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Mathematics and Indispensability[1]

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1. Introduction

Mathematics plays an indispensable role in the explanations that modern science provides of empirical phenomena. From this unexceptionable observation, a controversial philosophical conclusion is sometimes drawn. The claim is advanced that the empirical success of a scientific theory *confirms* the mathematical claims embedded within it. According to this line of thinking, we have reason to believe that mathematical statements are *true*, and that the entities they quantify over *exist*, because mathematics is indispensable to empirical science.

This indispensability argument for mathematical realism gives voice to an attitude towards confirmation elaborated by Quine. Quine's holism—his interpretation of Duhem's thesis—asserts that theories are confirmed only as totalities. A theory makes contact with experience only as a whole, and so it receives confirmation only as a whole. If mathematics is an inextricable part of a physical theory, then the empirical success of the theory confirms the entire theory—mathematics and all.[2,3]

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[2]Here is a characteristic passage from Quine:

> Ordinary interpreted scientific discourse is as irredeemably committed to abstract objects—to nations, species, numbers, functions, sets—as it is to apples and other bodies. All these things figure as values of the variables in our overall system of the world. The numbers and functions contribute just as genuinely to physical theory as do hypothetical particles. (1981, 149–50)

The implied conclusion is that our confidence in the existence of numbers and their ilk should be no more hesitant than our confidence in the existence of "apples and other bodies."

[3]Since this indispensability argument is often attributed to Putnam, it is well to consider one of his formulations of the argument:

Part of the appeal of this argument is that it applies to an issue in the philosophy of mathematics a form of argument that seems quite central to the activity of science as a whole. Why are we entitled to believe that there are genes and quarks? Because positing their existence is indispensable—we cannot explain what we observe without the theories that say that such things exist. Here Platonism in the philosophy of mathematics draws on the same resources that seem to nourish realism with respect to biology and physics.[4]

Indispensability arguments have been challenged by critics of scientific realism. For example, Van Fraassen (1980) and Fine (1984) contend that such arguments are question-begging. An exponent of Van Fraassen's constructive empiricism disavows inference to the best explanation. For Van Fraassen, the only scientific question about mathematics, genetics, and particle physics is whether the theories they contain are *empirically adequate.* To go beyond this judgment and draw some conclusion about *truth* is to hazard opinions that transcend what science demands and evidence can provide. Perhaps numbers, genes, and quarks *are* indispensable posits; but for a constructive empiricist, this is no strong evidence that such things exist.

Van Fraassen's empiricism is a form of agnosticism. He does not assert that numbers, genes, and quarks are useful fictions. Neither does he interpret scientific theories nonliterally; he grants that number theory, genetics, and particle physics quantify over things

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> [Q]uantification over mathematical entities is indispensable for science, both formal and physical; therefore we should accept such quantification; but this commits us to accepting the existence of the mathematical entities in question. This type of argument stems, of course, from Quine, who has for years stressed both the indispensability of quantification over mathematical entities and the intellectual dishonesty of denying the existence of what one daily presupposes. (1971, 347)

In this passage, Putnam makes no mention of *empirical* confirmation. However, I think it is fair to say that Putnam has been widely interpreted as arguing for mathematical realism on the grounds that mathematics is indispensable in *empirical theories that have been empirically successful.*

[4]You don't have to be a realist or a Platonist to think that the indispensability of a mathematical or physical postulate would be strong evidence for its truth. For example, Field (1980) develops his case for nominalism by trying to show that mathematical objects are dispensable.

we cannot observe. Constructive empiricism differs from fictionalism on the one hand and instrumentalism on the other. It enjoins us to suspend judgment about the truth value of statements that are about unobservables.

In the shifting terrain of this debate, at least one fixed point can be discerned. Realists persuaded by indispensability arguments affirm the existence of numbers, genes, and quarks. Van Fraassen's empiricism remains agnostic with respect to all three. The point of agreement is that the posits of mathematics and the posits of biology and physics *stand or fall together*. The mathematical Platonist can take heart from this consensus; even if the existence of numbers is still problematic, it seems no more problematic than the existence of genes or quarks.

If the two positions just described were the only ones possible, there could be no objection to this melding of numbers with genes and quarks. However, the position I call *contrastive empiricism* (Sober 1990a) stands opposed to both realism and to Van Fraassen's empiricism. As it turns out, contrastive empiricism entails that coalescing mathematics with empirical science is highly problematic. I believe that there is an important kernel of truth in abductive arguments for genes and quarks. But no counterpart argument exists for the case of numbers.

Of course, the existence of this third way would be uninteresting, if contrastive empiricism were wholly implausible. However, I will argue that contrastive empiricism captures what makes sense in standard versions of realism and empiricism, while avoiding the excesses of each. This third way is a middle way; it cannot be dismissed out of hand.[5]

[5]Contrastive empiricism is not the only position that sees a big difference between indispensability arguments concerning genes and quarks and indispensability arguments concerning numbers. Musgrave points out that

> [i]f we view [the indispensability argument for natural numbers] from a Popperian perspective, it begins to lose its charm. Imagine that all the evidence that induces scientists to believe (tentatively) in electrons turned out differently. . . . Popperians think this *might* happen to any of the theoretical posits of science. But can we imagine natural numbers going the way of phlogiston, can we imagine evidence piling up to the effect that there are no natural numbers? This must be possible, if the indispensability argument [for numbers] is right and natural numbers are a theoretical posit in the same epistemological boat as electrons.

