Confirmational holism and its mathematical (w)holes

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Abstract

I critically examine confirmational holism as it pertains to the indispensability arguments for mathematical Platonism. I employ a distinction between pure and applied mathematics that grows out of the often overlooked symbiotic relationship between mathematics and science. I argue that this distinction undercuts the notion that (pure) mathematical theories fall under the holistic scope of the confirmation of our scientific theories.

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1. Indispensability and confirmational holism

The indispensability approach to mathematical ontology is based on the indispensability argument for mathematical realism, which in its current form derives from Quine (1980a [1948], pp. 13–18; 1980b [1951], pp. 44–45; 1976 [1960], p. 121; 1981 [1978], pp. 149–150) and Putnam (1979a [1971], pp. 345–356; 1979b [1975], pp. 72–75). The argument holds that since many scientific theories cannot even be stated without mathematics, and since traditional mathematics makes reference to abstract entities, the reference must be taken seriously: abstract mathematical entities exist. The rough idea is that since we take at face value the existence claims of our best scientific theories when they are about unobservables, such as electrons and genes, we should also take at face value the existence claims of these same theories when they are about unobservable mathematical objects. Thus mathematical theories are confirmed as a part of the whole (confirmed) scientific theory. Built into this account is the notion that it is our whole theory of the world that enjoys empirical confirmation: confirmation cannot be differentially apportioned among the various statements and/or sub-theories that make it up. This is the doctrine of confirmational holism.

An important feature of Quine’s indispensability approach is the doctrine of confirmational holism.¹ This is the view that whole theories are the ‘smallest things’ that are confirmed by science; neither individual nor small groups of statements can be confirmed. Quine’s holism rests on his interpretation of Duhem’s 1914 thesis, which

¹ Mark Colyvan’s (2001) defense of indispensability offers a helpful consolidation of Quine’s indispensability reasoning and replies to critiques of it. Surprisingly little is said about confirmational holism, however. See Peressini (2003) for discussion and critique. An important criticism of indispensability that circumvents confirmational holism, overlooked in Peressini (2003), can be found in Jody Azzouni’s (1997) work showing how the quantification over mathematical objects in a scientific theory can be distinguished with respect to existential commitment from the quantification over physical objects.
is generally taken to assert that physical theories have experimental consequences only in conjunction with a set of background assumptions (Duham, 1954 [1914]). As an illustration of Duham’s insight, recall the familiar folk episode from the history of science, the discovery of the planet Neptune. Just before the discovery of Neptune, scientists were using Newton’s universal law of gravitation to predict the positions of the plants. This law (ULG) does not by itself make predictions about anything. It merely relates the force of gravity between any two objects to their masses and the distance between them. If we are interested in predicting the position of, say, the planet Uranus, we must include in our reasoning various assumptions about the whole solar system, including, the number of planets and their relative positions and mass. (This is because Uranus’s orbit will depend on the gravitational forces due to the other planets as well as the gravitational force of the sun.) Duham’s insight is that laws like Newton’s (ULG) do not themselves make predictions; in order to generate predictions, the law must be supplemented with various background assumptions.

An over-simplified schema of this confirmation episode goes as follows: let (L) be the scientific law we are confirming, let (B) be various background assumptions about number, position, and mass of the other planets in the system, and let (P) be the prediction of where Uranus will be at the time of interest. So then we have that

\[ L \land B \Rightarrow P, \]

where ‘⇒’ is understood to be an appropriate sense of entailment for understanding scientific deduction. Duham’s point is that whenever we have a law (theory) making a prediction, there must be some other ‘conjuncts’; we never have something like \( L \Rightarrow P \).

The importance of this observation for confirmational holism can be seen in cases in which the predictions are not borne out. Suppose that (as happened) Uranus fails to be where statement P predicted. What then do we conclude? Are we to reject Newton’s law of gravitation? This is not what happened. It was the background assumptions, B, that were questioned. Eventually Adams and Leverrier independently realized, using Newton’s laws, that another planet beyond Uranus would explain the error in the prediction. Neptune was actually discovered by looking for this hypothetical eighth planet.

This example illustrates the point that recalcitrant data do not force one to give up the statement that one is testing. One may just as readily give up some of the background assumptions that, in conjunction with the statement, entail the prediction. Quine leaps from this quite plausible point to confirmational holism as follows:

The unit of empirical significance is the whole of science... Any statement may be held true come what may, if we wish to make drastic enough adjustments... Conversely, by the same token, no statement is immune to revision. (Quine, 1980b [1951], p. 42)

Quine’s reasoning seems to be the following: (1) any statement that is susceptible to revision in a given testing situation is confirmed if the prediction comes true, and (2) no statement is immune from revision in a testing situation, thus (3) it is the whole of the theory that is being confirmed. As we will see, this radical holism is difficult to defend.

