Part II
The Challenge of Nominalism

The Compulsion to Believe: Logical Inference and Normativity

Jody Azzouni

Abstract
The interaction between intuitions about inference, and the normative constraints that logical principles applied to mechanically-recognizable derivations impose on (informal) inference, is explored. These intuitions are evaluated in a clear testcase: informal mathematical proof. It is argued that formal derivations are not the source of our intuitions of validity, and indeed, neither is the semantic recognition of validity, either as construed model-theoretically, or as driven by the subject-matter such inferences are directed towards. Rather, psychologically-engrained inference-packages (often opportunistically used by mathematicians) are the source of our sense of validity. Formal derivations, or the semantic construal of such, are after-the-fact norms imposed on our inference practices.

I

Mathematical proof amazed ancient Greeks. Here was a method—reasoning—from assumptions to unexpected new results. Furthermore, one saw that the conclusions *had to follow*. On my reading of Plato's *Meno*—and his other dialogues—the Greek discovery (of deduction) not only provoked Plato to the hopes of finally resolving ethical differences (by importing the method of reasoning from geometry), but also provided him—by means of a best explanation for why mathematical proof works—support for reincarnation.

Those were glorious days for *philosophy*, weren't they? So much seemed possible then by sheer reasoning alone—and there are *still* philosophers living off the meager echoes of *that* project. But some thousand years later most of us are—comparatively speaking—rather jaded about deduction; indeed, many philosophers, sociologists of knowledge, and others, are jaded enough to find tempting social constructivist views about mathematical proof. Social constructivists take mathematical proof as no different—sociologically speaking—from other practices that humans conform to: cuisine, tacit restrictions on polite conversation, linguistic rules, and so on. On such views, the plethora of alternative logics—and within them—the plethora of alternative mathematical systems, that were such a shocking discovery of the twentieth century, should have been expected; indeed, only sheer historical (and contingent) facts are

available to explain why mathematics took the particular developmental trajectory it took, and why it was tacitly based upon the particular logic (until the twentieth century) that it was based upon. Reason, on such a view, is a kind of fashionable dress of cultures—canons of reason, too, ebb and flow among peoples.

Social constructivist views, however, don't recognize how unusual mathematics—sociologically speaking—is. I must be brief;[1] but it's striking how, in contrast to politics and religion (and philosophy, for that matter), doctrinal "mistakes" lead in the latter cases to new views, or to new standards that views should presuppose, whereas in mathematics "mistakes" in proofs are eliminated—even if undetected for many years. One striking piece of evidence that mathematical proof, during its thousands-of-years development, remained largely within the confines of a particular (although tacit) logic, was that the grand regimentation of it by logicists—Frege, and later, Russell and Whitehead—largely *succeeded* with respect to the mathematics of the time. Indeed, the plethora of "alternative mathematics"—based on different logics, and different substantive mathematical principles—became a topic for mathematical exploration largely because logicists had made what appeared to be the logic of mathematical proof (indeed logic *tout court*) *explicit*, and so practitioners could—for the first time—consider "changing the rules."

Call the following theses *the traditional view*: (i) informal mathematical proofs, though in the vernacular, correspond to derivations of formal languages (perhaps by being abbreviations), where derivations are mechanically-recognizable constructions without missing steps[2]; (ii) this (tacit) correspondence explains the uniqueness of mathematics as a social practice. The properties of informal mathematical practice—including its apparent imperviousness to changes in its logic—is explained by mathematicians (when constructing or reading informal proofs) actually "grasping" derivations (in formal systems) corresponding to these proofs.

The traditional view satisfies thricewise: First, it provides causal machinery—derivations—for explaining the uniqueness of mathematics as a social practice; second, via those same derivations, it provides normative standards by which

1 See Azzouni 2006, chapter 6, for the longer version.

2 I argue in my 2005 that the background logic of derivations, at least with respect to traditional mathematics, induces the same consequence relation as the first-order predicate calculus—but this isn't required of the traditional view. If, however, the traditional view takes the formal language as *one* specific language, then it's refuted by Gödel's theorem. Read the traditional view, therefore, as taking the formal derivations in question as belonging to an open-ended *family* of formal languages.

informal mathematical proofs are (ultimately) to be judged by practitioners as correct or incorrect; and so, third, it uses the phenomenological impression of compulsion—that mathematical proof induces in cognoscenti—to connect the perception of causal machinery (derivations) to normative standards (also derivations). How nice when the descriptive and the normative dovetail so compatibly.[3]

And yet how suspicious. (Philosophy begins in wonder, but that pleasant sensation never lasts.) I confess to being tempted by the traditional view[4]—so described—even long after I had learned to resist attributing to us the magical powers purported knowledge of abstracta invite: that somehow, by the magic of pure descriptions, we grasp truths about eternally aloof objects. Descartes, we remember, began modern philosophy with a similar welding of the (psychological) impression of compulsion with the recognition of a standard (clarity and distinctness) for recognizing metaphysical necessity. No surprise that he was a first-rate mathematician.