2. Contrastive Empiricism

How should we represent the idea that a set of observations favors one explanatory hypothesis over another? It is standard scientific practice to understand this idea in terms of the concept of *likelihood.* The likelihood of a hypothesis, H, relative to a set of observations O, is the probability the hypothesis confers on the observations. Don't confuse the likelihood of the hypothesis with its probability; $P(O/H)$ is quite different from $P(H/O)$. The *Likelihood Principle* (Edwards 1972) says that this mathematical idea can be used to characterize the idea of differential support:

> Observation O favors H_1 over H_2 if and only if $P(O/H_1) > P(O/H_2)$.

How is the idea of "indispensability" connected with the Likelihood Principle? When a scientist considers a set of competing hypotheses, and one of them says that the observations were quite probable, while the other hypotheses say that the observations were immensely improbable, it is natural to conclude that only the first hypothesis makes the observations nonmiraculous. The scientist may be inclined to regard the first hypothesis as indispensable. The data discriminate among the competing hypotheses in such a way

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> But surely, if natural numbers do exist, they exist of necessity, in all possible worlds. If so, no empirical evidence concerning the nature of the actual world can tell against them. If so, no empirical evidence can tell in favour of them either. (1986, 90–91)

Musgrave then elaborates this argument within a hypothetico-deductive framework. He points out that if a mathematical theory is not merely logically consistent but also consistent with any (consistent) scientific theory, then adding it to a "scientific theory does not enable us to derive any conclusions about the world that do not follow from the scientific theory alone" (1986, 91).

Although there are important points of affinity between this Popperian formulation and contrastive empiricism, a few differences should be noted. First and most importantly, contrastive empiricism is not a form of hypothetico-deductivism. Second, my argument will be independent of whether we can imagine empirical evidence being developed against the claim that numbers exist. And finally, I do not assume that necessary truths cannot be tested empirically. Both my agreements and my disagreements will be spelled out in what follows.

that only one of them can be viewed as a plausible explanation of the data.

A limiting case of the Likelihood Principle arises when all the probabilities are 1 or 0. If O can be deduced from H_1 (perhaps with the aid of antecedently plausibly background assumptions A), while $\neg O$ can be deduced from H_2 (again with the aid of A), then the truth of O provides the strongest possible reason for favoring H_1 over H_2. H_1 is now indispensable in an especially strong sense.

The Likelihood Principle entails that the degree of support a theory enjoys should be understood relatively, not absolutely. A theory competes with other theories; observations reduce our uncertainty about this competition by discriminating among alternatives. The evidence we have for the theories we accept is evidence that favors those theories *over others.*

The idea that support is a relative matter means that O may favor H_1 over H_2, but may fail to discriminate between H_1 and H_3. The evidence we have for the theories we believe does not favor those theories *over all possible alternatives.* Our evidence is far less powerful, the range of alternatives that we consider far more modest.

If we view the Likelihood Principle as the vehicle by which observations are brought to bear on theories, what sorts of theories can we evaluate in the light of observations? The Likelihood Principle imposes no limitations on the vocabulary that scientific theories may deploy. Theories may be evaluated for their likelihoods whether or not they quantify over unobservables. Van Fraassen (1980, 22), though he cites likelihood with approval, constructs an epistemology that drastically restricts that concept's domain of application. My view is that science aims to evaluate the support of hypotheses; it makes no difference whether those theories are strictly about observables.

According to contrastive empiricism, science attempts to solve discrimination problems. Consider the following triplet of hypotheses:

(X_1) Moriarty (not Jones) committed the murder.
(X_2) Jones (not Moriarty) committed the murder.
(X_3) Moriarty did not commit the murder, although all the evidence will make it appear that he did.

The clues gathered by Holmes may discriminate between X_1 and X_2. But no evidence, gathered by Holmes or by anyone else, will discriminate between X_1 and X_3. Contrastive empiricism views the first discrimination problem, but not the second, as scientifically soluble.

Precisely the same conclusions apply to the following triplet of hypotheses:

(Y_1) Space-time is curved.
(Y_2) Space-time is flat.
(Y_3) Space-time is not curved, although all the evidence will make it appear that it is.

An independently plausible physics allows empirical evidence to be brought to bear on the task of discriminating between Y_1 and Y_2. However, no evidence can solve the problem of discriminating between Y_1 and Y_3.

X_1, I suppose, is a hypothesis that is "strictly about observables," so Van Fraassen's constructive empiricism will have no objection to our forming an opinion about its truth value. Y_1, however, is not "strictly about observables," so Van Fraassen holds that it is not the business of science to form an opinion about its truth value. For Van Fraassen, the epistemic attitude one should take towards a hypothesis depends on what the hypothesis is about.

Contrastive empiricism differs from constructive empiricism in this respect. Contrastive empiricism emphasizes the parallelism between the *X*s and the *Y*s. Observation can separate X_1 from some competing hypotheses, but not from others; the same is true for Y_1. The fact, if it is a fact,[6] that X_1 is strictly about observables, while Y_1 is not, makes no difference, as far as contrastive empiricism is concerned.[7]

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[6]In Sober 1990a I raise some questions about the use Van Fraassen makes of the concept of *aboutness* in his formulation of constructive empiricism.

