reject all of this as myth unsupported by "real-life mathematical experience" (xiii). Instead, mathematics is a human creation, constrained (but not fully determined) by rules; mathematical truth and ontology change over time; mathematical knowledge is fallible and based on conjecture and refutation.

Hersh offers a *methodological* diagnosis of the "deplorable" (ix) state of the mainstream. "Traditional philosophy recognizes only the front of mathematics. But it's impossible to understand the front while ignoring the back" (36) or, as is often suggested (25, 39, 41, 177, 216, 236), while being ignorant of the back. It is not clear how humanism, informed with an insider's perspective, is supposed to help. An insider's view *might*: (1) help to foster a better appreciation of the messiness of mathematics and thereby frame problems "*in the right context, with a new possibility of solving them*" (249, xii); (2) show that there's no need to philosophize about mathematics by looking "for a hidden meaning" (23); and (3) provide a philosophical account of what mathematics really is. However, a prolegomenon (1) need not be therapeutic (2), and neither contributes a positive account (3). Hersh pays lip-service to (1) and (2), but most of the time he's doing (3)—criticizing the foolish answers supplied by Boeotian philosophers and replacing them with his own answers.

An insider's view might reasonably be expected to promote (1). The extent to which it can help with (2) or (3) is questionable. As Hersh admits, most insiders, being weekday Platonists and weekend formalists, are philosophically confused. Moreover, many philosophers from Descartes to Hilbert were insiders acquainted with the backstage. Why, then, did they chose to ignore it? A rather obvious answer is that their concerns and questions were not Hersh's. *His* question is, what is mathematics? *Theirs* is, how do we account for the objectivity of mathematical beliefs and reliability of mathematical reasoning? Mathematics is surely a social institution; so are physics and the Supreme Court. Whether, and to what extent, these truisms are relevant to our understanding of mathematical, physical, and constitutional objectivity and reasoning are other matters.

Although he recognizes the distinction, Hersh frequently takes himself to be responding to the traditional question when he's responding only to his own question. The resulting confusion becomes especially evident in his treatment of mathematical ontology. Numbers are neither physical nor mental (14); they are conceptual objects (15) or shared ideas (18) created by us with properties that are difficult to discover (16). However, just as our representations of chairs are not chairs, so our representations of numbers are not numbers. Hersh responds that numbers do not exist, but they do have properties. "This may sound paradoxical," he adds, but "it's an honest account of the actual state of affairs" (20). Hersh holds that arithmetical reasoning turns on the properties of objects only as those objects are represented in shared ideas. Read too literally, this claim is false. Presumably *that there's no greatest prime* was not literally included in *any* shared idea of number at some early stage in the evolution of arithmetical lore. It's a truth about numbers, not about our shared ideas of them. Hersh seems to respond (86–87) that our shared ideas at a given time possess unactualized potential content. He offers no account of what constitutes this potential content, however. Hersh correctly argues that mathematical extensions are neither arbitrary nor forced (7–13). But what, then, does constrain them? All we're told is that "tradition, taste, judgement, and consensus" matter. This is neither good therapy nor a good positive account.

Part Three (Chapter 13) discusses connections that Hersh sees between philosophy of mathematics, education, and political ideology. Unlike humanism, we are told, mainstream philosophy of mathematics tends to be pedagogically elitist and politically right-wing! The book ends with a doggeral about six blind men, each of whom boldly asserts (on the basis of his limited tactile experience of an elephant-part) a partial truth about an elephant. Mainstream philosophies may well be in this position with respect to mathematics, but Hersh's humanist answer hardly adds up to the elephant. *Michael Liston, University of Wisconsin-Milwaukee.*

Penelope Maddy, ***Naturalism in Mathematics.*** Oxford: Oxford University Press (1997), viii + 254 pp., $35.00 (cloth).

Let me start by saying that this is a good book, one that I highly recommend. It is interesting, original, and well-written, and it makes an important contribution to the philosophy

of mathematics. It is also historically fascinatin
insights about the history of mathematics and
The book is mainly about mathematical met
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(A) The CH question—i.e., the question
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(B) Philosophical considerations about th
wholly irrelevant to the argument for
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endorse CH.
(C) Mathematical naturalism, i.e., the v
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(D) In light of (C), the proper way to ar
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but rather, to use *mathematical* argur
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goals, practices, and methodologies
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(E) The proper way to go about deciding
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of mathematics. It is also historically fascinating: it includes all sorts of interesting facts and insights about the history of mathematics and the natural sciences.