2

What's wrong with the traditional view? This: It requires mathematicians to have enough of a grip on the—otherwise unexplicated—derivations (taken to correspond to informal proofs) to explain in terms of those derivations the sense of validity an informal proof induces in its readers. Because informal mathematical proofs always—from the point of view of the formal derivation—skip numerous steps, this is possible only if the missing steps (via the mathematician's *awareness* of what's missing) are causally active in the phenomenology of mathematical proof. That is, perception—in some sense—of the derivations that correspond to informal proofs can thus be the source of the compulsion induced by informal mathematical proofs only if—somehow—

3 Although the phenomenological sensation of the compulsion to believe is the central topic of *this* paper, by no means is it my full story of how mathematicians are convinced that a proof for a result exists, nor even the full story of how a mathematician—when surveying a proof—is convinced of its validity. The division of intellectual labor within mathematics itself (see my 1994, Part III, § 2) operating as it does both with respect to results the mathematician presumes the truth of, and even during the cognizing of a particular proof (where some steps in a proof are accepted on authority), already shows this.

4 Indications of backsliding to the traditional view may remain visible in my 2006; they're, I hope, absent from my 2005—which was written after.

the tacit recognition of the missing steps gives enough of a grip on the course of a derivation corresponding to an informal proof to explain why the mathematician *feels* the conclusion follows from the premises. Only in this way will the normative standards that corresponding derivations supply to informal proof—that such derivations are themselves *the standards* by which mistakes in informal proofs are recognized—actually be operative in the recognition (on the part of mathematicians) that a proof is valid. (And only in this way can the uniqueness of mathematics as a social practice be explained by the "perception" by mathematicians of correlated derivations.)

I've come to believe that the requirements just laid out are impossible to meet. Therefore, the normative role of the correlated derivations *isn't* connected to the phenomenology of compulsion that informal mathematical proof induces. Furthermore, I suggest this coming apart of a psychological impression, and the normative standards that it's supposed to be an impression of, is widespread: Philosophers often—as the Cartesian example indicates—treat psychological compulsion (that something seems like it *must be* a certain way) as a kind of perception (e.g., that of the metaphysics of possibility and necessity), and therefore as having—for that reason—normative force. How this fails with respect to informal mathematical proof illustrates a general phenomenon.

Consider a derivation that's to replace an informal proof. When such is constructed, not only will it be very much longer than the original—involving syntactic manipulations that a mathematician couldn't even be aware of, but it will be padded with additional assumptions that mathematicians also—often—couldn't be aware of.[5] But there's an interesting phenomenological point about the relationship of the formalized proof to its unformalized cousin: Even if we understand *that* each step in a derivation follows from earlier ones, that knowledge needn't contribute to our understanding of the informal proof; rather, one often gets lost in the details of the formal derivation, and can't tell what the "main ideas" are. So, at least phenomenologically, it seems that the source of compulsion—the sense that an informal proof is valid—*isn't* due to a perception of the correlated formal proof. Indeed, the epistemic process is usu-

5 Mackenzie 2005 is a news-brief about "proof assistants": software that checks proofs that have been formalized into appropriate (mechanically checkable) form. We find that "all in all, people who have used proof verifiers say they can formalize about a page of textbook mathematics in a week." There is also an anecdote in Moorehead 1992, p. 92, that Whitehead estimated completing *Principia Mathematica* would take "a short period of one year," a tenfold underestimation by someone who had published much mathematics.
I chose the word "couldn't" deliberately. Often the explicitation of—heretofore tacit—assumptions underlying an informal proof are matters of major mathematical discovery.

ally the reverse: One understands *why* a formal proof is possible only because of the way that it was constructed—starting from an informal proof.

One might attempt to save the purported causal role of derivations in the recognition of the validity of informal proofs by borrowing a page from linguistics. A truism in that field is that our ability to distinguish grammatical from ungrammatical sentences is due to complex (subconscious) processing. Strikingly, what's processed (be it rules, or whatever) is so inaccessible to introspection that one can only discover it empirically. And this means that were one to see a description of the mechanisms by which one distinguishes grammatical from ungrammatical sentences, they would remain introspectively alien: One would fail to see how such contributed to one's understanding of grammar.

The traditional view still has a hope if one or another sort of formal derivation neurophysiologically (as it were) underlies the mathematician's grasp of informal proof. Unfortunately, the empirical prospects for this hypothesis aren't good: As we gain an understanding of the neurophysiological bases of mathematical abilities, the result isn't the discovery that the grasping of derivations of one or another formal system lies in back of our abilities; rather, it's that there is a patchwork of narrow modularized capacities that are brought to bear on mathematics. These capacities—dispositions, in some sense—are proving to be fragmentedly piecemeal in their scope, and ones that, in addition to enabling mathematical task-solving—e.g., adding—are equally the source of common mathematical errors.[6]

So informal mathematical proofs are perspicuous—and therefore often clearly communicate the logical status (validity or otherwise) of an informal proof, in contrast to formal derivations that are impenetrable except in the step-by-step mechanical sense that each step follows from earlier ones. Indeed, even in the case of an informal proof, one's sense that it's valid often *precedes* a close examination of its steps; one gets the sense of validity first—on the basis of a broad overview—and then looks to the steps to see *how* the trick is turned.

The foregoing leaves us with two things that need explanation. The first is where the phenomenological feel of psychological compulsion—the "it must be" that we feel when recognizing the validity of an inference—is coming from: What (causally) is it about a good informal proof that compels assent? And second (and notice these queries will now have answers that aren't linked) an explanation of the normative status of strict derivations must be given as well.