[7]Numbers, genes, and quarks have in common the fact that our standard theories about each can be contrasted with predictively equivalent alternatives. All of these discrimination problems are insoluble, according to contrastive empiricism. However, it will be argued in what follows that arithmetic nonetheless differs from genetics and particle physics; the difference concerns whether our confidence in the theories we believe rests on their having been tested against predictively *non*equivalent alternatives.

Contrastive empiricism differs from constructive empiricism in that the former does not limit science to the task of assigning truth values to hypotheses that are strictly about observables. What the hypotheses are *about* is irrelevant; what matters is that the competing hypotheses make different claims about what we can observe. Put elliptically, the difference between the two empiricisms is that constructive empiricism focuses on *propositions*, whereas contrastive empiricism focuses on *problems*. The former position says that science can assign truth values only to *propositions* of a particular sort; the latter says that science can solve *problems* only when they have a particular character.[8]

Notice that an observation solves the problem of discriminating between "H_1 is true" and "H_2 is true" if and only if it solves the problem of discriminating between "H_1 is empirically adequate" and "H_2 is empirically adequate." Those who see likelihood as *the* device by which observations give us information about the plausibility of hypotheses will find no difference between these two problems. It follows that Van Fraassen's constructive empiricism is committed to there being some other epistemic guide to evaluating theories besides their likelihoods. Indeed, at least one of Van Fraassen's arguments for constructive empiricism brings an additional consideration out into the open. Van Fraassen (1980, 36) says that "H is true" entails but is not entailed by "H is empirically adequate" (if H is not strictly about observables), which means that the first is always less *probable* than the second. Here Van Fraassen seems to join forces with the Bayesians, who think that the probabilities of hypotheses, and not just their likelihoods, are well-defined and epistemically relevant.

Even though Van Fraassen here embraces a Bayesian idea to defend constructive empiricism, Bayesians should be reluctant to follow him at this point. Logically weaker hypotheses are always more probable, so a resolute focus on maximizing probability in-

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[8]Contrastive empiricism has some affinities with the empiricism of Carnap (1956); I agree with Carnap that there are two types of *questions*. However, contrastive empiricism has no commitment to verificationism; nor do I think that answering an external question is usefully thought of as adopting a language. And surely Carnap was wrong to think that the sentences that answer external questions must have some special *linguistic* characteristic.

evitably leads one to weaker and weaker hypotheses (an idea that Popper has emphasized). In any event, why not say that "*H* is true" does not compete with "*H* is empirically adequate," in which case the difference in probability between them would not matter, even if those probabilities were perfectly well-defined?

Although contrastive empiricism does not limit science to a study of observable phenomena, the concept of observation is nonetheless an important one for that position. If we are to discriminate among explanations by appeal to observations, what should we count as an observation? Certainly, there is no need for the idea that observations are absolutely theory-neutral or that they are incorrigible. In practice, what counts as an observation is relative to the discrimination problem at hand. The statement "The meter reads 9.4" may count as an observation statement in one problem, but may be the very hypothesis under test in another. In the dispute between two theories, an observation is any detectable feature of the environment on which both theories can agree. Instrumentation may be used at will. And it is not to be denied that we vouchsafe the reliability of instrumentation by appeal to theories. When scientists engage in detection, that process ultimately makes contact with the senses they possess. However, instruments extend the reach of the senses, by producing surface phenomena (meter readings, etc.) that are reliable indicators of what we cannot observe directly. Contrastive empiricism requires the concept of *detection*, not that will-o'-the-wisp, direct and theory-neutral observation.

Detection has a purely ontological as well as an epistemic side. If we can detect the states of *X* by discovering the states of *Y*, then *X* and *Y* must be correlated. Correlation is an objective concept; whether two parts of reality are correlated does not depend on anyone's being able to know that they are. But there is more to detectability than correlation and it is here that epistemic considerations become relevant. What is valuable about the states of a measuring device (whether that device is the human eye or a radar screen) is not just that they are correlated with the states of some other part of reality, *but that we can know what state the measuring device is in and also know which states of the device are associated with which states of the part of reality of interest.* For observations to help us adjudicate between rival theories, we must be able to agree on which observation statements are true and on how those statements should be interpreted without first having to know which rival theory is true. This is why the idea of detection involves the requirement that we be able to fix the evidential significance of observations without begging the question.[9]

Even with this relaxed view of observation, contrastive empiricism still differs from some characteristic forms of scientific realism. Realists do not always reach agnostic conclusions when they confront pairs of theories they believe to be empirically equivalent (see, for example, Boyd 1985). Empiricists (whether of the constructive or the contrastive variety) decline to say which of Y_1 and Y_3 is true. Realists, on the other hand, often appeal to nonobservational considerations to decide this question. For example, they sometimes say that Y_1 is simpler or less *ad hoc* than Y_3, or that Y_1 is a better explanation of certain empirical regularities than Y_3 is. Realists think that such considerations solve discrimination problems that empiricists think are insoluble. In this respect, realism embraces a kind of *rationalism*—a conviction that nonempirical considerations are relevant to deciding which hypotheses are true. The realist holds that the methods of science are more *powerful* than the empiricist allows.

