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# PLURALISM IN LOGIC

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**Abstract.** A number of people have proposed that we should be *pluralists* about logic, but there are several things this can mean. Are there versions of logical pluralism that are both high on the interest scale and also true? After discussing some forms of pluralism that seem either insufficiently interesting or quite unlikely to be true, the paper suggests a new form which might be both interesting and true; however, the scope of the pluralism that it allows logic is extremely narrow.

There are quite a few theses about logic that are in one way or another *pluralist*: they hold (i) that there is no uniquely correct logic, and (ii) that because of this, some or all debates about logic are illusory, or need to be somehow reconceived as not straightforwardly factual. Pluralist theses differ markedly over the reasons offered for there being no uniquely correct logic. Some such theses are more interesting than others, because they more radically affect how we are initially inclined to understand debates about logic. Can one find a pluralist thesis that is high on the interest scale, and also true?

**§1. The boundaries of logic.** One form of pluralism that strikes me as true though of somewhat limited interest is Tarski's (1936) thesis that there is no principled division of concepts into the logical and the nonlogical, and the related view that there is no principled division between logical truths and truths that don't belong to logic.[1] This seems plausible: there seems little point to a debate between a person who takes first-order logic with identity to be logic and someone who thinks that only first-order logic without identity is really logic. Well, there might be a point if the second person were to claim that some of the axioms of identity that the first person was proposing aren't true, but I'm imagining that the two parties agree on the *truth* of the axioms of identity, they just disagree as to whether they should count as part of logic. Even then, there could be a substantive issue behind the scenes, if they took being logical as associated with some higher epistemological status than is attainable in the nonlogical realm. But it's hard to find any plausible thesis according to which logic has this higher epistemological status; and even if such a thesis were assumed, it would be clearer to put the debate as about whether the laws of identity have this alleged special epistemic status.

Some debates about the demarcation between logic and nonlogic can seem more interesting, but I doubt that the demarcation itself ever really is. Consider, for instance, disputes over ontological commitment. Someone who wants to avoid ontological commitment to sets, but still be able to talk about finitude and infinitude, might hold that Quine's insistence

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[1] The view can concede that there are principled necessary conditions or principled sufficient conditions, or both; just no principled dichotomy.




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on assessing ontological commitment with respect to first-order logic is unnecessarily restrictive: one can talk of finitude and infinitude without use of set theory if one expands first-order logic to include the quantifier 'there are infinitely many' (or more powerful devices from which it can be defined). But here the issue isn't really about the scope of logic, it is whether to allow the quantifier 'there are infinitely many' as primitive. Taking it to be primitive doesn't require a decision on whether it is a logical or nonlogical primitive, or on which truths governing it count as logical truths.

The situation is rather similar in the case of disputes as to whether full impredicative higher order logic should count as logic; but here it is more complicated because there are a larger number of genuine issues that are likely to underlie the dispute, and they interrelate in complex ways. Among the relevant issues are:

(i) the grammatical issue of whether quantifying into the predicate position is legitimate;
(ii) the issue of how the range of the quantifiers would have to be understood (sets? classes, including proper classes? properties? "pluralities" in the second-order case, somehow extended if one allows still higher order logic?);
(iii) the issue of whether it is intelligible to construe the quantifiers impredicatively in these cases. (For instance, does it make sense to talk of impredicative proper classes, or to take one's plural quantifiers to be impredicative when the "plurality" in question is too big to be a set?)
(iv) various issues about ontological commitments of second-order quantifiers: for example, is there a sense in which these aren't real ontological commitments, or alternatively, in which the ontological commitments they make are already implicit in sentences without second-order quantifiers?
(v) the issue of whether second-order quantification is determinate.

These and other issues are genuine, but can be separated from the issue of whether higher order logic is "really logic". Indeed, it is desirable to separate them, because one might give answers of the sort associated with the thesis that "second-order logic is logic" to some but not all of them.

In general, then, I'm inclined to agree with any pluralism based on the arbitrariness of the demarcation between logic and nonlogic. That kind of pluralism doesn't strike me as altogether exciting (though I know it has been denied), but I'll leave that for the reader to judge.