6 See, e.g., Dehaene 1991 or Dahaene 1997. Notice the point: these "dispositions" are as much the foundation of our competence (in those aspects of mathematics they apply to) as they are the foundation of our incompetence.

First the psychological compulsion (the sense of validity of informal proof): We come equipped—neurophysiologically—with "inference packages."[7] Inference packages are topic-specific, bundled, sets of principles naturally applied to certain areas: various visualization capabilities, language-manipulation capacities, kinesthetic abilities, and so on. For example, we can spontaneously visualize how line figures change when moved about on a surface. We recognize this—in part—by factoring in the curvature of the surface they are on; but success in this endeavor occurs without any introspective grip on what inferential principles we're using. That is to say, we can't explicitly formulate generalizations about the curvature of surfaces and how that curvature affects properties of figures on the surfaces.[8] "Intuition," as mathematicians use the term, involves inference packages—which amount to our grasping bundles of principles without necessarily being able to distinguish specific assumptions[9]; so does the elusive "understanding" that some proofs provide, and others don't.

Inference packages—though genuinely "syntactic" insofar as they involve the manipulation of sets of bundled principles to enable the drawing of implications from those principles—don't introspectively present as syntactic for two reasons. The first is that inference packages are psychologically designed for specific situations—e.g., geometric visualizations: Thus the concepts involved in such packages—introspectively speaking—seem to come *intrinsically attached* to fixed subject matters—indeed they seem to come with an implicit interpretation; this is something that "concepts" governed by syntactic rules don't come with. In order for a mathematician to shift the application of an inference package from one (mathematical) domain to another, she must—often with difficulty—"reinterpret" the concepts in an inference package.[10] Second,

7 I first described "inference packages" in my 2005.

8 Indeed, it took mathematical talent of a pretty high order to determine what these generalizations look like.

9 A qualification. This way of putting the matter makes it sound like it's simply a *fact* that a *bundle* of principles—logically equivalent to the inference package as a whole—is the *same* as that package. This isn't *quite* my view. Rather, they are *deemed* identical in light of the later construal of the package (in terms of the various principles it's identified with). Also, the package—in operation—can at times *deviate* from what we *take* (from the vantage point of the various later principles) to be the right answer. See the last three paragraphs of section 3 for a discussion of the first point. My second point is touched on in the last paragraph of this paper, and discussed more fully in my 2005.

10 An example: I may mentally depict four-dimensional geometric spaces via visualization of a three-dimensional space where the points are endowed with—in addition—real-valued "temperatures" ranging from $-\infty$ to $+\infty$.

inferring via such packages is accompanied by a psychological compulsion to believe that if *this* is the case, then *that* must be the case. Such a compulsion always accompanying inferences bundled with an implicit subject-matter gives the impression that the subject matter—what the inferences are about—is *itself* forcing the conclusions. This phenomenological impression, it should be noted, is rarely present during mechanical manipulation (according to rules) to determine that something follows from something else.[11]

I've described the compulsion in inference (in much of mathematical proof) as including the impression of the subject matter forcing conclusions from premises. And, I think, this is why the thought of making mathematical proofs explicit not only required every step be present, but was also accompanied by the idea that doing so would supply a complete (interpreted) *language* for mathematics—a complete set of primitive mathematical concepts. The rules governing such concepts were to be (semantically) transparent: They would capture (fully) what such concepts *meant*. (So each concept would come with a set of definitional axioms.) In turn, every step in a proof being present, plus all the concepts in any proof being drawn from this complete set of concepts, would result in proofs that would make transparent how the psychological compulsion—the recognition of validity—present in our perception of informal proofs resulted from our tacit recognition of the underlying complete proofs.[12]

On my picture—on the contrary—the source of the compulsive impression of validity is due to the inference packages that psychologically enable

11 Hacking (1973, p. 202-03) notes the operation of this distinction in what he takes as the emergence of our contemporary notion of proof in Leibniz. He writes: "Geometrical demonstrations can appear to rely on their content. Their validity may seem to depend on facts about the very shapes under study, and whose actual construction is the aim of the traditional Euclidean theorems." He adds: "[a]lgebra is specifically a matter of getting rid of some content. Hence in virtue of Descartes's discovery, geometrical proof can be conceived as purely formal." It's a contingent psychological fact that inference packages—that enable the algebraic manipulation of proofs—seem to lack content. But given this is true (and it seems to be), geometrical proofs almost always seem explanatory and to provide understanding (for *why* something is the case) in ways that algebraic ones don't.

12 If "mathematical" is replaced with "logical," as it should be on Frege's view, then this—nearly enough—is Frege's project (and indeed, that of Russell and Whitehead). Frege's logical language is interpreted. The contemporary vision of logical languages as open to the reinterpretation of their "nonlogical vocabulary" comes later. Gödel's theorem—in various forms—shatters the logicist project; but I'm focusing on the *epistemic* drawbacks of that project, specifically with respect to the attempt to analyze *what* we are recognizing when we recognize a successful informal mathematical proof as such.

mathematicians to construct and understand informal proofs: Formal derivations are too far away (psychologically speaking) to play a causal role in the psychological story of how the ordinary mathematician either constructs or understands informal proofs.[13]

3

The second bit to be explained—recall—is how, despite the absence of a causal role, derivations nevertheless came to play a normative role in informal inference: why we currently take them to embody standards of correctness/incorrectness for informal mathematical proof. Here's how that happened. It's already a normative given in mathematical practice that an informal proof is to be faulted if (i) it relies on substantial assumptions that are tacit, or (ii) if it skips steps that are nontrivial to establish.[14] Thus the status of a successful informal proof is seen as promissory in the sense that should the explicitating of tacit details reveal a non sequitor or a false assumption, the proof is taken to have failed.