I don't object to the idea that Y_1 and Y_3 differ in the respects just mentioned. I also don't object to the idea that the concepts cited—simplicity, *ad hoc*ness, and explanatoriness—sometimes provide reasons that are pertinent to judging truth values. What I deny is that they do so in a way that transcends the bearing of likelihood (Sober 1988, 1990b). When greater explanatoriness reflects greater likelihood, it makes perfect sense to prefer the more explanatory hypothesis. However, Y_1 and Y_3 (and also X_1 and X_3) have *identical* likelihoods; this fact leads contrastive empiricism to deny that these discrimination problems are scientifically soluble.

A full exposition of contrastive empiricism would have to consider whether there are nonobservational signs of truth. For example, we would have to examine the claim that the *simplicity* or *parsimoniousness* of a hypothesis is a ground for deciding whether the hypothesis is true that goes beyond what the observations tell

[9]This account of how contrastive empiricism understands the concept of observation is, I think, more adequate than the one I suggested in Sober 1990a.

us. I believe that there is considerable reason to reject this idea (Sober 1988, 1990b; Forster and Sober forthcoming). But we need not pursue this question here. My goal in this paper is to evaluate indispensability arguments about mathematics. These arguments claim that the *empirical* success of a scientific theory is an *empirical* reason to regard the mathematical claims it embeds as true. So even if there were *non*empirical reasons for accepting a theory, these would not be relevant to the task at hand.

3. What's Wrong with Indispensability Arguments?

The Likelihood Principle has important implications for indispensability arguments. In a sense to be clarified presently, the only statements that are tested by observations are statements that are *dispensable*. Indispensability is not a synonym for empirical confirmation, but its very antithesis.

Let us suppose that mathematics is an indispensable part of any scientific explanation of the observations we have at hand. That is, each of the competing hypotheses ($H_1, H_2, \ldots, H_n$) embeds a set (M) of mathematical propositions. Suppose it emerges that the observations favor one of those hypotheses (H_1, say) over the others. Can we conclude that the empirical success of that hypothesis is an empirical reason for regarding the embedded mathematical statements as true?

If the Likelihood Principle is correct, the answer is *no*. It is an important feature of the Likelihood Principle that *which observational outcome occurs makes a difference:*

$$P(O/H_1) > P(O/H_2) \text{ if and only if } P(\neg O/H_1) < P(\neg O/H_2).$$

If the observations (O) favor H_1 over H_2, then, if the observational outcome had failed to occur, the opposite verdict about the hypotheses would be required.[10]

[10]In this way, the Likelihood Principle entails that there is a symmetrical relationship between confirmation and disconfirmation: a hypothesis is supportable by observations if and only if there are observations that would count against it. This conforms to the Popperian ideas quoted from

If the mathematical statements M are part of *every* competing hypothesis, then, no matter which hypothesis comes out best in the light of the observations, M will be part of that best hypothesis. M is not tested by this exercise, but is simply a background assumption common to the hypotheses under test.

Scientists wishing to discriminate among a set of competing hypotheses standardly recognize that they need to make background assumptions. These assumptions make it possible for them to bring observations to bear on the hypotheses. If the testing problem is statistical, they will call these background assumptions their "statistical model." It is a truism that the model *of* an experiment is not tested *in* the experiment. The model is supposed to include only statements that can be regarded as plausible regardless of which hypothesis turns out to be true. The background assumptions inevitably include some amount of mathematics. If the mathematical statements M are part of each hypothesis under test, then the observational outcome does not favor M *over any of its competitors.*

This last phrase allows us to say what it would take for the mathematical statements M to be supported by observations. We would need to test M against M', where M' is a competing set of mathematical hypotheses. What is required is that M and M' confer different probabilities on some set O of statements that can be checked by observation. It is perfectly permissible for background assumptions (A) to mediate the relation of M and M' to O. The observations would then be interpreted in terms of the Likelihood Principle:

$$O \text{ favors } M \text{ over } M' \text{ if and only if } \mathrm{P}_{\mathrm{A}}(O/M) > \mathrm{P}_{\mathrm{A}}(O/M').$$

In this testing procedure, M is *not in*dispensable; there is at least one other candidate hypothesis that we can consider. What is more, M is (to use a Popperian phrase) *placed at risk.* If O turns out to be true, we favor M; but if $\neg O$ turns out to be true, we favor M' instead.

Formulating the indispensability argument in the format specified by the Likelihood Principle shows how unrealistic that argu-

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Musgrave 1986 in note 5, though not to Popper's thesis that falsification and verification are asymmetrically related.

ment is. For example, do we really have alternative hypotheses to the hypotheses of arithmetic? If we could make sense of such alternatives, could they be said to confer probabilities on observations that differ from the probabilities entailed by the propositions of arithmetic themselves? I suggest that both these questions deserve negative answers.

None of these critical comments apply to abductive arguments concerning genes or quarks. The genetic hypothesis competes with other hypotheses, and these different hypotheses make different predictions (usually probabilistic in character) about what we observe. The same can be said for the theory of quarks. An observation justifies our believing such hypotheses precisely to the extent that it discriminates between those hypotheses *and others.*

If we provide such arguments from observations with a deductive formulation, the difference between the "indispensability" of genes and quarks and the "indispensability" of numbers becomes vivid:

(I) H_1 or H_2
$(H_1$ & $A)$ entails O and $(H_2$ & $A)$ entails $\neg O$
A & O

H_1

(II) H_1 or H_2
$(H_1$ & $A)$ entails O and $(H_2$ & $A)$ entails $\neg O$
H_1 does not entail O (or $\neg O$)
H_2 does not entail $\neg O$ (or O)
O

A

(I) is a perfectly respectable indispensability argument; the observational outcome O refutes H_2 and establishes H_1. We might describe this result by saying that H_1 is "indispensable" with respect to the task of explaining O (*modulo* the assumption that H_2 is the only other alternative).