**§2. Radical pluralism.** A *quite exciting* form of pluralism would be the claim that alternative logics (or any meeting minimal conditions) never genuinely conflict. Carnap appears to have suggested this, with his "Principle of Tolerance":

> ...let any postulates and rules of inference be chosen arbitrarily; then this choice, whatever it may be, will determine what meaning is to be assigned to the fundamental logical symbols. By this method, also, the conflict between the divergent points of view on the problem of the foundations of mathematics disappears.... The standpoint we have suggested—we will call it the *Principle of Tolerance* ...—relates not only to mathematics, but to all questions of logic. (Carnap, 1934, p. xv)

In discussing this form of pluralism, it would be a mistake to engage in debates on such questions as:

> "Does intuitionist logic (or quantum logic, or relevance logic) genuinely conflict with classical logic?"

That's a bad question, because it's *obvious* that there are *uses of* intuitionist logic (or quantum or relevance logic) that an advocate of classical logic can engage in. For instance, a classical logician could defend the use of intuitionist logic in certain contexts, either

(i) as appropriate under special readings of some of the connectives, for example $\neg$ and $\rightarrow$; or

(ii) as appropriate under a more restrictive reading of 'logically implies' or 'logical consequence'.[2]

(Beall and Restall, 2006, defend a view of sort (ii): they hold that while classical logic is always appropriate,[3] intuitionist logic sometimes is too, since requiring intuitionist proof typically gives more information than requiring a classical proof. Kripke has argued in lectures for a view of sort (i), according to which intuitionism is best viewed as a view that takes mathematical sentences to assert the existence of mental constructions; the classical $\neg$ and $\rightarrow$ are *applicable* to such sentences, but result in sentences of no mathematical interest ("I haven't performed a construction of ..."); to get a mathematically interesting claim *about mental constructions* one must give $\neg$ and $\rightarrow$ special readings, which turn out to obey intuitionist laws.)

Similar points can be made about quantum logic or relevance logic. Accepting classical logic for quantum mechanics certainly doesn't debar one from pointing out that if one gives nonstandard interpretations of $\wedge$ and $\vee$ in terms of lattice operations in the lattice of subspaces of Hilbert space, the resulting "logic" is nonclassical. And accepting classical logic across the board doesn't necessarily rule out that one might in some special circumstances take an interest in inferences that are not only classically valid but also meet some special relevance conditions, and perhaps these will result in a restricted kind of consequence that accords with some relevance logic.

So in debating the interesting form of pluralism that started this section, one needs to focus on nonstandard logicians who take their preferred logic as *an all-purpose logic*. The radical form of pluralism stated at the start of this section is that there is no genuine debate between advocates of different all-purpose logics.

For instance, consider Michael Dummett, who (as I understand him) conceives of intuitionism as an *all-purpose logic* (Dummett, 1978). In some special circumstances (i.e., when effectively decidable questions are at issue), premises are available which make the use of the logic in those circumstances effectively classical. His view is that *classical logic is never appropriate except as legitimized in special circumstances from intuitionistic principles.*

Analogously for quantum logic, as conceived by Hilary Putnam in the late 1960s (Putnam, 1968): while he may not have been entirely consistent on this, the dominant idea seemed to be that quantum logic was the logic of the world: the distributive laws apply *to a high degree of approximation* to macroscopic objects because quantum superpositions are insignificant for them, but this is merely an approximation that results from the underlying

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[2] I use these terms more or less interchangeably; or more accurately, I take '$\Gamma$ logically implies $A$' to be equivalent to '$A$ is a logical consequence of $\Gamma$'.

[3] That is the official position of the book, though Beall himself would qualify it.

quantum principles in a nondistributive logic. (Similarly, I take the view to have been that classical logic works within mathematics only because mathematical objects have distributivity among their nonlogical properties.)

Of course I'm not defending either Dummett's or Putnam's views. I rehearse them only to make clear what the view of pluralism mentioned at the start of this section would say about them. What it would say is that *even the conflict between Dummett or Putnam and the classical logician* isn't genuine.

That is certainly an exciting form of logical pluralism, and it would be natural to take Carnap's remarks on tolerance as advocating it. But it is hard to imagine how it could be adequately defended. Carnap himself thought it could be defended by supposing that the logical connectives differ in meaning when used by advocates of different all-purpose logics. But the notion of difference of meaning is unhelpful in this context. On some readings of "differ in meaning", any big difference in theory generates a difference in meaning. On such readings, the connectives do indeed differ in meaning between advocates of the different all-purpose logics, just as 'electron' differs in meaning between Thomson's theory and Rutherford's; but Rutherford's theory disagrees with Thomson's despite this difference in meaning, and it is unclear why we shouldn't say the same thing about alternative all-purpose logics. Of course if the connectives differ in meaning between the theories in a more substantial sense—if, for instance, the proponent of one all-purpose logic should translate the proponent of the other non-homophonically—then that might suffice to remove disagreement between the two. But it is hard to see what grounds there could be for a claim of difference of meaning in any such strong sense, or indeed, in any stronger sense than exemplified by the difference of meaning in 'electron' from one theory to another.