1.1. Problems with global confirmational holism

It has been recognized (Krips, 1982; Sober, 1993; Maddy, 1992, 1997; Gibson, 1998; Creath, 1991), most notably by Quine himself (1976 [1960], p. 13; 1975, p. 314; 1986, p. 427), that global holism, which takes the whole of science as the unit of confirmation, is rather untenable. Quine (1991, p. 268) had gone on to maintain that it is ‘largish’ blocks of theory that are confirmed as wholes—not necessarily the totality of our beliefs. This more restrained or localized version of holism is considerably more plausible. It gives due weight to the Duhamian insight that, by themselves, neither statements nor laws, nor even theories are ‘thick’ enough to entail observational consequences that may be checked empirically. So with respect to our former example, we might recognize gravitational theory, celestial mechanics, optics, and, of course, mathematics as all being complicitous in our predictions about where we should see bright spots in the night sky. It is much more reasonable to restrict the scope of the confirmation in this episode to these theories and leave, say, immunology and plate tectonics out of it.

I now consider two recent attempts to defend a holism robust enough to underwrite the empirical confirmation of mathematics.

1.2. Resnik’s pragmatic holism

Resnik (1997) elaborates and defends a version of confirmational holism that is Quinean right down to its pragmatic underpinning. He takes the Quine–Duham insight as recognizing that the statements of science generally imply observational claims only in conjunction with certain other ‘auxiliary’ hypotheses. He couples this insight with the ‘simple point of logic’ that

if a hypothesis \( H \) only implies an observational claim \( O \) when conjoined with auxiliary assumptions \( A \), then we cannot deductively infer the falsity of \( H \) from that of \( O \) but only that of the conjunction of \( H \) and \( A \). Furthermore, if we subscribe to a confirmation theory, on which a set of hypotheses is confirmed by its true observational consequences, then the truth of \( O \) confirms not \( H \) but rather \( H \land A \). (Ibid., p. 115)

Resnik addresses head-on the standard criticism (e.g. Parsons 1986; Chihara, 1990; Maddy, 1992; Sober, 1993) that holism does not square with how science and mathematics actually functions—that in particular, mathematicians do not look to the benefits that will accrue to science to help determine what axioms to add to set theory, nor do scientific
tests put the claims of mathematics at risk, and finally that mathematics has its own internal kinds of mathematical evidence that operate independently of science. He denies, however, that such methodological arguments refute holism.

According to Resnik, a holist may grant the rationality of holding certain auxiliary assumptions as fixed and thus that it is rational

for them to act as if the evidence they obtain bears upon the specific hypotheses being tested. Holists simply deny that, independently of holding the ‘auxiliaries’ fixed, a logical (or a priori) relationship obtains between the hypotheses tested and the evidence. As Duhem put it, ‘these reasons of good sense [for favoring certain hypotheses] do not impose themselves with the same implacable rigor that the prescriptions of logic do’. (Resnik, 1997, pp. 119–120, quoting Duhem 1954 [1914], pp. 217–218)

Thus Resnik holds fast to radical holism, maintaining that the Quine–Duhem point quoted above transfers the burden of proof to those who deny holism. I will call them separatists. Unless they can refute Duhem’s point of logic or his observation concerning theoretical hypotheses, they must show that the relations obtaining between specific statements and sensory experience that holists attribute to ‘good sense’ are backed by evidential relations holding independently of our judgements of ‘good sense’.

He explains away the above anti-holistic methodological observations by showing that ‘good sense’, in the form of pragmatic rationality, underwrites the special role mathematics has come to play in and science bids us to treat it as if it were known a priori. This will undercut the methodologically grounded arguments in favor of the apriority of mathematics. (Ibid., p. 120)

Resnik’s account is extreme in that he denies that confirmation can be non-arbitrarily apportioned differentially among any of the statements that go into the deduction of the empirical consequence. It is this extreme feature of Resnik’s holism that Hellman rejects.

1.3. Hellman’s moderate holism

Hellman (1999) argues that there is indeed a principled way to differentially apportion confirmation among the statements of a theory—a Bayesian way. He develops this from a generally holistic perspective that allows mathematical background theory to enjoy a ‘trickle’ of empirical confirmation as a result of its role in scientific theory. Hellman characterizes his moderate holism as follows:

(H1) In a typical (reconstructed) pattern of scientific reasoning, in which empirical consequences $E$ are deduced from lawlike statements $L$ of a scientific theory $T$, substantial auxiliary statements $A$ are also assumed, and purely mathematical axioms $M$ are presupposed, i.e., the pattern is $M, L, A | - E$.

Furthermore, when $E$ is borne out, typically there is some degree of confirmation or support of the conjunction of all the assumptions used in the deduction of $E$.

For radical holism one holds that in addition to (H1):

(H2) There is no non-arbitrary way of apportioning the support of $M \& L \& A$ by $E$ differentially among the different components. (Ibid., p. 29)

On his representation, Hellman is loading the import of the Quine–Duhem insight into the auxiliary statements $A$; as he puts it, these statements consist of ‘initial and boundary conditions, assumptions on the functioning of instruments, and background theory’ (ibid.).

As Hellman points out, in the Bayesian framework that he is provisionally using, (H1) does typically hold, but (H2) does not, since ‘it is of the essence of Bayesian confirmation to apportion support differentially’ (ibid., pp. 29–30). Hellman goes on to defend (Bayesian) mechanisms whereby confirmation of the conjunction may be passed on to the conjuncts ($M$ in particular) without falling into the known pitfalls. And while his account is not without its problems, Hellman does succeed in showing how, in a Bayesian framework, sense can be made of the claim that mathematical claims are empirically confirmed as a result of their presence in our confirmed scientific theories.