(II), however, is highly questionable. We might be tempted to say that A is "indispensable" in this case; after all, the hypotheses H_1

and H_2 have implications about the observations (O) only if we assume that A is true. (II), of course, is not deductively valid. Nor does a probabilistic recasting of (II) make it any more acceptable. In (II), A is an "indispensable background assumption," but the observation statement O does nothing to show that A is true.

In (I), it is the observational outcome O that shows that H_1 is "indispensable"; if the observations had been different (so that we regarded $\neg O$ as true), then we would have said that it is H_2, not H_1, that is "indispensable." However, in (II) the type of "indispensability" possessed by A is something we can ascertain prior to the observational result; the observational outcome O could be deleted or negated without changing the basic force of the argument. Genes and quarks are indispensable *a posteriori*. Numbers appear to be quite different; they seem to be *a priori* indispensable.

Argument (II) contains a redundant premise. That redundant premise (O) gives the erroneous impression that the observational outcome plays a role in supporting the conclusion that is drawn. However, the basic idea behind (II) can be expressed more concisely as follows:

(III) H_1 or H_2 or . . . or H_n
For each H_i, H_i entails M

M

(III) is valid. In addition, (III), unlike (I) and (II), does not pretend that the empirical content of a scientific theory can be separated from its mathematical content. However, (III) does not mention the fact that one of the competing hypotheses turned out to be predictively successful.[11]

The contrast just introduced—between *a priori* indispensability and *a posteriori* indispensability—allows us to clarify the formulation of the indispensability argument with which this paper began. I construed the indispensability argument as offering an *empirical*

[11]Population biologists would call proposition M in argument (III) a *robust theorem* of the models described. It is an important question for the empirical sciences whether and why the robustness of a proposition is a ground for thinking that it is true. See Orzack and Sober (forthcoming) for discussion.

argument. We wish to explain why a given theory is predictively successful. This is an *observation*. The explanation endorsed is that the theory is true (or approximately so); if the theory includes mathematics, the preferred explanation entails that the embedded mathematical statements are true.

Although Putnam (1971) is often thought of as having defended this line of argument (see note 3, above), it is worth considering what he says later in the same essay, when he considers the problem of predictively equivalent theories. When it comes to comparing the realist's claim that a theory *T* is true with the "demon hypothesis," according to which *T* is false though a demon is making it appear as if *T* is true, Putnam invokes the concept of *prior plausibility* as a reason for preferring the former. Putnam says that judgments of prior plausibility are influenced by considerations of simplicity and by other considerations as well. These are not spelled out, nor is it explained why these considerations favor realism over its alternatives. Indeed, Putnam asserts that saying that realism is more plausible than the demon hypothesis "is neither to make a judgment of empirical fact nor to state a theorem of deductive logic; it is to take a methodological stand" (1971, 353). Putnam reports the stand he has taken and asserts that "all rational men" do as he has done.

I have already mentioned that I am skeptical of these nonempirical considerations. But the point of importance here is that Putnam's discussion of prior plausibility is not relevant to the *empirical* indispensability of mathematics, or of anything else. No one claims that genes and quarks are *a priori* indispensable. We believe in them because the observations are one way rather than another. If mathematical realism is to be justified by a similar argument, remarks about the need for a prior plausibility ordering are beside the point.

4. Objections

It may be objected[12] that I have failed to consider a way in which simple arithmetic propositions receive empirical confirmation in

[12] I owe this formulation of the objection to Penelope Maddy.

accordance with arguments that conform to pattern (I). For example, consider the set of competing hypotheses H_n:

(H_n) $2 + 2 = n$.

Each member of this set, when conjoined with the following auxiliary assumption A, makes a different prediction about how many apples there are on the table:

(A) There are 2 + 2 apples on the table.

Surely, the objection goes, when we observe that there are 4 apples on the table, this result favors H_4 over the other members of H_n. What is more, the inference just described can be represented as a pattern (I) argument:

H_1 or H_2 or . . . H_n or . . .
For each i, H_i & A implies that there are i apples on the table.
A
There are 4 apples on the table.

H_4

Although this argument is valid, it nonetheless conceals a salient fact about how we reason in such circumstances. If there had failed to be 4 apples on the table, I do not think we would have concluded that 2 + 2 has a sum different from 4. Rather, we would have concluded that the auxiliary assumption A is mistaken. If this is how we comport ourselves, then the "experiment" just described need never have been run. If we hold our belief that 2 + 2 = 4 immune from revision in this experiment, then the outcome of the experiment does not offer genuine support of that proposition.[13]

In the real world, we frequently encounter quantities that fail to combine additively. Pour two gallons of salt into two gallons of

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[13]Although this claim may seem to contradict Quine's idea that no belief is immune from revision, it does not. I am talking about the attitudes *we actually have in this experiment*; I do not suggest that we should hold '2 + 2 = 4' immune from revision in *all* possible experiments.

