On the Scope and Force of Indispensability Arguments[1]

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1. Introduction and Background

One nice thing about the title of this symposium is the ease of finding a one-word answer. A disadvantage, however, is that "science" is ambiguous: is "philosophy of mathematics" included in "philosophy of science" or not? If so, the one-word answer is *obviously* obvious; but if not, the same answer is still pretty obvious, or so I hope it will appear by the end of this symposium. In fact, I believe that recent work in foundations of mathematics, especially of the sort represented in this symposium, serves to sharpen considerably central debates in both philosophy of mathematics and philosophy of science generally, especially on the question of *realism* (vs. instrumentalism, or varieties of constructivism, or nominalism), and on the question of the *indispensability* of highly theoretical concepts and axioms for the more observational portions of mathematics and science.

More specifically, I would like to focus attention on the following three questions:

(1) To what extent is classical, infinitistic (non-constructive) reasoning indispensable for *scientifically applicable mathematics*?

(2) To what extent are *impredicative set existence principles* indispensable for (a) *finitistic mathematics*; and for (b) *scientifically applicable mathematcs*?

(3) More broadly, what can we say today about the status of Gödel-Quine-Putnam indispensability arguments as a source of justification of abstract mathematical axioms and methods?

It will not be possible to treat more than the first of these questions in any kind of detail here, but the following remarks on each of them may be useful by way of background.

The first question, of course, goes back to the controversy between Brouwer and Hilbert over the justifiability of classical infinitistic reasoning and existence assumptions. Many classicists—including Hilbert—recognized a genuine challenge to classicism that the intuitionist critique had raised, and today that challenge can be seen as all the greater in light of the striking undecidability results of Gödel, Cohen, and their successors. (I

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refer here especially to the whole subject of large cardinal axioms and related problems in higher set theory.) However Hilbert also regarded Brouwer's constructivism as incapable of doing justice to mathematics as a scientific subject. (Recall his famous line from (1927) that "taking the principle of the excluded middle from the mathematician would be the same, say, as proscribing the telescope to the astronomer or to the boxer the use of his fists.") And a similar conclusion was finally reached by Hilbert's great student, Hermann Weyl (1949), who had strong sympathies for Brouwer's program and had much earlier developed his own variety of constructivism, a form of predicative analysis. A turning point, however, came in the 1960's with the appearance of Erret Bishop's *Foundations of Constructive Analysis* (1967) which succeeded in constructivizing large portions of classical functional analysis of just the sort used in the physical sciences (including the theory of operators on Banach and Hilbert spaces, and even measure theory), but without relying on the controversial Brouwerian theory of choice sequences. (Instead, Bishop worked with a schematic notion of "constructively converging sequence" and reasoned with intuitionistic logic, so that virtually any mathematician can read and assent to his theorems and proofs.) From the standpoint of applications of mathematics, it is no exaggeration to say that Bishop's work breathed new life into constructivism. Whether Bishop's constructivism can really serve the needs of the natural sciences is a question I will return to momentarily.

Concerning the second question, *predicativist* mathematics—as initiated by Poincaré and Weyl, and developed by Feferman, et al.—falls between Bishop constructivism and classicism: it employs classical logic, but it limits its set or class existence axioms to those that are definable or specifiable by formulas of the mathematical language which themselves quantify only over sets (or functions) "already definable" (in an inductively specified sequence of stages). Indeed, predicativism subscribes to "definitionism", the doctrine that all mathematical objects should be explicitly specifiable in a humanly learnable language, and since the possibilities so specifiable are only countably many, predicative mathematics seeks to get by without ever countenancing uncountable totalities (as described from "outside", that is: from inside a predicativist system, of course Cantor's theorem is provable and predicative types of internally higher cardinalities may be recognized). Despite this, largely because it is not confined to intuitionistic logic, predicative systems can be very powerful, and it is much harder to find examples of mathematical results used in the physical sciences, say, that predicativism cannot reproduce than it is to find such results that exceed the powers of Bishop constructivism.