In the paper under consideration, Hellman is primarily interested in exploring how much and the extent to which the mathematics used by science is in fact indispensable. Much of the discussion surrounding the indispensability approach has focused on just this. An equally important question is the nature of the relationship by which a mathematical theory finds itself present in empirical theory. This is the process of application. Attention to this process

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1. This is because $\Pr(M \& L \& A \vert E)$ typically exceeds $\Pr(M \& L \& A)$. Notice too that on Hellman’s representation of the confirmation episode, the whole scientific theory itself remains out of the confirmational fray. Perhaps he intends $T$ to be the whole of our theory of the world, which I take it is a way of emphasizing the moderate character of his holism.

2. For example, even dispensable mathematical theories also receive incremental confirmation—just not as much as the ‘weakest’ version; also, this solution requires that dispensable (stronger than necessary) theories be ‘initially less secure’ and the problem of priors rears its head with a mathematical/axiomatic vengeance. Notice too that the assumption that gets the incremental support off the ground is that $M$ is less than certain, i.e. $\Pr(M) < 1$ but this requires that one be prepared to accept that an axiom has a determinate truth value, which is uncomfortably close to the issue at hand.

3. Some discussion of the process of application in the context of indispensability can be found in Resnik (1992), Steiner (1995), and Peressini (1997). Also, Hellman (1999) briefly describes how he understands application from the modal-structuralist perspective, which if formalized, would closely correspond to the ‘mathematized scientific theory’ I develop below.
requires us first to consider the distinction between pure mathematical theory and its application in mathematized scientific theories.

2. Applying mathematics

There is an obvious and natural distinction between mathematical theories and mathematical scientific theories. A mathematical theory is a theory whose (apparent) subject matter is some sort of mathematical object; for example, number theory, real analysis, functional analysis, group theory, set theory, and so on. These are the theories that occupy mathematicians; I will refer to them as pure mathematical theories. On the other hand there are scientific theories which, to varying degrees, make use of pure mathematical theories, that is, mathematical (or mathematized) scientific theories. Examples of mathematical scientific theories are found in virtually all areas of science, from quantum mechanics to population genetics. I note that the applications of mathematics mentioned so far have been physical applications. Physical application of mathematics should not be conflated with another sort that will come up below; namely, an application of pure mathematics to other pure mathematics.

More precisely, the distinction between pure mathematical theories and mathematical scientific theories is underwritten by the latter’s deployment of a physical interpretation of part of the mathematical vocabulary that mathematical theories lack. At least some of the sets, numbers, functions, vectors, groups, and operators of scientific theories have an associated (operationally defined) physical interpretation evinced by their units; in this sense the mathematics present in the theory is applied. Mathematical theories, on the other hand, are pure in that they lack this physical interpretation. The sets, numbers, functions, vectors, groups, and operators that appear in the physical theory are interpreted as physical properties such as spatial location, rotation, mass, momentum, velocity, expected number of offspring, fitness, mutation rate, orbital energy, valence number, and so on. The propositions of pure group theory, on other hand, lack any such physical interpretation. It is quite clear in virtue of the physical interpretation that the (pure) mathematical terminology of the pure theory is not synonymous to its counterparts in the mathematized scientific theory, despite the fact that they share sufficient structural properties to underwrite the application. Scientific statements concerning the group of symmetries of the water molecule are not making the same claim as a corresponding statement regarding the pure group \( \mathbb{Z}_2 \). From the perspective of empirical confirmation, this physical interpretation that distinguishes the pure theory from the pure theory is anything but trivial, as I will illustrate in the context of a particular application below.

Before examining the details of a physical application of pure mathematics, consider first how the pure mathematical theory may be applied within pure mathematics itself. From pure group theory we have:

\[ (\text{GT}) \quad (\forall a \in G)(\exists b \in G)[a \cdot b = e] \]

where \( G \) is a group. This states that for any element in a group, there is another element in the group such that the group product of the elements is the identity element. As such, this proposition has implications for particular pure groups, for example, \( \mathbb{Z}_2 \), integers modulo 2; or \( P_3 \), the permutation group for 3 elements; or \( SU(2) \), the group of \( 2 \times 2 \) unitary matrices with determinant equal to 1. In particular, the general pure proposition (GT) gives rise to the particular pure proposition (GT\(_{\mathbb{Z}_2}\)) in which the free variable \( G \) is replaced by the particular pure group \( \mathbb{Z}_2 \). Formally speaking, (GT\(_{\mathbb{Z}_2}\)) follows from (GT) and the additional premise

\[ (\text{AP}) \quad \mathbb{Z}_2 \text{ is a group.} \]

The relationship between (GT) and (GT\(_{\mathbb{Z}_2}\)) is purely mathematical and a priori in that (AP) is purely mathematical and a priori; this is true of pure applications in general and is in marked contrast to physical applications of pure mathematics.