The book is mainly about mathematical methodology, especially as it relates to undecidable propositions like the continuum hypothesis (CH). The central theses here are as follows.

(A) The CH question—i.e., the question of whether or not we should endorse CH—is a perfectly meaningful and legitimate question, worthy of mathematical pursuit.

(B) Philosophical considerations about the reality of abstract mathematical objects are wholly irrelevant to the argument for the legitimacy of the CH question; moreover, such considerations are also irrelevant to the question of whether or not we should endorse CH.

(C) Mathematical naturalism, i.e., the view that the theories and methodologies of mathematics do not stand in need of a supra-mathematical, first-philosophical justification.

(D) In light of (C), the proper way to argue for the legitimacy of the CH question is not to use philosophical arguments about mathematical realism and antirealism, but rather, to use *mathematical* arguments. Thus, a good naturalistic argument for the legitimacy of the CH question would proceed by (a) laying bare the nature of the goals, practices, and methodologies of mathematics; (b) explaining how these goals, practices, and methodologies are rational and justifiable (albeit justifiable from an internal, naturalistic point of view, as opposed to an external, first-philosophical point of view); and (c) explaining how, if we take these goals, practices, and methodologies for granted, it becomes clear that the CH question is a legitimate question, worthy of mathematical pursuit.

(E) The proper way to go about deciding whether or not we should endorse CH is to hunt for new set-theoretic axioms that might settle the question. Moreover, arguments for and against axiom candidates of this sort are (when cogent) purely mathematical, i.e., not philosophical. We can appreciate how these arguments proceed by simply looking at the history and practice of mathematics. In particular, arguments here can draw upon (i) the intuitiveness of the given axiom candidate; (ii) pragmatic considerations concerning the overall attractiveness of the resulting theory; and (iii) considerations involving various mathematical maxims and how well the given axiom candidate harmonizes with these maxims (and how well it aids us in our attempt to achieve various mathematical goals).

(F) Finally, it *could* turn out that mathematical arguments of the above sort do not settle the CH question. For there could turn out to be good mathematical reasons for embracing two different set theories that answered the CH question differently. But these reasons would not be philosophically based, and they would not show that the CH question was meaningless or illegitimate. They would be based on the mathematical attractiveness of the two theories and a belief that the benefits of pursuing both theories outweighed any desire to come up with a single, unifying set theory.

Maddy doesn't provide a detailed argument for all of the above points, but I think that this provides an accurate picture of the view that she is proposing. Her actual argument is centered around a case study that she makes of one proposition that has been taken as an axiom candidate, namely, the axiom of constructibility, i.e., 'V = L'. (Roughly, 'V = L' says that all sets in the cumulative hierarchy are definable by first-order formulae.) Maddy explains how there is a strong mathematical case against 'V = L' based on the fact that it is *restrictive* (because it rules out the study of hierarchies containing non-constructible, i.e., non-definable, sets). She explains that to endorse a restrictive axiom of this sort would be irrational and unacceptable because it would fly in the face of one of the most fundamental goals of mathematical practice, namely, the goal of using set theory to provide a foundation for mathematics without encumbering it. This goal, as well as the goal of studying whatever structures are mathematically interesting, leads to a methodological maxim that Maddy calls MAXIMIZE, which dictates that set theory "should be as generous . . . as possible . . . [and] provide models for all mathematical objects and instantiations for all mathematical structures . . . [and the widest possible] range of available isomorphism types" (210–211). Thus, in a nutshell, the naturalistic argument against 'V = L' is that (a) it violates MAXIMIZE, and (b) this is unacceptable because MAXIMIZE is an important (and internally justifiable) maxim of mathematics.