Concerning the broader question of the state of indispensability arguments, recall that these go back to Gödel, who proved in effect that there is no limit to the extent of set theoretic strength that may be required to prove certain finitistic statments, e.g. statements of consistency of formal systems, e.g. of set theory itself, coded as sentences of arithemtic. Here of course indispensability means "for purposes of proving finitistic statements"; but the examples Gödel discovered were metamathematical in character and depended on coding of syntax. Since Gödel's observations, the mathematical community has wondered whether all examples of undecidables (of number theory and analysis, especially) would be of this character, or whether there might not be real mathematical problems of the sort ordinarily encountered in mathematical work requiring higher axioms of set theory for their solution. A breakthrough along these lines was achieved in the mid-seventies with the well-known result of Paris and Harrington (1977) providing a finite form of Ramsey's partition theorem provable in a rather weak set theory but undecidable in Peano arithmetic. Since then, many further examples have been discovered, especially by Harvey Friedman, who has pursued very far the rigorous investigation of indispensability arguments in connection with both finitistic and "low-level infinitistic" mathematical problems. An important example concerns a theorem of Kruskal, which states that there is no "bad sequence" of finite trees, i.e. no infinite sequence of distinct finite trees such that no member is embeddable in a later member. This is not a finitistic statement, but Friedman discovered a finite "miniaturization" which follows from Kruskal's theorem (and so is true) but which cannot be proved in the subsystem of classical analysis known as ATR_0, which is generally taken to correspond to *predicative analysis*. [The finite form of Kruskal states that $\forall c \exists k$ so large that for any finite sequence $\langle T_0, T_1, \ldots, T_k \rangle$ of finite trees with $\text{card}(T_i) \leq c(i+1)$, there are $i < j \leq k$ such that T_i is embeddable in T_j. The condition on the cardinality of the trees is a linear growth restriction. For further details, see (Simpson 1985a).] The statement is somewhat more complicated than Kruskal's theorem, to be sure, and I suppose it was discovered with ulterior motives, but it is surely mathematical as opposed to metamathematical, and, anyway, are motives relevant? (Is "trust" really the issue?)

It should be noted that Friedman has found further results along these lines requiring still more powerful impredicative existence assumptions to prove. Indeed they do become more complicated to state.

When it comes to "low-level infinitistic" statements, Friedman has achieved a whole series of results that are equally striking and important: these show that various statements at the level of Cantor's diagonal argument (and in fact closely related to that argument) require stronger and stronger set theoretical axioms of infinity—large cardinal axioms—to prove. (Some cases are provable in ZFC with the axiom of measurable cardinals but not in ZFC with the axiom "there is a Ramsey cardinal"! For a survey of results with further references, see (Nerode and Harrington 1985).) Again the statements are clearly "mathematical" as opposed to "metamathematical", and again I will not get into the question of ulterior motives.

Whereas Gödel emphasized indispensability for finitistic or other sufficiently low-level mathematical statements, Quine (1953) (1976) and Putnam (1971) extended the potential significance of indispensability in an interesting way by considering applications of mathematics in the natural sciences. As already suggested, indispensability arguments can be used *negatively*, to rule against restrictive programs such as Brouwer's intuitionism, by showing such programs incapable of expressing or proving mathematical results needed by the sciences. More problematically (and therefore (?) philosophically more interestingly), they can be used *positively* as in "inferences to the best explanation" to argue for the *truth* of the key mathematical axioms in question. This strategy belongs to the view known as "quasi-empirical realism". It might also be called, "trickle-in mathematics": On Quine's holistic view of testing, positive confirmation results perforce when a mathematical axiom must be used as part of a whole theory to derive testable consequences; confirmation of the theory by such consequences "trickles in" toward the center of the conceptual system. The result of deleting the axiom would be a theory with reduced explanatory scope, *ceteris paribus*, a poorer theory. Here, I take it, the question of ulterior motives does not arise: the aim is to serve the needs of the natural sciences, wherever they may take us.

