited in responsible linguistic investigations to support such hypotheses. Until some evidence of this kind can be adduced in support of its implausible hypotheses, I for one will be prepared to dismiss hermeneuticism as a desperate device of "ostrich nominalism".

4 ***Revolutionary nominalism*** It is one thing to observe that matters could equally well have been arranged otherwise than they currently are. It is quite another thing to urge that a rearrangement would constitute an *improvement.* To say that the British convention of driving on the lefthand side of the road is no worse than our own convention of driving on the righthand side is not to advance a *criticism* of our current traffic laws.

"Science," Putnam tells us, lives "extremely happily on the rich diet of impredicative sets" ([12], p. 56). The work of Chihara and Field suggests that science could survive on more meager fare, on a diet of inscription-possibilities or of spatiotemporal regions. But would science be *healthier* after such a change of menu?

When scientists abandoned caloric fluid and luminiferous ether, it was because they had discovered alternative theories that were *empirically* superior, of wider scope and greater accuracy in predicting the results of observations and experiments. Now the alternative theories concocted by Chihara and Field cannot be claimed to be empirically *superior* to our current theories, for they have been designed to be empirically *equivalent.*

Will it be urged that those alternatives are somehow *pragmatically* superior? Their awkward and ungainly character makes it difficult to claim that they are more convenient and efficient as systematizations of the data of experience. Will it be urged that, despite their unnatural and artificial character, they somehow contribute to clarity, simplicity, intelligibility, and the like, in ways that matter to working scientists? Something of the sort *must* be urged if a nominalistic revolution in science is to be *motivated.*

The proviso, "in ways that matter to working scientists", is crucial, if a mere instrumentalist opposition to science is to be avoided. It is pointless and futile to urge a revolution in the practice of *physicists* motivated only by considerations appealing only to *philosophers* of a certain type. Physicists are too well aware of the dismal historical record of philosophical interference in science to accept such dictation from outsiders.

Now the avoidance of ontological commitments to abstract entities does not seem to have won recognition in the scientific community as being in itself a goal of the scientific enterprise on a par with scope and accuracy, and convenience and efficiency, in the prediction and control of experience. It seems, on the contrary, a matter to which most working scientists attach no importance whatsoever. It seems distinctively and exclusively a preoccupation of philosophers of a certain type. Thus Goodman is able to cite only a few linguists who are nominalistically inclined, and not one physicist:

> Paucity of means often conduces to clarity and progress in science as well as philosophy. Some scientists indeed–for example, certain workers in structural linguistics–have even imposed the full restriction of nominalism upon themselves in order to avoid confusion and self-deception.

One would search the *physics* journals in vain for any expression of nominalistic qualms and scruples, of reluctance and hesitancy to use mathematical apparatus, of suspicion that such "Platonistic claptrappery" as complex numbers may be a source of "confusion and self-deception".

The proposed nominalistic revolution in physics can be scientifically motivated only by showing that the avoidance of ontological commitments to abstract entities would somehow serve indirectly to advance us toward some more recognizably scientific goals. For my own part, I cannot discern any such scientific *benefits* to be expected from the proposed revolution, while I do discern a couple of nonnegligible *costs.*

First, any major revolution involves transition costs: the rewriting of textbooks, redesign of programs of instruction, and so forth. A reform along the lines of Chihara's [1] would involve reworking the mathematics curriculum for science and engineering students, avoiding impredicative methods in favor of predicative parodies that are harder to learn and not so easy to apply.

A reform along the lines of Field's [3] would involve reworking the physics curriculum, so that each basic theory would intially be presented in qualitative rather than quantitative form. A course on measurement theory would have to be crammed into the already crowded study plan, to explain and justify the use of the usual numerical apparatus. This is educational reform in precisely the wrong direction: away from applications, toward entanglement in logical subtleties.