**§3. Beall and Restall (1).** An *obviously uninteresting* construal of pluralism is that 'implies' can mean different things, and that on different meanings of it, different statements of form "$\Gamma$ implies $B$" come out true. That's uninteresting because the analog would hold for any word other than 'implies'. *Slightly* more interesting would be that the current meaning of 'implies' is indeterminate, and can be precisified in different ways, with different precisifications affecting which statements of form "$\Gamma$ implies $B$" come out true. I guess that's true—for instance, Grice's notion of implicature probably falls under some precisifications of the ordinary English word 'implies'—but personally I find it hard to get excited about issues related to the extent of indeterminacy in English words.

Beall and Restall reawakened discussion of logical pluralism a few years back, and their version is in the same ballpark as these, but better. It strengthens the obviously uninteresting version, by *restricting to meanings of a common type*: meanings of form "For all cases, if all members of $\Gamma$ are true in that case then $B$ is true in that case." And they argue that some standard nonclassical logics, like intuitionist logic and relevance logic, can be generated in this way by an appropriate construal of 'cases'.[4] I have some doubts about the last point as regards relevance logic,[5] but this is not a point I want to press.

[4] The "cases" aren't necessarily actual: they can include possible cases, or on some construals even impossible ones.

[5] Beall and Restall motivate the invalidity of $A, \neg A \vdash B$ and $A, A \to B \vdash B$, but in a way that suggests that the problem in both cases lies not in the corresponding single-premise inferences $A \wedge \neg A \vdash B$ and $A \wedge (A \to B) \vdash B$, but rather in $\wedge$-Introduction. This doesn't accord with standard relevance logics.

That doubt aside, I'm prepared to concede that Beall–Restall pluralism is correct. But while it is more exciting than the view that 'implies' can mean different things, I'm not sure that it's exciting *enough*. If it's unexciting that 'implies' can have many meanings, why is it so exciting that it may have many meanings *of this form*? This still falls *far* short of the kind of pluralism that says that advocates of apparently competing *all-purpose* logics don't really disagree.

**§4. Digression on the meaning of connectives.** I have another main worry about the Beall–Restall approach to pluralism, but before stating it I'd like to comment on a point that they attach great importance to: the distinction between pluralism based on difference in meanings of the ground-level connectives and pluralism based on difference in meaning of the word 'implies'. Carnap's pluralism was apparently of the former sort: the different languages that he allowed the logician to construct apparently differed in the meanings of their connectives. Beall and Restall think that what's interesting is to have pluralism of consequence in languages whose connectives have the same meaning. Thus in the examples from the previous section, the classical logician who defends the use of intuitionism on Kripkean grounds wouldn't count as an illustration of Beall–Restall pluralism, but the classical logician who defends it on the ground of forcing the search for more informative proofs would.

This distinction does seem to me to have some interest, but I'm skeptical that it can be made as sharply as many people apparently think. The reason is that the notion of sameness of meaning across different logics strikes me as quite obscure, and insofar as it is clear does not seem to be very well-behaved. There is no space here for a full treatment of this point (which should be familiar from Quine), and it isn't of crucial importance to my paper, so I'll just confine myself to an illustration. Consider three logics: classical logic C, intuitionist logic I, and some logic P of the kind that has been advocated in recent years as a means of maintaining naive assumptions about truth and property-instantiation even in the face of the semantic and property-theoretic paradoxes. P might be either one of Priest's dialetheic logics that contain a detachable conditional (e.g. Priest, 2002), or my own nondialetheic logic with detachable conditional and restrictions on excluded middle (Field, 2008). A feature of both these alternatives for P is that they employ both (i) a negation operator $\neg$ that (in combination with the usual $\wedge$ and $\vee$) obeys the deMorgan laws and the equivalence of $\neg\neg A$ to $A$, and (ii) a conditional not definable in terms of $\neg$ and $\vee$ (or $\neg$ and $\wedge$). From the conditional and the falsum $\perp$ we can define another unary operation, $A \rightarrow \perp$, with *some* "negation-like" properties but which is not equivalent to the deMorgan negation $\neg$: for instance, some deMorgan laws would fail for it, and applying it twice to a sentence results in something inequivalent to the original sentence.