water; you will not obtain a volume of four gallons. Place two chickens together with two foxes; this will not produce four organisms, but just two foxes and a pile of feathers. Evidently, these processes do not shake our confidence in arithmetic. If we interpret *nonadditive* cases in this way, we can hardly claim that observed examples of *additivity* offer genuine confirmation of our arithmetic beliefs.[14]

It would be naive to insist that if mathematics is empirical, then a single experiment of the kind just described should suffice for us to reject propositions like '2 + 2 = 4'. As Quine has emphasized, there are perfectly empirical propositions that we are loathe to abandon when a prediction fails. The fact that we do not abandon '2 + 2 = 4' when we count gallons or organisms is of a piece with the fact that nineteenth-century astronomers did not abandon Newtonian mechanics when they encountered predictive anomalies. If it is good policy not to reject highly confirmed theories at the drop of a hat, then it may be possible to explain why we hold tenaciously to our mathematics and still argue that mathematics, in fact, is empirical.

My response to this line of argument is that I have no quarrel with the following conditional claim: *IF* our acceptance of '2 + 2 = 4' is justified empirically, then it still may make sense to retain our belief in it when our trafficking with gallons or organisms seems to engender a counterexample. I am not attempting to show that no observations could rationally dislodge our confidence in this proposition. Rather, my goal is to undermine a particular line of argument that purports to show that our justification for accepting '2 + 2 = 4' *is* purely empirical. For me, the suspect claim is that '2 + 2 = 4' is confirmed each time we add two objects to two other objects and obtain four. This claim would be true only if failures of additivity would *dis*confirm the arithmetic statement.

Thus, for all I've said, it may be true that genuinely *a priori* propositions and superbly well-entrenched empirical propositions have in common the fact that we are disinclined to reject them. If

[14]Here I am restating one aspect of an objection that Ayer (1958) formulated against Mill's claim that mathematical statements are inductive generalizations. My version of it, of course, is not tied to verificationism nor to a purely deductivist notion of empirical test. Nor am I arguing that no observation could disconfirm our belief that 2 + 2 = 4.

so, our disinclination to reject '2 + 2 = 4' when gallons or organisms behave nonadditively does *not* show that the arithmetic statement is *a priori*. On the other hand, our disinclination to reject *does* show something important about our inclination to accept. If we are not prepared to reject '2 + 2 = 4' in the face of nonadditivity, we cannot claim that our justification for accepting '2 + 2 = 4' rests on our encounters with additivity (Musgrave 1986).

I hope it is clear that my argument is perfectly consistent with the fact that the history of mathematics contains propositions that mathematicians believed for empirical reasons. Plateau's problem in the nineteenth century is an example. The problem, roughly, was to find the surface of least area that is bounded by a given closed contour. Plateau dipped pieces of wire into soap suds and empirically discovered the solution for a number of cases (Courant and Robbins 1969, 385–97). In this example, observations clearly do make a difference; the observational outcome of the experiment influences which mathematical statement we believe.[15] My skepticism about indispensability arguments does not involve a denial of examples such as this. What I doubt is that successful prediction provides a *general* grounding for the mathematics used in empirical science.

There is another objection that I want to consider, one that challenges a basic idea of contrastive empiricism.[16] Contrastive empiricism says that observations bear on hypotheses solely by virtue of solving discrimination problems; O supports H_1 only if there exists a competing hypothesis H_2 and H_1 confers on O a higher probability than H_2 does. The objection asserts that a theory sometimes gains confirmational support because it is able to predict outcomes that no earlier theory was able to address. It isn't that the earlier theories entailed *false* predictions (or assigned very low probabilities to the events that the new theory says were to be

[15]The use of computers to check complicated proofs (as in the solution of the four-color problem in topology) may constitute a further example in which empirical evidence (viz., the printout that a computer generates) really does shape the mathematics we believe. But here again we can see the Likelihood Principle at work: for the observations to favor one hypothesis, it must be true that a different hypothesis would have been favored if the observations had been different.

[16]I owe this objection to Geoffrey Hellman.

expected); rather, the earlier theories were *entirely silent* on the issue that the new theory addresses successfully.

If this point were correct, it would provide a quite general refutation of contrastive empiricism. However, its particular relevance to the role of mathematics in empirical science deserves emphasis. The suggestion is that stronger mathematical assumptions facilitate empirical predictions that cannot be obtained from weaker mathematics.[17] Again, it isn't that a theory equipped with weaker mathematics entails false predictions; rather, the theory with the weaker mathematics does not make predictions about matters that the theory with the stronger mathematics is able to address successfully.

My reply to this objection has two parts. First, in the empirical case in which it is alleged that theories sometimes are confirmed without competing against alternatives, I suggest that we *can* find competitors if only we set our mind to it. For example, it might be suggested that Newtonian theory correctly predicted the return of Halley's comet and that no other theory at the time was able to say anything on the issue, either true or false. However, this suggestion ignores the fact that alternatives to Newton's theory can be constructed from Newton's laws themselves. The inverse square law competes with an inverse cube law and with many others besides. If Newton's laws make a true prediction about the return of Halley's comet, there are alternatives constructable from Newton's laws that make false predictions about that event.

Further evidence for this claim may be found in the fact that scientists often discount predictive successes when the predicted outcome was expected.[18] The fact that T successfully predicts O is often judged to be no strong evidence for T, if scientists believe that O would have happened even if T were false. We philosophers have no good account of what it means to conditionalize on the negation of a theory; but this should not lead us to airbrush the phenomenon. The fact of the matter is that when scientists lack a developed

[17]Hellman (1992) argues this point in connection with an example about constructivist mathematics and quantum mechanics.