Second, the physicist who puts on nominalistic blinders may be unable to see certain potentially important paths for the development of science. I have in mind here not an inevitable logical consequence of nominalistic revolution, but a likely psychological consequence: Chihara ([1], p. 209) promises that he will recant his nominalism should some future physical theory turn out to require mathematical objects indispensably. But the danger I have in mind is that if science goes nominalist today, that future theory *may simply never be discovered.* Yuri Manin has noted this point in connection with intuitionism:

> Unfortunately, it seems that it is these "extremes"–bold extrapolations, abstractions which are infinite and do not lend themselves to a constructivist interpretation–which make classical mathematics effective. One should try to imagine how much help mathematics could have provided twentieth century quantum physics if for the past hundred years it had developed using only abstractions from "constructive objects". Most likely, the standard calculations with infinite dimensional representations of Lie groups which today play an important role in understanding the microworld, would simply never have occurred to anyone. ([11], pp. 172-173)

(Mention of quantum mechanics should remind us that it is unclear whether the methods of Chihara and Field are adequate even for present-day science in its entirety. For Chihara the problem is a minor one, and could probably be solved by adopting a somewhat stronger system of predicative analysis than the particular weak system Σ_ω he favors. For Field, the problem is a major one, for he has given us no idea how he proposes to treat quantum theory, which differs radically (owing to its use of *infinite-dimensional* apparatus and to its *statistical* character) from the one theory he does treat in detail, Newtonian gravitational theory).

But I need not enlarge on the costs for present-day and future physics of a nominalistic revolution. Surely the burden of proof is on the revolutionary, who proposes a drastic departure from our thus far eminently successful policy of ontological tolerance in common sense and scientific theory construction.

Until it is shown that nominalism offers physical science some substantive advantages, I for one am prepared to dismiss its revolutionary proposals as motivated only by medieval superstition ("Ockham's razor") and fastidious bigotry (cf. [4], Objection viii).

Chihara and Field have gone a long way toward constructing nominalistic alternatives empirically equivalent and pragmatically only slightly inferior to our current scientific theories. Their work suggests that an ontology of abstracta may be one feature of those current theories that is merely *conventional,* in the best sense of the word (that of David Lewis [8]). This suffices to cast considerable doubt on some more *extreme* versions of realism.

It does not suffice to cast doubt on *moderate* versions of realism, which merely observe that our current theories seem to invoke abstracta and that we do not yet have reasons to abandon those theories. For to characterize some feature of our present ways of doing things (in scientific theorizing or in driving) as conventional is not in itself to criticize that feature. And Chihara and Field have not come close to constructing nominalistic alternatives that are manifestly *superior* (empirically or pragmatically) to our current scientific theories.

*5 **Nominalism, ontological and epistemological*** I have rejected nominalism in its traditional *ontological* form, as the doctrine that there exist no abstract entities. I equally reject it in its currently fashionable *epistemological* form, as the thesis that even if there exist any abstract entities, still we could never come to know about their existence. Epistemological nominalism is usually supported by an argument of the following form: All entities of which we can have knowledge are causally connected with our organism; no abstract entities are causally connected with our organism; ergo, no abstract entities are entities of which we can have knowledge.

The argument is, of course, valid, a syllogism in Camestres. But the premisses are dubious and debatable. As for the minor premiss, of course a cyclic group does not act on our organs of sight, touch, and hearing in the same way as an alarm clock. And nobody nowadays, except perhaps a stray numerologist or two, would imagine that mathematical objects act on us through some mysterious sixth sense of ESP unknown to orthodox physiology. Nonetheless, as Maddy has skillfully argued in [9], there is a good deal of research in developmental psychology and neurophysiology that can be read as showing that we do, in a sense, have causal contact with certain abstracta.

As for the major premiss, it rests on a *causal theory of knowledge.* That theory has many opponents, who regard it as a half-truth arrived at by overhasty generalization from too narrow a range of cases, to which the cases of knowledge of mathematical objects, ethical values, other minds, and so forth are just so many counterexamples. Significantly, that theory has also a good many half-hearted sympathizers, who do not regard it as wrongheaded or misguided, but merely as in need of amendment. In many amended versions, the notion of causality disappears, to be replaced by that of reliability or explanation or something of the sort, and with it disappears the major premiss of the epistemological nominalist's syllogism. Again Maddy [10] provides a useful survey of the issues.

The more cautious sympathizers with the causal approach to the theory of knowledge now maintain only that the abstractness and consequent causal inertness and isolation of mathematical objects creates difficulties for the epistemologist trying to account for mathematical knowledge. I am surprised to find Field citing these epistemological difficulties as if they in themselves constituted some sort of grounds for nominalism:

> [Nominalism] saves us from having to believe in a large realm of . . . entities which are very unlike the other entities we believe in (due for instance to their causal isolation from us and from everything that we experience) and which give rise to substantial philosophical perplexities because of those differences. ([3], p. 98)

A footnote to this passage makes it plain that Field's "philosophical perplexities" are precisely the epistemological difficulties just alluded to. (Incidentally, the same footnote provides a good bibliography of works arguing for epistemological nominalism.)