As a simple but illustrative example of a physical application, consider physical chemistry (the molecular orbital theory of chemical bonding) and group theory. In the analysis of molecular bonding, the physical symmetry of molecules is described by symmetry elements defined in terms of the operations of rotations; these rotations are described, analyzed, and related to other physical properties by group theory. In particular, the symmetry of an \( \text{H}_2\text{O} \) molecule is characterized as the \( C_{2v} \) symmetry, which involves one rotation operation of 180° in which the mirror plane contains the main rotation axis.\(^5\) It turns out that the \( C_{2v} \) symmetry is modeled by the pure group \( \mathbb{Z}_2 \). Thus \( \mathbb{Z}_2 \) is used along with (some of) the pure propositions concerning it, which follow from the general pure theory in the sense just described. In the physical application, however, the members of \( \mathbb{Z}_2 \) are further interpreted as (components of) the physical property of rotation and the group operation is taken to correspond to the composition of rotations, thus allowing the mathematical terminology to fall under the physical principles which relate the property of rotation to the rest of the physical theory. This physical interpretation of (part of) the pure theory is such that the resulting applied mathematical propositions of the physical theory imply claims about the physical world. Let the applied group used in physical chemistry be denoted by \( C_{2v}(\mathbb{Z}_2) \). The crucial point here is that even though the physical theory propositions concerning \( C_{2v}(\mathbb{Z}_2) \) constitute an application of the pure theory, it is a qualitatively different kind from the pure application.

\(^5\) For the details of this application of group theory in chemistry, see Molloy (2004).
Let \( p \) be a proposition of the pure theory and \( p' \) the corresponding proposition in the physical theory. Proposition \( p' \) does not follow from \( p \) (and its attendant theory) conjoined with a pure mathematical premise like (AP); this is because the physical application requires empirical bridge principles to underwrite the physical interpretation. These principles distinguish pure mathematics from mathematized physical theory and enable claims about the physical world to be deduced from the latter. That the composition of two particular elements, \( a \) and \( b \in \mathbb{Z}_2 \), yields the identity element is not related in any obvious way to the empirical fact that inducing two particular rotations on a molecule will leave the molecule in the original physical position/state. The latter is not merely a special case of the former; it is not merely a case of going from the universal to the particular. Any relationship between the two propositions involves substantive empirical bridge principles linking the pure mathematical vocabulary to the physical object/property vocabulary. It is only within the physical theory itself, which contains propositions corresponding to at least some of those of the pure theory, that certain of its propositions imply that physical rotations \( a' \) and \( b' \in C_2(\mathbb{Z}_2) \) performed on a molecule will leave its position/state unchanged.

Another way of characterizing the difference between mathematical and physical applications of pure mathematics focuses on the ‘immediacy’ of the application. Pure mathematical propositions apply ‘immediately’ to other pure mathematical settings: the implicit range of the free variable, \( G \), in (GT) are all pure groups (e.g. \( \mathbb{Z}_2 \), \( P_3 \), and \( SU(2) \)). In this sense the application is immediate relative to physical applications. Physical applications are substantially less immediate in that they require substantive auxiliary premises which take the form of empirical bridge principles between the language of pure mathematics and the language of physical theory; it is only by way of such auxiliary premises that the mathematics of the physical theory says anything about the physical world.

It is possible to render formal treatments of application in such a way that both pure and physical applications will include a premise of the form ‘\( X \) is a group’, where the ‘only’ difference is that in physical applications this premise is a physical claim rather than a mathematical claim. While this formal similarity allows for a succinct abbreviation of the details of an application, it may also be misleading. In the case of pure application this ‘premise’ is little more than a ‘formality’, since the free variables of the pure theory range over the appropriate pure mathematical ‘objects’. In physical applications, however, this ‘premise’ is much more complicated; at the very least it encodes the substantive empirical bridge premises required for the pure theory to have physical implications. This abbreviated way of representing the application may misleadingly suggest that the physical application of mathematics is essentially the same as pure application—that it involves nothing more than replacing some mathematical terminology with physical terminology. As I think is clear, this does not characterize genuine physical application of mathematics; rather, it describes a trivial change in notation.\(^6\)

### 2.1. The pure/applied relationship

Suppose that theories \( s \) and \( m \) bear the ‘applied’ relationship to one another, that is, that \( s \) is an applied version of \( m \) (\( s \text{\texttt{R}} m \)). One naturally wonders whether, in general, it is necessary that a pure theory \( m \) be worked out before we can have an applied theory \( s \). If one were to form a naive opinion based only on pure mathematics texts, then it would appear that pure mathematical theories are worked out by elegantly deducing consequences from various mathematical postulates—a triumph of a priori reasoning. Then, only after the pure theory has been worked out, would it be applied to real problems. But this picture is seriously distorted. Historically this is rarely how theories develop, and even today, mathematics texts offer little insight into how pure mathematical research proceeds. Nor would it be right to suppose that progress in pure mathematics is always due to developments in the scientific use of mathematics. As it turns out, neither the pure theory nor the applied theory is in all cases epistemically prior. I illustrate this below by considering several historical episodes in the development of science and mathematics. Before doing this, however, I consider further the formal character of the applied relationship.