Finally, it should be remarked that, although Quine has tended to assume that scientific indispensability would result in a justification of abstract set theoretic principles, when we take into account the power of restricted systems, such as Bishop's constructivist analysis and, especially, Feferman's flexible systems of predicative types, it is far from clear just how far such justification can extend. If, as Feferman has conjectured (1988) (1989), virtually all scientifically applicable mathematics can be carried out in a system like his W (for Weyl, or "Weyl Vindicated" (that's one 'W' and one 'V', not two 'W''s!)), then Quine-Putnam indispensability arguments might not even take us to the existence of an uncountable cardinal; perhaps, more drastically, they could not even help justify the existence of *all* subsets of natural numbers! (It should be mentioned that results as to the power of

Friedman's subsystems of analysis, employed in "reverse mathematics", could be invoked to raise much the same question. It is quite remarkable just how much of ordinary mathematics can be carried out in these weak subsystems, even weaker than ATR_0.) (For a survey, see Simpson (1985b).) [Incidentally, in "reverse mathematics" one demonstrates that a characteristic *axiom* of one of these subsystems of analysis, e.g. the (weak) König infinity lemma, is derivable (in a weaker base theory) from an important mathematical *theorem*, e.g. the Heine-Borel theorem, or the separable Hahn-Banach theorem, etc., thereby showing that the proof of such a theorem *requires* at least that much strength. In this sense, reverse mathematics is but the rigorous pursuit of (internal) indispensability arguments.]

2. Beyond Brouwer and Bishop: On the Need for Nonconstructive Analysis

Despite the remarkable extent to which Bishop and his successors have taken constructive mathematics, there *are* limitations pertaining to physical applications of mathematics which I believe tell quite decisively against the adequacy of constructive mathematics. There are two types of limitations I will discuss, the first pertaining to the expressive power of constructivist mathematical language, the second concerning the power of constructivist systems to recover proofs of important classical theorems of mathematical physics.

Let us consider the first type first. The problem was alluded to by Weyl (1949, p. 61) when he wrote, "The propositions of theoretical physics, however, certainly lack that feature which Brouwer demands of the propositions of mathematics, namely that each should carry within itself its own intuitively comprehensible meaning." If we supplement this with explicit reference to the intuitionistic logical apparatus of connectives and quantifiers together with the restriction of function quantifiers to *constructive functions*, then we have a clear indication of where the problems lie. As Putnam has emphasized (e.g. in (1975, p. 75)), constructive quantifiers are frequently inappropriate in physical contexts, and my points here can be seen as an elaboration of this insight.

Examples of essential use of non-constructive quantifiers can be found in a wide range of contexts, ranging from quite ordinary ones to key portions of functional analysis used in quantum mechanics. Let us consider the ordinary ones first.

Suppose I conjecture that, no matter how long we were to search, we would never encounter intelligent life outside the solar system (but that we would never know that there is no such life either). If we attempt to read this conjecture using intuitionistic logical machinery, we end up conjecturing the following:

> that we will have a method of reducing to absurdity any "proof" that we had encountered intelligent life outside the solar system.

Here you may read "proof" in any way suitable to empirical contexts such as this one (and similarly, you could adjust the meanings of "method" and "reducing to absurdity" as well). Evidently, this is a serious distortion of the conjecture, which says that we may never know one way or the other, not that we will be able to *refute* the claim to encounter such life. But the distortion is part and parcel of the intended meaning of the intuitionistic combination $\forall x \neg \ldots$: the universal quantifier carries a strong existential commitment to the having of a method of proving what follows, and the negation is defined as intuitionistic implication of absurdity (having a method of passing from a proof of the negated statement to a proof of absurdity). But such distortions inevitably result whenever we use 'never' in this manner, to assert that we may never decide something.