Maddy's claim that the debate about mathematical realism is irrelevant to the CH question—thesis (B) above—might seem a bit surprising. For, prima facie, it seems that the realism debate should be relevant here. In particular, it seems that (a) if there is a real universe of sets, then CH is either true or false of that universe, and (b) if there are no sets for CH to be true or false *about*, then it couldn't have a determinate truth value and, hence, there couldn't be a legitimate question here to answer. Now, Maddy argues that this traditional stance flies in the face of practice: regardless of whether realism or antirealism is true, there could turn out to be good mathematical arguments for CH, or ~CH, or the claim that there is no "right answer" here. But she doesn't say *how this could be*, or in other words, where the traditional stance goes wrong. My own view is that (a) Maddy is right here and (b) we can explain "how this could be" by rejecting traditional versions of realism and antirealism and developing and motivating alternative versions of these views that account for the fact that there could be good reasons for accepting CH or ~CH or neither (and that entail that mathematicians can and should decide what to say here without concern for the realism debate, i.e., by considering only mathematical arguments; see my 1998 for the details). I suspect that Maddy would approve of this general approach, for she acknowledges that some versions of realism and antirealism might be naturalistically kosher, i.e., consistent with mathematical practice.

The question of the naturalistic acceptability of mathematical realism brings us to an interesting facet of Maddy's book: one of its central theses is that the kind of realism that Maddy defended earlier in her career (1990) is *not* naturalistically kosher. Maddy argues, for instance, that her early view cannot account for the fact that the standard (and proper) argument against 'V = L' pays no attention to the "true nature of the actual universe of sets"—i.e., ignores the question 'Do there really exist any non-constructible sets in the cumulative hierarchy?'—and concentrates instead on the fact that 'V = L' is restrictive. More generally, Maddy argues that early-Maddian realism is inconsistent with MAXIMIZE, i.e., with the fact that mathematicians want to posit and study as many objects as they can, without concern for the question, 'Which of these objects really exist?' (But again, Maddy is not arguing against *all* versions of realism here. She allows that other versions of realism might be consistent with mathematical practice, and more specifically, with MAXIMIZE and the standard argument against 'V = L'.)[1] *Mark Balaguer, California State University, Los Angeles.*

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Maddy, P. (1990), *Realism in Mathematics.* Oxford: Oxford University Press.

Ernst Mayr, ***This is Biology: The Science of the Living World.*** Cambridge, MA: Harvard University Press (1997), xix + 323 pp., $15.95 (paper).

Ernst Mayr's latest book is well-titled. If an educated but biologically innocent friend were to ask me what biology now was, I could do no better than hand them this book. Apart from being among the most influential evolutionary biologists of the twentieth century, Mayr has devoted many years to studying the history and philosophy of biology. So the book doesn't just say something of what biologists now believe, but has serious things to say about how they came to believe those things, and even about the more abstract aspects of one's picture of the world that make it possible to believe things like that.

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[1] I would like to thank Penelope Maddy for commenting on an earlier version of this review.

Though I would expect readers of this journal
it is not a book addressed primarily to philosoph
ideas that ought to be taken very seriously by ph
requires a middle way between vitalism and red
shared by philosophers, is forcefully stated. Le
discussions is the observation that an antireduct
biology generally referred to as organicism has b
for most of this century. (Mayr (17) attributes
Mayr claims that this view, incorporating a ro
amounts to a consensus within contemporary
consensus is as solid in genetics departments as
delighted to find that Mayr was right.

I am less confident than Mayr of the value, o
nitions of fundamental terms. Indeed, I suspect
ocation of which he accuses philosophers in the
one hand, he concedes that we should not expec
thing more like "explication" (59), but on the oth
of equivocation" can be accomplished by provis
me that an explication is quite a different kind o
the most that can be expected for central theoret
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But having said this, I think Mayr is right to co
or consensus among philosophers as should be e
distinctions between, theories, hypotheses, laws,
the merits of this complaint, Mayr offers an inte
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of crucial concepts in evolutionary biology he lis
petition, altruism, biopopulation, and many othe

Proponents of the semantic view of theories,
theories by models, have often suggested a too
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have proved fruitful; acquisition of a new concep
I'm not sure that Mayr would welcome this dir
leaving aside some worries about the overempha
mental, I myself find the idea attractive.

As one might well excuse in someone who ha
years, Mayr has strong opinions about a numbe
philosophical issues underlying biology in partic
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tated, for instance, by the remark that "sociolog
successful in their endeavor . . . I know of no
socioeconomic factor on the development of a s
no doubt more will applaud. However, more ofte
on the topic of systematics, where his authority is
to his own acknowledges their legitimacy for sp
clarity why cladistic taxonomy is useless as a
concedes that it is appropriate, in fact, for a sp
(143–146).

This review has given little sense of the scope
development, evolution, ecology, the biological s