Mathematicians (like all of us) take the ability to engage in a complicated activity that apparently involves many presuppositions to indicate that—in some sense—we have a (tacit) grip on all the presuppositions. Thus it's easy to think that a mathematician's understanding of an informal proof turns on a tacit grasp of a version of that proof without the missing steps, and without the missing assumptions. So part of the story for why derivations operate as norms for ordinary mathematical proof is an error theory: Given that one takes an ordinary mathematical proof to be skipping steps (given that one accepts the model that an informal mathematical proof requires "filling out"), one was—and is—routinely mistaken about how much is missing (how much is skipped)

13 However, the contemporary role of the computer in mathematical practice has induced a new *causal role* for (tokens of) formal derivations in ordinary mathematical practice—although that causal role *isn't* psychological. Computer proofs provide warrants that certain (mathematical) results are true—such is based on empirical results that computers have verified certain derivations: This may take the form of good empirical reasons to think that a computer has actually "constructed" a token of a formal derivation.

14 It's not seen as creative to fill in missing steps in proofs—that's left to textbooks. But new proofs of established results are of interest to creative mathematicians if they use a significantly different approach; and, of course, "substantial" and "nontrivial" are professional judgments.

in such.[15] That derivations have the status of norms for informal proofs is also due, however, to there being no principled stopping point in the explicitation of an informal proof earlier than a (formal) derivation that can be taken to correspond to it. Only in such a derivation does the process of possible analysis seem finished: only there is every step "present," and every concept that was tacitly involved in the informal proof now explicit.[16]

Notice that this normative role doesn't require derivations to have a *causal* (psychological) role in how the mathematician recognizes errors in informal proofs. That can be explained not by requiring psychological access to a strict derivation that "fully explicitates" the informal proof, but by access to equally informal explicitations of proofs that fill in (some) missing steps or assumptions.

One may worry that this explanation—contrary to the advertisement in the last paragraph—nevertheless (surreptitiously) brings in perception of formal derivations. The recognition of gaps in a proof must involve a sense of what a gapless version of that proof would be like. (And if that requires sensing—somehow—the formal derivation corresponding to the proof then such are back in the picture.)[17] What, therefore, is the source of the thought that informal proofs are missing steps, and what is it that allows the mathematician to regard a proof that "fills out" some of these gaps to be an elucidation of the original proof, as opposed to something new?[18] One point of this concern is that the answer to this question shouldn't turn on a "tacit" perception of the goal (the more explicit proof, and ultimately, a formal derivation).

I don't want to suggest—because I doubt it's true—that as one comprehends and becomes convinced of any particular informal proof, that one necessarily has—then and there—a perception of its gaps. Sometimes, of course, that's true. We *often* have the sense that steps have been skipped (and not just in mathematics, of course), and we often request that some of these be filled in.

15 Recall footnote 6.

16 Nevertheless, there is latitude in what derivation corresponds to an informal proof because, when—from the perspective of formal derivations—steps are missing, there are often non-equivalent ways of traversing the gaps. This hasn't affected, however, the normative status of derivations. If one or another derivation corresponds to an informal proof, and the concepts made explicit aren't too arduous for mathematicians to be taken to have presupposed, the informal proof is taken as corresponding to that proof.

17 We seem to be tripping over the paradox Plato (1961, p. 363) mentions in 80-81 of *the Meno*.

18 My thanks to Nancy Bauer, Sylvain Bromberger, and Eric Swanson for this particular formulation of the question.

Here, one *can* (often) rely on the idea that a fuller explicitation of the argument is playing a psychological role: One needn't, however, take that fuller explicitation to be anything like a formal derivation.

But apart from this, there are various models in the practice of mathematics—ones that arise quite early—that are taken to mark out in a clear way a contrast between what's "explicit" and "tacit" in an informal proof, and in a way that offers a contrast of completeness for informal proofs that supplements the above perception of gaps in arguments.[19]

Consider sheer calculation; one first learns about multiplication by its relation to addition, and about addition by its relation to counting[20]; furthermore, one sees how various errors can arise, both at the ground level (by inadvertantly skipping a numeral, or an object), and by introducing shortcuts (in addition and multiplication). One, therefore, has—in the informal context—a *full* characterization of how mistakes arise, and how, by utilizing other methods, one can triangulate access to right answers by means of multiple approaches. The importance of this triangulation through multiple approaches is that various mistakes (in the different approaches) don't coordinate into *systematic* (and thus uncorrectable) errors.