Ironically, this very distortion occurs within constructive mathematics itself, as it is commonly practiced. Namely, it occurs in connection with the so-called method of weak

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counterexamples, which is used to demonstrate the essential non-constructivity of various classical theorems by showing that, were the theorem in question constructively provable, a method would become available (e.g. by a clever construction of a real number) that would automatically solve an unsolved existence problem in number theory of a general form. To quote Dummett, the method gains its force from the consideration that "we can be virtually certain that the supply of such unsolved problems will never dry up...Such recognition that a ... statement is unprovable does not amount to a proof of its negation..." (Dummett 1977, p. 45) Surely this is correct. However, if we attempt to formalize this use of 'never' using intuitionistic connectives, that is precisely what we unintentionally end up claiming! Indeed,

$$\forall_i c \neg_i (c \text{ proves p})$$

actually is equivalent to the intuitionistic negation of p. For this reason, the intuitionistic statement that p is (absolutely) undecidable involves an internal contradiction! (For a rigorous demonstration of this, see (Hellman forthcoming).) Moreover, asserting intuitionistically that the supply of unsolved problems of a certain form will *never* dry up (in the sense that they will always be available) commits one to the having of a *method of producing* such problems, which is clearly not intended. This shows that expressing the very rationale behind the method of weak counterexamples requires use of non-intuitionistic connectives! Since open-ended infinitistic quantification (over "constructions") is involved, this strongly suggests a parasitic dependence on classical logic of the very sort intuitionism seeks to avoid. (Cf. (Hellman 1989).)

To sum up: In intuitionistic mathematics as in politics, *never say "never"*!

Turning to quantum mechanics, an important theorem of Pour-El and Richards (1983) implies that unbounded linear operators (meeting the rather weak condition of being *closed*) of the sort commonly used in quantum mechanics fail to preserve computability of vectors as inputs, where this was spelled out axiomatically for Banach spaces appealing to notions of recursive analysis and some intuitive closure conditions that any notion of computability (of Banach space elements and sequences thereof) should satisfy. [An operator T is *closed* just in case, if $<x_i>$ is a sequence of vectors converging to x and $<T(x_i)>$ is a sequence converging to y, then x is in the domain of T and $T(x) = y$.] Both Feferman and I independently and separately have generalized the Pour-El and Richards theorem so that it applies to constructivist theories such as Bishop's and does not depend on recursive analysis. ((Feferman 1984), (Hellman 1993b).) The upshot is that unbounded linear operators of the sort used in quantum mechanics necessarily fail to preserve *constructivity* of inputs, and are thus *non-constructive functions*, not countenanced by Bishop's mathematics!

Now unbounded linear operators abound in quantum mechanics. Indeed most operators of interest, including those for position, linear momentum, energy, creation and annihilation, etc., are of this class. It should be noted, moreover, that due to the variety of potential functions, the Hamiltonian operators for energy form an infinite class. Now nothing prevents the constructivist from writing down an expression for one of these unbounded operators and using it to perform calculations. Rather what is ruled out is quantification over such operators as mathematical objects. Thus, the mathematical theory of such objects, including central analytic theorems such as the spectral theorem, Stone's theorem, etc., is inaccessible.

Now it might be suggested that, in practice, *bounded* operators suffice for physical problems, since we usually know that the values of the magnitudes of interest lie within a finite interval of the real line. (So, the argument runs, a projection operator from the spectral family for the original (unbounded) operator can always be used instead of the unbounded operator.) Now, even for purposes of physical calculations, this suggestion confronts difficulties arising from incompatible pairs of observables (such as position and momentum): as is well-known from Fourier analysis, if one of such a pair of quantities is confined in a state to a bounded interval, the other can take on arbitrarily large values with positive measure, so that calculation of, say, the expectation value of the latter in that state will in general require an unbounded operator.