Now let's ask how to intertranslate the logical connectives between the different logics—or rather, between three people C, I, and P who use these logics *as their all-purpose logic*. Let's consider them pairwise.

(i) What should a classical logician say about what Michael Dummett (or some other person who takes intuitionist logic as their all-purpose logic) calls negation? I think the natural attitude to take is to translate the term homophonically, since there is no better intuitionist candidate for 'not' as the classical logician understands it. (Of course it would be different if the classical logician were translating someone who generally employs classical logic but employs intuitionist logic in mathematics on the grounds Kripke discusses: such a person uses intuitionist negation as a specially explained connective employed only in connection with mathematical constructions, which can live alongside of classical negation although classical negation has no mathematical interest in that context.)

(ii) What should a classical logician say about what someone who advocates a deMorgan logic P of the sort I've mentioned? In such theories, the deMorgan negation behaves far more like classical negation than does the operator $A \rightarrow \bot$, so a classical logician is likely to translate deMorgan negation by 'not'.

(iii) But now suppose the advocate of P is translating Dummett: should she translate the intuitionist negation as deMorgan negation or as $A \rightarrow \bot$? I'd say the latter: not only does the intuitionist typically define negation as $A \rightarrow \bot$, but (more significantly) this correlation yields a closer correspondence in laws.

If these remarks are right, then *proper translation* of connectives is not transitive, so there is no notion of *intertheoretic sameness of meaning* of connectives that proper translation preserves.

Of course it might be said that translation is a highly pragmatic matter, which has no direct bearing on *sameness of meaning*. But is there any reason to think that we have a clear notion of intertheoretic sameness of meaning that would give different results? Certainly anyone who claims that we do have one owes an account of how decisions about sameness of meaning are to be made in situations like this, and why they result in transitivity.

It may be that in the context Beall and Restall are operating in, the problem is not so severe. For they are considering a kind of pluralism where the same speaker can employ different consequence relations in different contexts: for example classical consequence when concerned only with real-world preservation of truth, intuitionist or relevance consequence when additional considerations are imposed. In this single-speaker case, where there is clearly no variation in overall theory, issues about intertheoretic sameness of meaning don't arise, and the speaker can simply stipulate that he is using the term 'not' with the same meaning in the context of both consequence relations. But while this defense is open to Beall and Restall, it is not open to people who want to take their distinction between variation of meaning in ground-level connectives and variation of meaning in the notion of implication and apply it in a context closer to the one of Section 2.

**§5. Beall and Restall (2): The nature of consequence.** In Section 3 I expressed one worry about Beall–Restall pluralism, but I have another that is in some ways more important. Recall that Beall and Restall focused on definitions of consequence of the form

(F) $\Gamma$ implies $B$ (i.e. $B$ is a consequence of $\Gamma$, i.e. the argument from $\Gamma$ to $B$ is valid) if and only if for all cases, if all members of $\Gamma$ are true in that case then $B$ is true in that case.

They then say that 'case' can be construed in a variety of ways, and their pluralism consists in the fact that different construals lead to variation in the extension of 'implies'. My new reservation about this is that I don't think that implication ought to be understood along the lines of (F), *however* 'cases' is understood. Beall and Restall are presupposing that a nonpluralist view takes implication to be given by (F), on one construal of 'cases', and that the error in the view is its failure to recognize other construals of 'cases'. But I contend that issues of pluralism aside, implication isn't to be explained by a definition of form (F).

This may seem surprising: don't logic books standardly explicate implication or consequence in terms of truth-preservation in all models?

(M) $\Gamma$ implies $B$ (i.e. $B$ is a consequence of $\Gamma$, i.e. the argument from $\Gamma$ to $B$ is valid) if and only if for all models M, if all members of $\Gamma$ are true in M then $B$ is true in M.

Yes they do, and of course I have no complaint about this. So first I need to explain why this is irrelevant to Beall and Restall's purposes: there is a constraint (*) that they need to impose on how 'cases' in (F) is to be understood, which precludes them from understanding "cases" as models. Once this is explained, I'll then argue that unless we are to give up a very important feature of the normal meaning of 'implies', we can't maintain that 'implies' (or to accommodate pluralism, any version of 'implies') is *even extensionally* capturable in a formulation of form (F) that meets Constraint (*).