As stated above, \( s \text{\texttt{R}} m \) is a two-place relationship, where \( m \) is a pure mathematical theory and \( s \) is an applied mathematical theory or mathematized scientific theory. Until now, I have been taking ‘applied mathematical theories’ and ‘mathematized scientific theories’ to be the same thing; however, they must be distinguished. Because not every mathematized scientific theory is also an application of a (pure) mathematical theory. There are mathematized scientific theories that do not bear the ‘applied’ relationship to any pure mathematical theory, and so, strictly speaking, should not be considered applied mathematical theories. In these examples a scientist develops a technique in order to solve certain physical problems by what appear to be mathematical methods (e.g. evaluating a certain type of integral, multiplying an integrand by a certain ‘function’,

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\(^6\) While bridge principles are always operant in some form or another in an application of pure mathematics, they are more and less prominent and intricate, depending on the nature of the application. In arithmetic (number of apples on the table) and algebraic applications (the possible permutations of players in a bridge tournament), the bridge principles are generally simpler, more immediate, and involve less idealization than other sorts of applications. It is easier in such settings to miss the significance of the auxiliary bridge premises. Steiner (1978, p. 24 ff.) explicitly fleshes out such auxiliary bridge premises for the application of simple arithmetic.
dividing by a certain mysteriously small quantity, etc.). But in fact the new 'mathematical' method makes no sense mathematically, and hence is not a physically interpreted version of a pure theory.

In such cases in which the mathematized scientific theory is worked out first, and then only later, if ever, a pure mathematical theory is worked out, we have the inverse of the operation of application—call it abstraction. The history of science and mathematics abounds with examples going in each direction, as we will see in the next section.

As discussed above, pure mathematical theory is often applied within pure mathematics itself. Analytic number theory is a prominent example. Number theory deals with integers, while analysis deals with continuous sets of numbers such as the reals or their extension, the complex numbers. In pure analytic number theory, methods and results from the pure theory of complex analysis are used to express and prove facts about the integers. The area of mathematics known as numerical analysis provides a wealth of other such examples. Numerical analysis deals with computation; it focuses on computational means of obtaining numerical results for mathematical expressions. For a mundane example, the methods by which your hand-held calculator computes its square roots, sines, and cosines are not what you might expect; the highly theoretical tools of the theory of numerical analysis have been employed to design accurate and efficient algorithms to compute these functions. Numerical methods ingeniously and indirectly arrive at the actual numbers used to approximate the analytic solution of a physical problem. Although this (pure) application of pure mathematics is rarely seen in textbooks or classrooms, it is essential to prediction and confirmation.

2.2. Historical discussion

Instances of applied mathematical theories can be loosely categorized by whether they are (primarily) examples of (1) moving from a mathematized scientific theory to a pure mathematical theory (abstraction), or conversely, (2) moving from a pure mathematical theory to a mathematized scientific theory (application). Of course these 'directions' are only approximations; history, as usual, resists such neat categorization.

Only relatively late in the history of mathematics do we begin to see clear examples of the application of pure theories; this is because it was late in the history of mathematics that mathematical theories reached the level of abstraction that we have today. Early mathematical breakthroughs often took place in theoretical environments in which the mathematics was not clearly divorced from the physical problems it was developed to solve. The case of Euclidean geometry is a well known example of the transition from mathematized scientific theory to pure mathematical theory. Initially (Euclidean) geometry was taken to be about physical space itself; proving geometrical theorems amounted to deducing facts about physical space. The development of non-Euclidean geometries, however, forced people to revise these views. These internally consistent alternatives to Euclidean geometry were considered to be on the same mathematical footing as Euclid's geometry. If Euclidean geometry is in some sense 'more true', it would have to be so in a non-mathematical sense, that is, true of the physical world.

Newton's work on the calculus is another example. In his second and preferred presentation of the Methodus fluxionum et serierum infinitarum, Newton presents his version of the derivative (fluxion) in dynamical terms—based on the idea of rate of change with respect to time. In response to Berkeley's criticism of infinitesimals quantities, Maclaurin's authoritative presentation of Newton's calculus sought to base this calculus on our intuitions of space, motion, velocity, and time. As Newton himself wrote, the theorems of the calculus do not deal with 'fictions' or 'ghosts of departed quantities', but rather with things that have an 'existence in nature' (Guicciardini 1989, p. 51).

Finally, Steiner (1992) gives examples of mathematical devices used by present-day physicists that still lack a consistent pure mathematical underpinning. Quantum field theory makes use of an integral called the Feynman integral, which unlike any integral in pure mathematics, is taken over an infinite dimensional space. As Steiner demonstrates, a general pure theory of such an integral has not yet been worked out. Another such device used in quantum electrodynamics is called 'renormalization' (see ibid., pp. 164 ff., for details). In these examples, the mathematics-like devices employed by the scientific theory are motivated by physical considerations—they make sense given the physical interpretation. Equally important, these techniques accurately describe and predict the physical phenomena. What lacks, however, is a corresponding pure theory in which the techniques make sense. In the context of pure mathematical theories, these techniques make as much sense as 'dividing by zero'. If and when these techniques are given a pure mathematical foundation, the move from mathematized scientific theory to pure mathematical theory will be complete.