To bring out just how odd this argument is, I want to consider a parallel: Suppose that Burrhus Skinner were to confess that after all those years of work with his rats and pigeons he's still "substantially perplexed" by the ability of freshman students to master calculus and mechanics. Now what mathematician or physicist would take that as motivation for rewriting the textbooks in those subjects? What linguist would take it as evidence that the sentences in those textbooks have some bizarre and outré depth grammar? No one would take it as an indication of anything but the inadequacies of behaviorist learning theory.

Likewise, a philospher's confession that knowledge in pure and applied mathematics perplexes him constitutes no sort of argument for nominalism, but merely an indication that the philosopher's approach to cognition is, like Skinner's, inadequate.

Conclusion Unless he is content to lapse into a mere instrumentalist or "as if" philosophy of science, the philosopher who wishes to argue for nominalism faces a dilemma: He must search either for evidence for an implausible hypothesis in linguistics, or else for motivation for a costly revolution in physics. Neither horn seems very promising, and that is why I am not a nominalist.

Appendix For the reader's convenience, I here outline the constructions of Chihara and Field, and the claims which those authors make for their constructions. I will not advance any *technical* objections against those constructions (though in fact I have one small reservation about Chihara's approach, and share with Kripke several large reservations about Field's), since my aim has been to argue that even if the constructions are technically flawless, they do not suffice to establish nominalism.

A Chihara's modal nominalism I here outline the constructions of [1], Chap. V and Appendix. Chihara's strategy is to reinterpret in a nominalistically acceptable fashion a portion of pure mathematics: arithmetic first, then so-called *predicative* analysis. He then argues that the portion of mathematics so reinterpreted suffices for scientific applications, and dismisses the rest of mathematics (the impredicative part) as mythology.

To illustrate Chihara's approach to arithmetic, consider Euclid's famous theorem:

(0) $(\forall \text{ number } m)\,(\exists \text{ number } n)\,(m < n \;\&\; n \text{ is prime})$

As a first attempt to avoid mathematical objects, let us rewrite this as:

(1) $(\forall \text{ numeral } a)\,(\exists \text{ numeral } b) \ldots$

(I will indicate only the transformation of the *prefix* of (0); this isn't to say that the transformation of the *matrix* doesn't require some caution.) Now if numerals are taken as *types* (patterns of inscription), then they are themselves abstract entities akin to shapes, and (1) is not much of an improvement on (0). Now if numerals are taken as *tokens* (individual inscriptions), then they are concrete entities, made of chalk or ink, but there may not be (indeed, almost certainly aren't) enough of them around to make (1) true. To get a version of (0) that is both true and committed only to concrete entities, we must introduce the *modal* notions of necessity ($\Box$) and possibility ($\Diamond$). Then, taking numerals as tokens, our final reinterpretation of (0) is:

(2) $\Box(\forall \text{ numeral } a) \Diamond(\exists \text{ numeral } b) \ldots$

Informally this says: However long a tally you could ever write down, I could write down a still longer one such that . . .. Here we have the idea behind the approach to arithmetic in [1].

Chihara's approach to analysis builds on the work of the *predicativists* (specifically, Hao Wang), mathematical constructivists somewhat more liberal than intuitionists. Predicativists accept uncritically classical *arithmetic* (theory of natural, or equivalently rational, numbers), but in *analysis* (theory of real numbers, or equivalently of *sets* of natural numbers) they accept only what is definable. To begin with, they accept those sets of natural numbers that are definable by purely arithmetical conditions, conditions quantifying only over natural numbers. These are the *order zero* sets. Next they accept those sets of natural numbers that are definable by conditions quantifying over natural numbers and order zero sets. These are the *order one* sets. And so on, through higher and higher order. (Just how high to go is a delicate question.) A surprisingly large portion of classical mathematics can be "parodied" within this framework, as the survey [2] shows. Intuitively, it is plausible that a theory of definable sets should be reducible to arithmetic plus truth-predicates, with quantification over definable sets being replaced by quantification over the code numbers of their defining conditions, and the membership relation replaced by the relation "n is the code number of a formula with one free variable that is *true* of m". The details can be worked out, and we get a reduction of predicative analysis to something that has already been shown to be nominalistically reinterpretable.