Syllogistic reasoning, on the other hand, seems to exhibit *entirely explicit reasoning*: Valid inferences are recognized by sheer grammatical form. Here it looks like the analysis of a (quite short) proof has come to an end: There are no missing steps. Most ordinary mathematical reasoning, of course, doesn't look anything like this.[21] Finally, there is also the example of compass and straightedge constructions in Euclidean geometry.[22] Here too, a mechanical proof-system is in place; and proofs are seen as incomplete only in the tame respect either that there are assumptions that one suspects should be instead proven (e.g., the fifth postulate), or that there are cases missing.[23] Proofs in

19 These "arise quite early" both in the sense that one runs across such cases early in one's mathematical education, and in the sense—historically—that they arose early in the development of mathematics.

20 I'm not speaking of recursive definitions; I mean the related informal point that, e.g., the adding of 17 to 15 can be executed by counting 17 items starting with the word "16"; similarly, that multiplying 6 by 7 amounts to "taking 6 seven times," i.e., counting 6 items seven times.

21 It's significant, however, that syllogistic reasoning turns out—from the perspective of the first-order calculus—as *in*completely analyzed because there are missing connectives. I make something of this shortly.

22 See my 2004.

23 That is, there is sensitivity to the danger of a mismatch between the cases depicted by a diagram, and the cases actually under consideration.

other branches of (traditional) mathematics clearly differ, both in the absence of intuitively-justified mechanical-methods of reasoning, and in the absence of clear signs that all the steps and assumptions are largely present.

That these *exemplars* place external pressure—by comparison—on one's sense that other informal proofs (in mathematics) are gapless is compatible with such exemplars themselves subsequently being inexplicit vis-à-vis formal derivation. But one can ask why explicitations of proofs—ones that are supplemented by extra assumptions, and additional steps—are seen as *explicitations*. For example, imagine that a particular assumption is analyzed into several sub-assumptions that can subsequently be separated. Why is this seen as a matter of more fully analyzing the proof, as opposed to being a replacement of the original proof by a new one with different assumptions (some of which imply the original assumption)?

I claim that, strictly speaking, there is no fact of the matter about whether an "explicitation" of a proof really is an explicitation of *that* proof, as opposed to a stipulative embedding of it into a different proof.[24] Rather, it suffices to point to the reasons for why mathematicians will embrace such embeddings, and (consequently) *take* them to be explicitations.[25] First, there is our tendency to bundle assumptions together, and infer conclusions from them as a group. This case, which can reasonably be taken as one in which we *do* have psychological access to the separable assumptions, is confounded with cases where we don't. Thus, an analysis of a proof—that analyzes it into additional steps and assumptions (that we recognize to imply steps in the original proof)—is always presumed to be an *analysis* of the *original* proof.

It's important that such explicitations (almost always) respect the original proof—and so nothing is lost; but because (from the point of view of the explicitation) assumptions have been made explicit—separated out from one another in certain cases—and inferential steps have also been made explicit, theorem-proving capacities are greatly increased. This is because one is no longer restricted to applying what amounts to the principles only as a group. In general, explicitating increases proof-theoretic strength.

It's also worth stressing that the "explicitation of proofs" often involves a supplementation of the concepts involved in the proof—e.g., *number*, *function*, *integral*, and so on, so that proofs in one language are (often implicitly)

24 Recall the qualification of footnote 10.

25 These positive reasons are exactly the same ones that drive us, when working with explicit algorithmic systems, to embed algorithmic systems conservatively in stronger ones. See Azzouni 1994, Part II, § 6.

assimilated to proofs in a different language.[26] In this case, strictly speaking, new assumptions have come into play, although they are—especially in the context of informal mathematics—not always seen that way.[27] It's important to the understanding of mathematical practice to recognize that there is no sharp introspective distinction between analyzing a proof to tease out tacit assumptions that one has—in principle—introspective access to (because they are assumptions that, at one stage, one has *learned* to bundle together), and where such analysis amounts to embedding that proof in another so that the result is a genuine supplementation. The simultaneous demands to conserve already established results, and at the same time to develop new and interesting mathematics, work together to make such a distinction irrelevant to mathematical practice.

4

There is another aspect of the normative role of formal derivations that may seem still unexplained by the foregoing. This is that we often recognize the standards in one or another practice as ones easily changed. Say that the standards in such cases have "weak normative role," and distinguish this from "strong normative role," where the standards are perceived as unchangeable. Traffic laws have only "weak normative role": We recognize such laws can be changed, even though changes—in certain cases—won't be good.[28] But the (classical) logical principles governing derivations that we take to tacitly govern informal mathematical proofs seem to have strong normative role. Classical logic strikes many to be a standard for reasoning that we can't drop. This is a large part of the intuition that many have that such principles are "a priori."

For such, our recognition of validity isn't like the recognition of a standard we *happen* to have. Instead, they react to proposals of alternative logics with the bafflement suitable towards an incomprehensible or irrational suggestion. They recognize, of course, that an alternative logic—one that allows true con-

26 As a result, some proofs, with respect to an earlier set of concepts, can become special cases with respect to the later concepts. Lakatos 1976 is a famous discussion of this phenomenon.