[18]I say "often" here because of the "problem of old evidence" (see, for example, the discussion in Eells 1990). The fact that Newtonian theory was able to explain facts about the tides was taken as evidence for the theory, even though those facts were known before the theory was proposed.

substantive alternative to a theory, they contrast the theory with its own negation. This is a contrastive alternative that is always available for the asking.

I now want to consider the suggestion that stronger mathematics may allow empirical predictions that weaker mathematics cannot produce. What I question is that the predictive success of the stronger theory is evidence that the mathematical assumptions are true. Suppose that S embeds stronger mathematics than W does and that S makes true predictions about matters on which W is silent. Is this evidence that the mathematics in S is true? I suggest that if the mathematics in S is given *credit* for these predictive successes, we should be prepared to *blame* those mathematical statements when they occur in theories that make false predictions. If S and W have the properties just mentioned, we can easily construct from them a pair of theories S' and W', such that S' embeds stronger mathematics than W' does, and S' makes *false* predictions about matters on which W' is silent. Strong mathematics facilitates true predictions, but it facilitates false predictions as well.

It is a striking fact that mathematics allows us to construct theories that make *true* predictions and that we could not construct such predictively *successful* theories without mathematics. It is less often noticed that mathematics allows us to construct theories that make *false* predictions and that we could not construct such predictively *unsuccessful* theories without mathematics. If the authority of mathematics depended on its empirical track record, *both* these patterns should matter to us. The fact that we do not doubt the mathematical parts of empirically *un*successful theories is something we should not forget. Empirical testing does not allow one to ignore the bad news and listen only to the good.

5. Concluding Remarks

Perhaps the indispensability of mathematical statements in empirical science is some sort of reason to regard those statements as true. Nothing I have said here shows that this vague statement is wrong. What I have criticized is the idea that a mathematical statement inherits the observational support that accrues to the empirically successful scientific theories in which it occurs.

Kant was talking about indispensability when he described various *a priori* truths as being *necessary for the possibility of experience.* However, he did not interpret this sort of indispensability as providing an *empirical* justification for the framework principles he was discussing. An assumption required by all coherent theories is not tested by observations.

The idea that the mathematical statements in an empirical theory are confirmed by the theory's predictive successes did not spring from nothing. The idea had its roots in *epistemological holism.*[19] Our beliefs, Quine (1953) says, face the tribunal of experience as a corporate body, not one at a time. Holism isn't just the logical point that theories require auxiliary assumptions if they are to be brought into contact with observations. It additionally maintains that evidence supports or infirms *whole* theories; evidential support cannot be apportioned differentially among a theory's proper parts.

The simple idea of the Likelihood Principle shows where epistemological holism goes wrong. Experience does not render judgments about a single belief *or* about a whole corpus of beliefs. Experience does not declare suspects guilty or innocent, quite apart from whether those suspects are individuals *or* corporations. Rather, experience solves discrimination problems. Given a set of hypotheses, experience helps determine which hypothesis, simple or complex, is most plausible.

It immediately follows that epistemological holism is mistaken. If experience is in the business of solving discrimination problems, then what will be true of the whole may not be true of the part. Consider two theories that share an assumption. For simplicity, we can represent each theory as a conjunction. T_1 makes an assertion of the form A & B; T_2 makes an assertion of the form A & C. Suppose an experience favors T_1 over T_2. I suggest that this test favors B over C but does not favor A over any alternatives that it may have. If *whole* theories compete against each other, it is an open question which *parts* of the theories we favor have actually been tested. Testing whole theories is not the same as testing the parts of those theories.[20]

[19] Laudan (1984, 227) has emphasized the connection between standard versions of scientific realism and epistemological holism.

[20] The defects in epistemological holism are also readily recognizable

Although I have formulated my criticism of indispensability arguments within the framework of contrastive empiricism, the core of the criticism can be separated somewhat from the details of that epistemology. Contrastive empiricism claims that likelihood is *the* vehicle by which the bearing of observations on hypotheses should be assessed. As a form of empiricism, it parts ways with epistemologies in which the plausibility of hypotheses is heavily influenced by *a priori* considerations (e.g., by the assumption that simplicity always augments plausibility). But even proponents of such nonempirical criteria should be able to agree that *empirical* considerations must be mediated by likelihoods.[21]

A symmetrical point holds true for epistemologies whose empiricism is more narrow than the one I endorse. Even if one maintains that science is limited to deciding whether theories are empirically adequate, it still may be agreed that judgments about empirical adequacy are best understood as solutions to discrimination problems.

I don't rule out the possibility that some of the theories that we now regard as mathematical eventually will undergo the metamorphosis that overtook geometry a century ago. Perhaps an alternative to number theory will be formulated someday and the old and new theories will turn out to make different predictions about observables. If this happens, we may find empirical reasons to favor one number theory over the other. What I deny is that anything like this scenario accurately describes the status of number theory *now*.[22] If we *now* have reason to regard such mathematical theories

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from a Bayesian point of view. An observation O can raise the probability of a theory T without thereby raising the probability of C, where C is a deductive consequence of T. For example, suppose I draw a card at random from a normal deck and place it face down on the table before me. Let H = "The card is the seven of hearts," let O = "The card is red," and let C = "The card is a seven." O raises the probability of H from 1/52 to 1/26, while the probability of C remains what it was before, namely, 1/13. It is astonishing how often realists formulate their indispensability arguments by appealing to what Hempel (1965, 31) called *the special consequence condition*.