Chihara's account of the application of mathematics in science is illustrated by Figure 1. While scientific theories are formulated mathematically in terms of sharply defined functions, at least in the overwhelming majority of applications empirical conditions define only fuzzy functions. For instance, the condition:

$f(t) = x$ iff the projectile is x meters above the floor of the chamber at t seconds after firing

cannot define a sharp function because of the fuzziness of the projectile and the chamber (viewed on a scale of micrometers) and of the firing-event (viewed on a scale of nanoseconds). Thus the application of a scientific theory to empirical conditions typically involves an element of idealization. Now predicative mathematics provides sufficiently many sharp functions to serve as idealizations of empirically-defined fuzzy functions, because any classical function can be approximated as closely as desired by a predicative function. Moreover, the theorems of analysis used in deriving consequences from basic scientific theories can all be parodied predicatively: This can be verified by comparing the mathematics curriculum for science and engineering students as listed in any college catalogue with the survey [2] of predicative mathematics. Thus predicative mathematics, which we have already seen to be nominalistically reinterpretable, suffices for scientific applications.

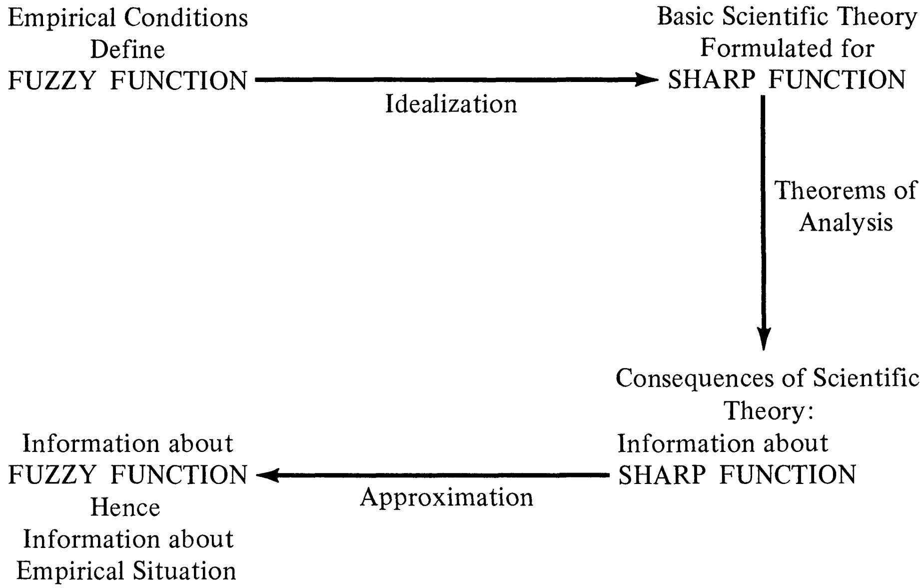

Figure 1.

B Field's spatiotemporal nominalism I here outline the construction of [3]. Field's strategy is to reformulate basic scientific theories and their consequences (the special information they entail about special situations) in a way that avoids all mathematical vocabulary, and then to argue that the consequences can be deduced from the basic theories without introducing any mathematics.

To illustrate Field's approach to the formulation of science, let us consider, as he does, thermodynamics. Here a typical *qualitative,* math-free, nominalistically acceptable notion would be the comparative relation R between point-events of space-time given by "x is cooler than y". Here a typical *quantitative,* mathematical, "Platonistic" notion would be that of temperature in a given scale, conceived of as a real-valued function r on point-events of space-time. *Measurement theory,* as surveyed in the compendium [6], is a corpus of theorems to the effect that suitable assumptions on qualitative relations entail the existence (and uniqueness up to stated changes of scale) of quantitative functions appropriately representing them. In thermodynamics, suitable assumptions would include that R is irreflexive and transitive, appropriate representation would include that xRy if and only if $r(x) < r(y)$, and stated changes of scale would be like those used in passing between Farenheit and Celsius. Once we have such a basic *representation theorem,* it becomes possible to reformulate any scale-invariant assumption on the quantitative functions as an assumption about the qualitative relations. In thermodynamics, *continuity* for the temperature function r can be reformulated in terms of a notion of *temperature-basic region,* itself defined in terms of the cooler relation R.