27 See section 7.11 of my 2006.

28 We could reverse the role of red and green traffic lights. Given the differential hard-wired responses we have to these colors, the result wouldn't be as optimal as our current conventions.

tradictions, say—*could* be adopted for proofs and reasoning; they recognize, that is, that such an alternative logic would allow practitioners the *mechanical recognition* of the "validities" so defined. But they balk at the idea that such "validities" would therefore "make sense."[29]

And yet, it's striking how many philosophers (and logicians) *lack* this otherwise extraordinarily powerful intuition. Given the presence or absence of such a powerful phenomenological impression operating in the background of one's views about the status of classical logic, it's no surprise that those, on the one hand, who explain (away) the source of the intuitions that purport to give classical logical principles strong normative role, often provide explanations that—to those gripped by such intuitions—seem to miss the point (and force) of the intuitions. On the other hand, if those gripped by such intuitions go beyond the mere assertion of them (the mere assertion that classical logical principles have strong normative role) by offering a *justification* for that role, they do so in ways that seem—to *their* opponents—obviously circular. I give—in the rest of this section, and in the next—some illustrations of this (depressing) aspect of the debate about the strong normative role of classical logic.[30]

Consider the suggestion that logical connectives have properties (e.g., Gentzen introduction and elimination rules, or axioms) by virtue of their meanings. This isn't an old view quietly buried with the logical positivists—it's still *au courant* here and there. But for those who feel the normative compulsion of the rules that govern the classical logical idioms, this characterization is too shallow: Meaning is a matter of stipulation; and if not—because meanings attributed to terms are so attributed to capture antecedent usage—still (without further grounds), antecedent usage is arbitrary, and could have been different. Construing logical connectives as having the properties they have because of the (antecedent) meanings they have procures—at best—weak normative role.[31]

29 Frege (1967, p. 14) expresses this widely shared sentiment: "But what if beings were even found whose laws of thought flatly contradicted ours and therefore frequently led to contrary results even in practice? The psychological logician could only acknowledge the fact and say simply: those laws hold for them, these laws hold for us. I should say: we have here a hitherto unknown type of madness."

30 An hypothesis (I have *no idea* how one would empirically test this claim): Those *with* this intuition reason—usually—via inference packages. Those *without it* usually reason formally or quasi-formally.

31 A way to see this is to note that contradictions *aren't* meaningless. (Their meaningfulness is marked by our recognition that they must be false.) Neither are liar paradoxes. (*Their* meaningfulness is marked by the nausea-inducing recognition, based on what they seem

Others—Quine notably—diagnose aprioristic intuitions about classical logic as due to the central role classical logic (currently) has in our web of belief.[32] To explore alternative logics is to explore ways of possibly dissolving such intuitions away by the exploration of an alternative web of beliefs with an alternative logic at its center. The bafflement, when initially faced with an alternative logic, therefore, is due to insufficient practice in an alternative mindset: live long enough amidst the inferences of an alternative logic and such atavistic intuitions (eventually) wither away.

This Quinean explanation offers a promissory note: Our initial fear of what looks to be a hitherto unknown form of madness vanishes with practice. Thus, on this view, one could say that the (apparent) impression that classical logical principles have strong normative role is merely the perception of their weak normative role accompanied by lack of (logical) imagination. This explanation can be supported by the recognition that logical principles—in practice—aren't applied as unanalyzable units rejected or accepted only as wholes. Rather, the shift to an alternative logic is—for certain classical laws of logic—a change in status from that of a topic-neutral principle, applying to any subject matter whatsoever, to a topic-specific law applicable only in special circumstances. Thus much of the *specific* reasoning

to say, that they must be false *and* true.) Similarly, Gentzen rules governing connectives (such as Prior's infamous "tonk") aren't meaningless either (*pace* Tennant 2005)—the rules governing such a connective allow the inference of every sentence. Such may be pragmatically undesirable, or trivial; but the charge of "meaninglessness" is a surreptitious violation of our intuitions about meaning that otherwise clearly allow us to "make sense" of such items.

Some may feel the paragraph—this footnote is appended to—gives attempts to root logic in meaning short shrift. Quine (1986, p. 81) once wrote in passing: "Here, evidently, is the deviant logician's predicament: when he tries to deny the doctrine he only changes the subject." Let's grant the point: what does it show? Not much because the issue should be about the possibility of dropping one set of topic-neutral devices that are constitutive of inference for another set. This issue—construed this way—is the focus of many of those who take "alternative logics" seriously. Who cares, therefore, whether or not the paraconsistent or intuitionistic "and" has the same "meaning" as the classical "and"? What's important, rather, is (i) that there are (formal) languages in which nonclassical operators play analogous roles to those played by classical connectives and quantifiers, and (ii) that in many cases, such competing logical operators cannot hold simultaneously sway over the same formal languages without collapsing into one another. (See Harris 1982 for the case of intuitionistic and classical logic.)

32 Nagel (1997, p. 61) expresses a Quinean thought as the antecedent of an argument that Quine himself would reject: "Certain forms of thought can't be intelligibly doubted because they force themselves into every attempt to think about anything."

allowed in a classical setting is retained if the logic shifts—but is now labeled differently.[33]

For those gripped by the intuition of the strong normative role of classical logic, these suggested "explaining away"*s* don't touch something fundamental in the perception of strong normative role of (classical) logical principles: what we might call *local* perceptions of validity. Consider:

> All men are mortal,
> Socrates is a man,
> Therefore: Socrates is mortal.