But even more important is the theoretical role of unbounded operators: in short, they are crucial to an understanding of the structure of quantum mechanics and its relation to classical mechanics. To mention very briefly how: (i) the *canonical commutation relations* between position and displacement operators (relying on Stone's theorem) forges an important link between physical spatial properties (localizability and homogeneity of space) and the commutation rules for the position and momentum operators; (ii) the *dynamical structure* of the theory, espressed in the derivation of the abstract Schrödinger equation from time-translation invariance, depends on Stone's theorem and unbounded Hermitian operators, identifiable with the Hamiltonian operators; (iii) the *correspondence principle*, expressing deep connections between quantum and classical mechanics depends on the functional calculus provided by the spectral theorem; (iv) the classical limit theorems and the Ehrenfest equations involve quantification over unbounded operators to calculate the form of the time derivative of expectation values (in terms of the expectation of the commutator with the Hamiltonian). This list should suffice to convince us that to deprive quantum physics of the theory of unbounded operators would be like telling the boxer to engage in a different sport!

Here we see an important link between philosophy of physics and philosophy of mathematics: the importance of theoretical understanding in physics leads to constraints on foundational mathematical programs, through an *expansive* conception of "scientific applications" to be respected. In terms of some favorite "isms": scientific realism certainly tends to rule out certain types of mathematical anti-realism. True to Wesley Salmon's dictum, that one person's *modus ponens* is another's *modus tollens*, an implication can be run the other way: the viability of constructivist mathematics would preclude taking seriously significant portions of theoretical physics. Some die-hard constructivists may espouse this line, "So much the worse for theoretical physics," but this surely has a ring of desperation about it. (Not all constructivists respond in the same way, it should be pointed out. While at least one prominent intuitionist is apparently prepared to adopt the latter stance, Martin-Löf has acknowledged the need to develop a constructive theory of unbounded operators for quantum mechanics.[2] I do not know how he proposes to get around the Pour-El and Richards limitation.)

This leaves me just enough time to mention one further limitative result, pertaining now not to the expressive power of constructivist language, but to limitations of constructive proof. As it turns out, one of the most important results in the mathematical foundations of quantum mechanics, while it can be constructively stated, cannot be constructively proved. This is the famous theorem of Gleason (1957) characterizing the generalized probability measures on the closed subspaces of a Hilbert space (of dimension three or greater). It says, in effect, that the only mathematically possible such measures are given by the quantum mechanical pure and mixed states (and, in particular, there can be no dispersion-free measures, this corollary ruling out one important class of no-hidden-variables theories for quantum mechanics). The theorem answers the central question: how can *probability* be introduced into quantum mechanics, given the Hilbert space formalism? It thus has fundamental importance quite apart from the famous corollary. However, not only is the proof non-constructive, relying on the classical Bolzano-Weierstrass theorem; *it is not possible to prove the theorem (or even its relevant special cases) constructively.* (The details are in Hellman (1993a).)

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Now in one respect, this sort of result is less decisive than the one just described on unbounded operators: what cannot be proved constructively is the constructive statement of Gleason's theorem, but this does not settle whether some alternative statement might be so provable that could be claimed "to do the same work". Some effort has been expended on this, so far to no avail, but the question is too indefinite to settle at this point. At least a challenge is posed: prove an adequate substitute, or face the consequences!

(Incidentally, in this connection, Martin-Löf (at the symposium cited in n. 2, above) took the position that Gleason's theorem isn't "part of physics" since it isn't used to perform calculations in the laboratory. But, once again, this rests on a narrow, excessively empirical conception of "physics", one certainly not supported by our best philosophy of physics, and so, again, the response has a ring of desperation about it.)

To conclude: the demand to respect scientifically applicable mathematics has important consequences tending to rule against the adequacy of Brouwer and Bishop constructivism. Classical, non-constructive concepts and reasoning would seem indispensable to some of our best science. However, as indicated in the first section, none of the objections to constructivism reviewed here tells against *predicativism*, or at least not without further argument. Whether, indeed, there are *any* serious limitations to predicativist mathematics' power to recover scientifically applicable mathematics remains an important open question in this field. Friedman's examples are suggestive here, but they do not fall within actually applied mathematics. The avenue of non-separable spaces may prove more promising, but that remains to be seen.

Notes

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[2]This emerged in comments on the author's presentation at a symposium of the IXth International Congress of Logic, Methodology, and Philosophy of Science, August 12, 1991, Uppsala, Sweden.

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