[21]A related point is that the main argument of this paper is neutral on the dispute between Bayesians and "likelihoodists." Both sides should be able to agree that empirical considerations enter into the assessment of the plausibility of hypothesis by way of the vehicle of likelihood.

[22]The history of science has been full of surprises. What seems untest-

as true, and to take their ontological commitments at face value, those reasons are not empirical.[23]

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REFERENCES

Ayer, A. J. 1958. *Language, Truth, and Logic.* London: Victor Gollanz.

Boyd, R. 1985. "Lex Orandi est Lex Credenti." In *Images of Science: Essays on Realism and Empiricism,* ed. P. Churchland and C. Hooker. Chicago: University of Chicago Press.

Carnap, R. 1956. "Empiricism, Semantics, and Ontology." In *Meaning and Necessity.* Chicago: University of Chicago Press.

Courant, R., and H. Robbins. 1969. *What is Mathematics?* Oxford: Oxford University Press.

Edwards, A. 1972. *Likelihood.* Cambridge: Cambridge University Press.

Eells, E. 1990. "Bayesian Problems of Old Evidence." In *Scientific Theories,* ed. C. Wade Savage. Minneapolis: University of Minnesota Press.

Field, H. 1980. *Science Without Numbers.* Princeton: Princeton University Press.

Fine, A. 1984. "The Natural Ontological Attitude." In *Scientific Realism,* ed. J. Leplin. Berkeley: University of California Press.

Forster, M., and E. Sober. Forthcoming. "How to Tell When Simpler, More Unified, or Less *Ad Hoc* Hypotheses will Make More Accurate Predictions."

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able at one time may become testable later on. Perhaps taking this lesson to heart allows us to "imagine" that arithmetic will be empirically tested in some future science. Musgrave (1986) denies that this is imaginable, and concludes that indispensability arguments for mathematics are mistaken (see note 5, above). My inclination, on the other hand, is not to deny that we can imagine this, as long as "imagination" is understood as the vague term that it is. What I resist is drawing the conclusion that arithmetic is *therefore* empirical, in the sense that our *current warrant* for believing it derives from observation. I can imagine that one day my house will be held up by hot air balloons; it does not follow that balloons are what holds it up *now.*

[23]The doubts developed in this paper about indispensability arguments complement those adumbrated by Maddy (1992, forthcoming). Maddy emphasizes the fact that natural scientists do not always interpret the different parts of a successful theory in the same way; they often judge that some elements are useful idealizations while others should be interpreted more realistically. Maddy's naturalism leads her to take this discrimination on the part of scientists as something that an adequate epistemology must capture. I agree, and suggest that contrastive empiricism may be able to do just that.

Hellman, G. 1992. "The Boxer and His Fists: The Constructivist in the Arena of Quantum Physics." *Proceedings of the Aristotelian Society* 92.
Hempel, C. 1965. "Studies in the Logic of Confirmation." In *Aspects of Scientific Explanation and Other Essays*. New York: Free Press.
Laudan, L. 1984. "A Confutation of Convergent Realism." In *Scientific Realism*, ed. J. Leplin. Berkeley: University of California Press.
Maddy, P. 1992. "Indispensability and Practice." *Journal of Philosophy* 89: 275–89.
Maddy, P. Forthcoming. "Taking Naturalism Seriously." In *Proceedings of the Ninth International Congress of Logic, Methodology, and Philosophy of Science*, ed. D. Prawitz, B. Skyrms, and D. Westerstahl. Dordrecht: North Holland Publishers.
Musgrave, A. 1986. "Arithmetical Platonism—Is Wright Wrong or Must Field Yield?" In *Essays in Honor of Bob Durant*, ed. M. Fricke. Dunedin: Otago University Philosophy Department.
Orzack, S., and E. Sober. Forthcoming. "A Look at Richard Levins's 'The Strategy of Model Building in Population Biology'." *Quarterly Review of Biology*.
Putnam, H. 1971. *Philosophy of Logic*. Harper and Row. Reprinted in *Mathematics, Matter, and Method*. Cambridge: Cambridge University Press, 1975. (Page numbers refer to the 1975 publication.)
Quine, W. 1953. "Two Dogmas of Empiricism." In *From a Logical Point of View*. Cambridge: Harvard University Press.
———. 1981. "Success and the Limits of Mathematization." In *Theories and Things*. Cambridge: Harvard University Press.
Sober, E. 1988. *Reconstructing the Past: Parsimony, Evolution and Inference*. Cambridge: MIT Press.
———. 1990a. "Contrastive Empiricism." In *Scientific Theories*, ed. W. Savage, 392–412. Vol. 14 of *Minnesota Studies in the Philosophy of Science*. Minneapolis: University of Minnesota Press.
———. 1990b. "Let's Razor Ockham's Razor." In *Explanation and Its Limits*, ed. D. Knowles. Royal Institute of Philosophy, suppl. vol. 27. Cambridge: Cambridge University Press, 73–94.
Van Fraassen, B. 1980. *The Scientific Image*. Oxford: Oxford University Press.