Consider now the opposite direction, moving from pure mathematical theory to mathematized scientific theory. Recently striking examples of scientists making use of previously developed pure mathematical theories to formulate their scientific theories have arisen. As Steven Weinberg puts it:

For example, the calculus had no genuine grounding in pure mathematics until Cauchy's work in the nineteenth century. Dirac's delta function is another such example. And to this day, Feynman path integrals in quantum field theory are without a pure mathematical foundation. See Peressini (1997), pp. 914–916, for discussion and additional references.
The mathematical structures that arise in the laws of nature . . . are often mathematical structures that were provided for us by mathematicians long before any thought of physical application arose. It is positively spooky how the physicists finds the mathematician has been there before him or her. (Cormack, Hauptman, & Weinberg, 1986)

The development of Einstein’s general theory of relativity is a case in point. His theory identifies the effects of gravity with structural features of a curved space-time (Riemannian geometry). Einstein, however, unlike Newton, did not need to invent the mathematics to go along with his physical insight. The calculus of four-dimensional Riemannian manifolds requires a special calculus of tensors (tensor analysis), which had been developed years earlier by Ricci and Levi-Civita, but had not yet been noticed by physicists. Einstein studied these results and used them as a basis for formulating his general theory of relativity. In just about the same way, the pure mathematical theory of Lie algebras was discovered by the physicist Murray Gell-Mann as just what he needed to describe the unitary spin properties of elementary particles. Yet another example can be found in abstract algebra. Abstract group theory grew out of the work of Evariste Galois on the solution of polynomial equations by radicals. Much later physicists discovered it as the mathematics needed for describing the symmetries of elementary particles and incorporated it into the physical theory of symmetries.

It must be stressed that the distinction between pure and applied mathematics is a logical distinction; we should not expect to be able definitively to place work done in the actual development of mathematics and science precisely into one of these two categories. I offered the development of the calculus from Newton to the present as an example of the epistemic process of moving from mathematized science to pure mathematics. Does this mean that Newton was doing only applied mathematics? Not exactly. The theory of the calculus, as Newton left it, certainly was not a pure mathematical theory as Quine suggested. Again, this is because there is a принципled way in which the pure mathematical theory is ‘insulated’ from the mathematized scientific theory: it is only the mathematized scientific theory that must be part of the ‘deduction’ of empirical consequences—the pure theory need not be. And in fact, as I argued above, the pure theory should not be, since the relationship between pure mathematical theory and mathematized scientific theory is nothing like the straightforward deductive one his account assumes.

I need to be especially clear here on precisely how the pure applied distinction militates against the Quine/Putnam Indispensability picture. I am not claiming that math’s role in science is less than one might have thought, nor am I suggesting a sort of dispensability move Harty Field has tried to make. (I actually think Resnik’s and other criticism of Field is on target.) Mathematized scientific theory cannot be unmathematized: the applied mathematics in much of science is indispensable in just this sense. My thrust here is that as far as the empirical confirmation of theory in science goes, the confirmation of mathematized scientific theory is NOT the same as (nor does it entail in any obvious way) the confirmation of the corresponding pure mathematical theory. ⁸ ⁹

3. Mathematical (w)holes

So what is to be said of the (holistic) empirical confirmation of pure mathematics? The account of the involvement of mathematics in scientific theory sketched above takes a toll on the plausibility of the holistic confirmation story. In particular, it militates against the simple inclusion of pure mathematical theory into the class of auxiliary assumptions—however exactly pure mathematical theory does participate, it is not simply (and wholly!) conjoined to the scientific theory only to function as another premise in the deduction of empirical consequences. The reason for this is that mathematized science is distinct from pure mathematical theory, as can be seen from the fact that science can (and has and does continue to) carry on its mathematized theorizing, predicting, testing, and confirming regardless of there being a pure mathematical theory of which it is an application. ⁸ I now weave my way back through the accounts of Quine, Resnik, and Hellman to consider briefly how this distinction complicates life for each of them.

3.1. Contra later Quine

With the notion of a pure mathematical theory developed above, Quine’s later localized version of holism would not seem up to the task of supporting the confirmation of pure mathematical theories merely as result of being applied in scientific theory. That is, the ordinary everyday work of mathematics in science does not itself force on us empirical confirmation of the pure mathematical theory as Quine suggested. Again, this is because there is a principled way in which the pure mathematical theory is ‘insulated’ from the mathematized scientific theory: it is only the mathematized scientific theory that must be part of the ‘deduction’ of empirical consequences—the pure theory need not be. And in fact, as I argued above, the pure theory should not be, since the relationship between pure mathematical theory and mathematized scientific theory is nothing like the straightforward deductive one his account assumes.

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3.2. Contra Resnik’s pragmatic holism

While Resnik’s program is rich and detailed enough to examine on a number of different points, I will limit my

⁸ And occasionally even when there is a pure theory that is applied, the application may be quite distantly related in that only some of its rules or structure are applied.