The forceful intuition of validity accompanying the understanding of this inference doesn't seem due either to the centrality of the form of reasoning depicted—although such a form *is* central and generalizable (and recognizable as such because of validity-preserving, and yet arbitrary, substitutions for "Socrates," "mortal," and "man")—nor does it seem due to the meaning of the words "All" and "is." The meaning of these words *does* enable the *expression* of this validity, but it doesn't seem the validity *is due (solely) to* the words having these meanings.

It might be thought that the just-described understanding of how logic can change—that a classical logical principle can lose its status as topic-neutral, while leaving undisturbed many if not most specific instances of it—responds to this local intuition of validity. It doesn't. We have the same compulsive feeling with respect to those (specific) inferences that are to be disallowed in the switch to an alternative logic. When faced with the liar paradox, for example, the compulsion is to unearth and deny a (perhaps tacit) assumption; in so doing the paradox is hoped to be revealed as only apparent (e.g., the liar-paradox-expression "doesn't express a proposition"). One is compelled to *reject* that the sentence can be both true and false. Similar remarks apply to the intuitionistic remodeling of the notion of negation: One feels that double-negation inferences in *every* case are *right*—and regardless of the many observations the intuitionist offers about the supposed treachery of infinity.

33 The move from classical logic to intuitionism or to a paraconsistent logic should be seen this way: e.g., the law of double negation—in intuitionism—is a law applied only in special circumstances. See Azzouni and Armour-Garb 2005.

5

On the other hand, when those gripped by the impression of the strong normative role of classical logic try to justify that role, they reach for the idea that the job of (deductive) inference is "truth-preservation." What's special about the forms of words, and their meanings, when used to express principles of classical logic, and that isn't special to other forms of words and meanings—that we might make up—is that (with respect to a classical form of reasoning) if the premises are true, then the conclusion *must* be true. So the idea would be that the strong normative role of classical logic traces back to a semantic property of the principles of classical logic: that those principles of inference are truth preserving, and the alternatives aren't.[34]

Unfortunately, the truth idiom is far too promiscuous to *exclusively* support a strong normative role for classical logic. Any way we have of characterizing the truth idiom—either in terms of the laws it obeys (e.g., all instances of *"Snow is white," is true if and only if snow is white*) or in terms of metaphysically-rich characterizations of truth involving one or another form of correspondence (e.g., to facts, or to objects bearing such and such properties, and so on)—is too weak *by itself* to do any work. One needs the very principles of logic that are supposedly being given strong normative role by the characterization of "truth-preservation." One might deny this is problematical by invoking Nagel's claim cited in footnote 33. In this case (as it's sometimes said) utilizing the logical principles in the characterization of truth preservation that in turn is to justify the strong normative role of those same principles is "virtuously circular." But virtuous circularity won't procure strong normative role because one needs to justify classical logical principles against competitors that—it must be claimed—themselves don't "preserve truth." Unfortunately, a notion of "truth preservation" can be crafted for *any* alternative logic—if we only substitute the principles of the alternative logic for the classical ones in the characterization of what "truth preservation" comes to. There is no escaping this by suggesting that the notion of "truth preservation" so described is different for different logics, since it's only different because of the differing logical principles accompanying the otherwise same notion of truth.

It's true that what drives belief in the strong normative role of classical logic is our intuitive sense of the validity of classical principles; in particular, what

34 I've cast the point in terms of truth-preserving inferences; but it can be cast in terms of truth—if one is thinking of logical principles as statements rather than as licenses for inference. The strategy is—essentially—the same one.

we seem to sense is a semantic fact about inferences licensed by classical logical principles, and not merely the syntactic fact that certain rules have been correctly applied. This is the source of the impression that if the premises of a syllogism are true, then the conclusion must be true.

But what does this intuition amount to? The modal thought involved ("must be") seems to be: It can't be otherwise. That is, we *can't see* how the premises could be true and the conclusion false. But this isn't a positive characterization of *anything*—it only expresses that we fail to see how something could be. One way of trying to give a positive characterization of this intuition is to take it as a perception of a genuine modality: We recognize that, *regardless of how the world might be*, if the premises are true, then the conclusion is true as well.

Attempting to so construe the intuition of the strong normativity of classical logical principles is too demanding (on *us*) in two ways. First, it takes us as recognizing (somehow) that varying the world in all sorts of ways (while keeping the premises true) keeps the conclusion true as well. This, to put it mildly, seems hard to do.[35] Second, when we try to systematically (and rigorously) characterize this suggested route to validity—by introducing a semantics (a model theory) that (in some sense) varies the world in the ways needed, we again require the use of the very logical principles the characterization of validity is supposed to underwrite. But (also again), since classical logical principles are being pitted against alternatives, this strategy is useless. (We have no grasp of how the world can vary apart from a characterization in terms of whatever logical principles we use: Different logical principles allow the world to vary in ways quite different from how the classical principles allow the world to vary.)

These considerations suggest that if we try to give a positive characterization of the intuition of the strong normative role of classical logic, we fall back on the very principles that we are trying to provide a strong normative role for. This isn't a virtuous circle in a context where proponents of alternative logics can help themselves to exactly the same strategy—and with the same (apparent) degree of success.