In this way the nominalist can reformulate the whole of science, both basic, general theoretical principles, and particular consequences for practical

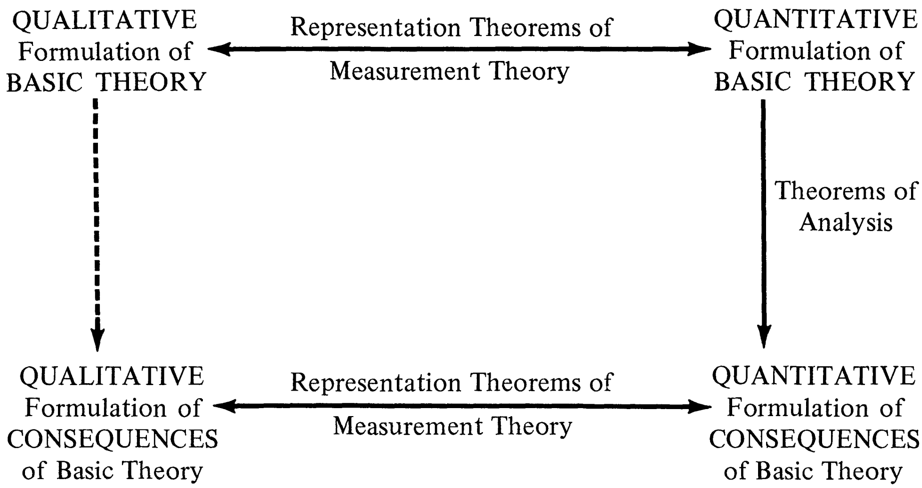

Figure 2.

applications. However, the only route we have seen so far from the qualitatively formulated version of a basic theory to the qualitatively formulated versions of its consequences involves a "Platonistic" detour, as in Figure 2: from qualitative basic theory by measurement theory to quantitative basic theory, thence by theorems of analysis to quantitative consequences, and thence by measurement theory again to qualitative consequences. It is, however, theoretically possible, though practically inconvenient, to avoid the introduction of mathematics, to avoid the detour through the quantitative and abstract:

> . . . the conclusions we arrive at by these means are not genuinely new, they are already derivable in a more long-winded fashion . . . without recourse to the mathematical entities. ([3], pp. 10-11)

> . . . for these purposes ["problem solving"] the usual numerical apparatus is a practical necessity. But it is a necessity that the nominalist has no need to forgo: he can treat the apparatus . . . as a useful instrument for making deductions from the nominalistic system that is ultimately of interest; an instrument which yields no conclusions not obtainable without it, but which yields them more easily. ([3], p. 91)

These claims are supported by appeal to *conservation theorems* from *proof theory* (the most important being perhaps one due to Scott Weinstein).

REFERENCES

[1] Chihara, C., *Ontology and the Vicious Circle Principle,* Cornell University Press, Ithaca, New York, 1973.

[2] Feferman, S., "Theories of finite type related to mathematical practice," in *Handbook of Mathematical Logic,* ed., K. J. Barwise, North Holland, Amsterdam, 1977, pp. 913-972.

[3] Field, H., *Science Without Numbers: A Defence of Nominalism,* Princeton University Press, Princeton, New Jersey, 1980.

[4] Goodman, N., "A world of individuals," in *Philosophy of Mathematics,* ed., P. Benacerraf and H. Putnam, Prentice-Hall, Englewood Cliffs, New Jersey, 1964, pp. 197-210.

[5] Goodman, N. and W. V. O. Quine, "Steps towards a constructive nominalism," *The Journal of Symbolic Logic,* vol. 12 (1947), pp. 97-122.

[6] Krantz, D., R. Luce, P. Suppes, and A. Tversky, *Foundations of Measurement,* vol. I, Academic Press, New York, 1971.

[7] Kripke, S., "Wittgenstein on rules and private language: An elementary exposition," in *Perspectives on the Philosophy of Wittgenstein,* ed., I. Block, Blackwell, London, 1981.

[8] Lewis, D. K., *Convention,* Harvard University Press, Cambridge, Massachusetts, 1969.

[9] Maddy, P., "Perception and mathematical intuition," *Philosophical Review*, vol. 89 (1980), pp. 163-196.

[10] Maddy, P., "Mathematical epistemology: What is the question?," to appear.

[11] Manin, Y., *A Course in Mathematical Logic,* trans. from Russian by N. Koblitz, Springer, Berlin, 1977.

[12] Putnam, H., *Philosophy of Logic,* Harper & Row, New York, 1971.

[13] Putnam, H., "Truth and necessity in mathematics," in *Mathematics, Matter, and Method,* Cambridge University Press, Cambridge, Massachusetts, 1975, pp. 1-11.

[14] Quine, W. V. O., "Response to David Armstrong," in *Theories and Things,* Harvard University Press, Cambridge, Massachusetts, 1981, pp. 182-184.

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