⁹ See Peressini (1999) for a positive account of how pure mathematical theory is confirmed.
focus to the ones most immediately related to the pure/applied distinction.\textsuperscript{10}

Implicit in Resnik’s take on Duhem is the assumption that there are only two possibilities for grounding a non-holistic account of confirmation. The first possibility is that the basis is purely logical, but of course the Quine–Duhem thesis (or elementary logic, really) conclusively rules this out. It would seem then, according to Resnik, that the only other possible basis is purely pragmatic (simplicity, precision, fertility, etc.). I use the term ‘purely’ because I take it that what Resnik has in mind with his pragmatic ‘good sense’ is a prudential basis, that is, one that is based in utility and is in marked contrast to evidential or truth-indicating considerations. This dichotomy seems to omit an important intermediate possibility, namely, that there is defeasible evidential (not purely logical) ground for non-holistically distributing confirmation among the various statements involved in the deduction of the observation. Fellow holist Hellman, in the work considered above, has done the honors of showing this to be a false dilemma by working out the beginnings of a possible way in which confirmation can be differentially apportioned in a non-arbitrary way.

The effect of the more complicated picture of applied mathematics developed above is, again, that it renders problematic the simple deductivist picture of the the pure mathematical theory that gives rise to it. I will argue that, on the contrary, consideration of the process of application sketched in Section 2 gives us reason to think that pure mathematics is not holistically confirmed by its role in scientific theory—even if we do think that scientific theory itself is holistically confirmed.

Since Hellman’s trickle account has the virtue of actually being fleshed out in some detail, I will cast the pure/applied account in terms of (something like) his notation conclusively rules this out. It would seem then, according to Resnik, that the only other possible basis is purely pragmatic (simplicity, precision, fertility, etc.). I use the term ‘purely’ because I take it that what Resnik has in mind with his pragmatic ‘good sense’ is a prudential basis, that is, one that is based in utility and is in marked contrast to evidential or truth-indicating considerations. This dichotomy seems to omit an important intermediate possibility, namely, that there is defeasible evidential (not purely logical) ground for non-holistically distributing confirmation among the various statements involved in the deduction of the observation. Fellow holist Hellman, in the work considered above, has done the honors of showing this to be a false dilemma by working out the beginnings of a possible way in which confirmation can be differentially apportioned in a non-arbitrary way.

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Since Hellman’s trickle account has the virtue of actually being fleshed out in some detail, I will cast the pure/applied account in terms of (something like) his notation in order to draw out the differences. Let $T_m$ be a mathematized scientific theory and $M_p$ the pure mathematical theory(ies) of which parts are applied in $T_m$ (Note that $M_p$ may be empty if $T_m$ is not an application of any pure theory.) A typical confirmation scheme may then be represented as

$$L_m, A | = E,$$

where as before the statements of $A$ are the auxiliary statements and $L_m$ the parts of $T_m$ that are part of the deduction of $E$. Again, the statements on the left of the turnstile are all and only those parts of the ‘theory’ that are used in the deduction of $E$. The significant difference, of course, is that in this rendition there are no pure mathematical propositions involved in the deduction of $E$.\textsuperscript{11} Another component of the account that bears mentioning is the relationship between $T_m$ and $M_p$.

As discussed above, the process of applying $M_p$ to give rise to $T_m$ involves the use of bridge principles linking its mathematical structure to a physical/scientific vocabulary that is ultimately tied to empirical consequences by the auxiliary statements of $A$. This ‘application step’ typically gets expressed as the assumption that the actual system (that is the object of $T_m$) exhibits—approximately—the pure relationship of pure mathematical theory to scientific theory. If the complicated picture is right, then it is just plain wrong that pure mathematical theory is as inextricably conjoined to the scientific amalgam as any other statements. There is a non-arbitrary and not merely pragmatic grounding of the ‘separatist’ character of pure mathematical theory.

It is not clear how to approach Resnik on this. Should one offer him a pragmatic argument? His position is poetically Duhemian: faced with the wealth of recalcitrant data in the methodology of science and mathematics, which bears no resemblance at all to anything holistic, he holds fast to his holism. He does this by ‘adjusting’ auxiliary hypotheses relating his holism to the methodological data so that it is consistent with it. Thus he maintains that it only appears as if observations bear on specific statements. What should one say in response to a move like this? I presume one ought to decline the implicit invitation to argue along pragmatic lines and insist instead that the ‘data’ has not been accounted for—that his ‘appears as if’ account does not satisfactorily deal with the methodological objection because it does not take it at ‘face value’.

3.3. Contra Hellman’s moderate holism

I note first that while Hellman’s trickle discussion may have shown that there is a cogent Bayesian mechanism for the confirmation of a particular pure mathematical axiom by science, or as he puts it, a mechanism for ‘differential apportionment of confirmation’, this in itself does not give us reason to think that the holistic confirmation of mathematized scientific theory in turn (holistically) confirms mathematical structure $M_p$. It is this auxiliary application assumption that is directly confirmed by the empirical success of the scientific theory.

\textsuperscript{10} Resnik’s model of confirmation, which is the Hypothetico-Deductive model, is problematic in itself. As fellow defender of holism and indispensability Geoffrey Hellman writes, ‘if positive indispensability arguments turned on excessive holism which denies differential apportionment of blame or credit in theory testing, such arguments could be dismissed from the start as resting on a thoroughly inadequate view of confirmation’ (1999, p. 29). Finally, Resnik’s resorting to an ultimately pragmatic (as opposed to evidential) account of the operation of science can only lead to (at best) a ‘thin blooded’ realism.