35 Do we, as it were, *imagine* the whole world going through all sorts of variations? Really?

6

A sheerly negative intuition—we *can't see* how the premises can be true and the conclusion false—invites *diagnosing* the intuition that classical logic has strong normative role. Thus, the apparent standoff between those who explain away intuitions of strong normative role for classical logic and those who justify such intuitions is more problematic for the latter.[36] In conclusion—therefore—I present one way of so diagnosing these intuitions. This is to take seriously a point made implicitly earlier, but not so far stated loudly: that we have no (introspective) grip on the principles we use to reason—other than the brute sense of compulsion induced in us when we reason—and so (here is the diagnosis) it's no surprise we can't imagine how alternative forms of reasoning are possible.

Here are the pieces needed to explain away intuitions of strong normative role for classical logic. First, one gives a nativist explanation for why we feel the compulsion to reason as classical principles dictate. Such an explanation, of course, doesn't require that the principles themselves be hardwired in us neurophysiologically; the view can get by with the weaker assumption that we have certain (hardwired) mental tendencies, which given the right nurturing, cause the emergence of dispositions to reason in accord with classical logical principles. Second, our use of such principles in reasoning remains tacit *even when we know (empirically) what those principles are.* That is, our conscious grasp of these logical principles amounts *only to* the brute compulsion to believe that if something is the case then something else must be the case as well.

This suffices to explain the impression of strong normative role for classical principles. If the feeling of brute compulsion is relatively rigid—then even if we practice the inferences of an alternative logic for the rest of our lives—we are nevertheless never to have the feeling of "understanding" (that it *must* be this way) that we have when we reason according to the dictates of that compulsion.[37]

36 Some might think this is unfair. But it's hard to see how to sustain the purported perception of strong normative role for classical logical principles *except* via an argument that a genuine perception of validity (i.e., truth preservation) is taking place. On the other hand, *Quine's* attempts to explain away intuitions of strong normative role for classical logic hardly exhaust the strategic options for those attempting to so explain away such intuitions. The diagnostician has more philosophical resources than the justifier (at least in this case).

37 This dramatic remark should be qualified: The feeling of "understanding" will be absent when the dictates of the alternative logic actually *deviate* (in specific cases) from classical principles.

It's worth adding this last reassuring point: In practice we disallow brute intuitions of validity, no matter how powerful they are. Our view of certain of Aristotle's syllogisms takes exactly this form: We diagnose the intuition of validity in such cases by locating a special assumption. We are willing to also explain away (fallacious) probabilistic intuitions—ones psychologically every bit as powerful as the ones that grip us when we reason in accord with classical logical principles. *In practice*, we recognize that intuitions of validity—no matter how powerful—are at best prima facie. The normativity that, in other moods, we presume such intuitions to be indications of, is actually a moving target to be decided ultimately (and instead) by our (collective) pragmatic needs.[38]

Bibliography

Azzouni, Jody 1994. *Metaphysical myths, mathematical practice: the ontology and epistemology of the exact sciences*. Cambridge: Cambridge University Press.

Azzouni, Jody 2004. Proof and ontology in Euclidean mathematics. In *New trends in the history and philosophy of mathematics,* edited by T. H. Kjeldsen, S. A. Pedersen, and L. M. Sonne-Hansen, 117-33. Denmark: University Press of Southern Denmark.

Azzouni, Jody 2005. Is there still a sense in which mathematics can have foundations? In *Essays on the foundations of mathematics and logic*, edited by G. Sica, 9-47. Monza, Italy: Polimetrica International Scientific Publisher.

Azzouni, Jody 2006. *Tracking reason: proof, consequence, and truth*. Oxford: Oxford University Press.

Azzouni, Jody, and Bradley Armour-Garb 2005. Standing on common ground. *Journal of Philosophy* CII(10): 532-544.

Bloor, David 1983. *Wittgenstein: A social theory of knowledge*. New York: Columbia University Press.

Dehaene, S. 1991. *Numerical cognition*. Oxford: Basil Blackwell.

Dehaene, S. 1997. *The number sense*. Oxford: Oxford University Press.

Frege, G. 1967. *The basic laws of arithmetic*, trans. J.L. Austin. Oxford: Basil Blackwell.

Hacking, Ian 1973. Leibniz and Descartes: Proof and eternal truths. In his (2002) *Historical ontology*, 200-13. Harvard: Harvard University Press.

Harris, J.H. 1982. What's so logical about the 'logical' axioms? *Studia Logica* 41: 159-71.

Lakatos, Imre 1976. *Proofs and refutations: The logic of mathematical discovery*. Cambridge: Cambridge University Press.

Mackenzie, Dana 2005. What in the name of Euclid is going on here? *Science* 307, March 4.

Moorehead, Caroline 1992. *Bertrand Russell: A life*. New York: Viking.

Nagel, Thomas 1997. *The last word*. Oxford: Oxford University Press.

Plato 1963. The Meno. In *The collected dialogues of Plato*, edited by E. Hamilton and H Cairns, 353-84. Princeton: Princeton University Press.

Quine, W.V. 1986. *Philosophy of Logic*, 2nd edition. Harvard: Harvard University Press.

Tennant, Neil 2005. Rule-circularity and the justification of deduction. *The Philosophical Quarterly* 55(221): 625-48.

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