\textsuperscript{11} Presumably Hellman takes a mathematical theory to be the deductive closure of a set of axioms. In general (for a logician type) this may make perfect sense. In fact, it seems to be the unreflective standard for how to think of mathematical ‘theories’. I have argued elsewhere that this is a less than benign assumption that should be questioned—especially in indispensability discussions. In my treatment here, I will follow his lead for pure mathematical theories, but as should be clear, mathematized scientific theories need not be anything like a deductive closure of a set of axioms, and will not be assumed to be.
Interestingly, for holists, this application assumption inevitably plays an important role. Holists, who embrace the confirmation of mathematics as a result of its participation in successful scientific theory, need to be able to explain another methodological reality, namely, that pure mathematical theories are not disconfirmed as a result of their participation in unsuccessful scientific theories. This counterintuitive asymmetry is typically explained by pointing to the application assumption and suggesting that disconfirmation stops there, but, of course, confirmation is passed through to the pure theory. So on the holist picture, the application assumption functions like a firewall on a network: it lets certain (confirmational) packets pass through to the pure theory, but absorbs and thereby stops other (disconfirmational) packets. The separatist account of confirmation has no such asymmetry to worry about; confirmation and disconfirmation both stop at the application assumption.

As we saw above, the holism of Quine and Resnik makes use of the oversimplified understanding of application that sees mathematized scientific theory as a relatively simple logical consequence of the pure theory and some unproblematic assumptions about the physical system. Hellman’s moderate holism, however, uses entailment in the other direction: the (confirmed) mathematized scientific theory (T_m) itself entails an axiom of the pure mathematical theory (M_p). The (only) example Hellman (1999) offers is that of the axiom of infinity (AI); an infinite set exists or is logically possible. Indeed, if T_m contains something like the assumption that space-time is densely ordered, then T_m ⊨ AI, and so we have that the support gained by T_m is passed on to AI in virtue of the weak monotonicity of the probability function, Pr.

In this case, the axiom of the pure mathematical theory (AI) is confirmed not directly by its entailing T_m (i.e. not because (AI) is applied in T_m); rather it is confirmed indirectly because it is entailed by the mathematized scientific theory, T_m. Thus it is not in virtue of the axiom’s role in grounding the mathematized scientific theory that it is confirmed. In fact, (AI) would have been confirmed in just this way even if it didn’t play a foundational role in the pure math that underwrites the mathematized scientific theory, since its confirmation is simply a logical consequence of the mathematized scientific theory. Thus in the end, Hellman’s particular instance of the confirmation of AI is less a result of his moderate holism and its holistic confirmational dynamics than it is a consequence of his Bayesian confirmation scheme and its limited preservation of the Special Consequence Condition. What is more, this sort of confirmation does nothing to implicate pure mathematical theory as a whole in the confirmation of the mathematized scientific theory, but rather only those special few axioms that are logical consequences of the mathematized scientific theory.

It seems then that Hellman’s moderate holism is so moderate as to be difficult to distinguish from a separatist account. There is another way in which Hellman’s moderate holism and the separatist account are not far apart. Hellman’s depiction of a rare (if not singular) instance of the empirical confirmation of an infinitistic existence/possibility axiom lines up with similarly indirect (though more perspicuous from a separatist perspective) empirical confirmation instances in which a pure mathematical claim is empirically (though non-holistically) confirmed. Consider for example an industrious (or unscrupulous) student who cannot figure out how to solve an indefinite integral for his/her Calc 3 course, but recognizes that such an integral is used by physicists to model a certain harmonic motion problem. S/he goes to the lab and starts doing measurements, forms a conjecture about the anti-derivative in question, checks it against further data, and continues to tweak the anti-derivative by testing it in a definite integral until it works with the experimental results. At the end of all this, our industrious engineering student would seem to have at least some reason to believe that s/he has the correct answer to the pure mathematical problem, and what is more, this seems to be an empirical confirmation of his/her conjecture.

In cases like these, the feature that facilitates the confirmation of the pure mathematical statement is explicit and direct auxiliary statements linking the pure mathematical conjecture to the physical theory. What is more, this special auxiliary statement needs to have independent support and be more securely established than the pure mathematical conjecture—this clearly is rarely (if ever) the case in general with respect to the pure mathematics applied in scientific theorizing. Finally, notice that this special auxiliary statement is present (though glossed over because it is so immediate) in Hellman’s account of the confirmation of AI, since in order to see that T_m ⊨ AI, we have to recognize that physical space-time is an instance of the sets talked about in AI.

4. Closing remark

While confirmational holists do recognize the process of application, its complicating implications have not been given a full accounting. As I have argued, when due attention is paid it, we find that the mathematical whole that is a pure mathematical theory exposes a mathematical hole in the holist account.

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12 Resnik calls revising this assumption rather than the pure theory a Euclidean rescue after the episode in the history of science that led to the recognition that mathematics need not be true (or false) simpliciter but rather true (or false) of a physical system. When it was recognized that space was not Euclidean (but Riemannian), the Euclidean theory was not rejected in some general sense, but rather only rejected as the theory of space.

13 See Sober (1993) for more discussion.

14 Hellman (1999), p. 30, nicely explains how this result can be had in his provisional Bayesian framework without embracing the dreaded Special Consequence Condition.
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