***5.1. The irrelevance of model-theoretic accounts to pluralism.*** In logic books, one typically defines implication (consequence, validity) via (M)—using a definition of 'model' appropriate to the logic in question. But "defines" here is used in the mathematician's sense of giving a mathematically useful explication, one that is intended only to capture the target notion extensionally and to provide a useful means for investigating the laws that that target notion obeys. Such a "definition" is not normally intended to capture the ordinary meaning of 'implies', and there are at least two important reasons why it would be totally inadequate for that purpose.

One of these reasons is that by varying the definition of 'model', this approach defines a large family of notions, 'classically valid', 'intuitionistically valid', and so on; one needn't accept the logic to accept the notion of validity. A classical logician and an intuitionist can agree on the model-theoretic definitions of classical validity and of intuitionist validity; what they disagree on is the question of which one coincides with genuine validity. For this question to be intelligible, they must have a handle on the idea of genuine validity independent of the model-theoretic definition. Of course, a pluralist will contest the idea of a single notion of genuine validity, and perhaps contend that the classical logician and the intuitionist shouldn't be arguing. But logical pluralism is certainly not an entirely trivial thesis, whereas it would be trivial to point out that by varying the definition of model one can get classical validity, intuitionist validity, and a whole variety of other such notions.

A second reason that model-theoretic definitions don't capture the ordinary meaning of 'implies', emphasized by Kreisel (1967) and Boolos (1985), is that it is essential to the model-theoretic definition that models have sets as their domains; otherwise truth-in-a-model wouldn't be set-theoretically definable, and so validity wouldn't be set-theoretically definable, and that would undermine the model-theoretic point of the definition. But if models have sets as their domains, then the full universe (since it includes every set) can't be the domain of a model. In other words, *if we were to understand 'cases' as models, then there would be no case corresponding to the actual world.* There is no obvious reason why a sentence couldn't be *true in all models* and yet *not true in the real world.*

This connects up with the previous point: the intuitionist regards instances of excluded middle as *true in all classical models*, while doubting that they are *true in the real world.*

As Kreisel convincingly argued, the fact that there is no model corresponding to the real world doesn't defeat the model-theoretic explications of 'classically valid', 'intuitionistically valid', and so forth: there are good informal reasons to think that these explications are extensionally correct, in which case the extensional laws that govern them govern the target notions as well. But the fact that these explications serve the model-theorist's purposes doesn't make them good definitions in any sense of definition relevant to the investigation of issues like logical pluralism.

No doubt Beall and Restall recognize this: I can find nothing in their account to suggest that they would have allowed an identification of "cases" with models, and their discussion would have proceeded in an entirely different manner had they done so. So I think that when they proposed (F), they were implicitly adopting the following constraint:

(*) It is assumed that the "all cases" in (F) must include an actual case, or at least a case for which truth in that case is equivalent to truth.[6]

In the rest of this section, then, I will take it for granted that in discussing (F), Constraint (*) is in play: that is, that *truth in all cases* implies *truth*.

***5.2. Why one shouldn't believe one's logic truth-preserving.*** I will now argue that unless we are to give up a very important feature of the normal meaning of 'implies', we can't maintain that 'implies' can be *even extensionally* captured in a formulation of form (F) that meets Condition (*).

The important feature of implication that I want to stress is that it has a broadly normative component. I don't mean that 'implies' is to be defined in terms of 'ought' or other uncontroversially normative notions—it is better to view it as a primitive notion, not defined at all—but that we accept norms that connect our beliefs about implication to our views of proper epistemic behavior. Very roughly, our views about implication constrain our views about *how we ought to reason*, or (perhaps better) about *the proper interrelations among our beliefs*. For instance, we saw in Section 5.1 that a nonclassical logician can accept that there is a well-defined notion of classical validity, but doubt that it corresponds to genuine validity. It seems to me that the reason a nonclassical logician has these doubts is that he doesn't think classical validity is *how we should reason*: he thinks that it imposes the wrong *constraints on our beliefs*.

There is more than one way to spell this out. My preferred way (see Field, 2009b, for more detail) is in terms of degrees of belief.[7] A partial constraint on belief is this:

> If one knows [is certain] that $A$ implies $B$ then one's degrees of belief should be such that one's degree of belief in $B$ is at least that of $A$.

More generally, and representing degree of belief by $D$:

> If one knows [is certain] that $A_1$ through $A_n$ together imply $B$ then one's degrees of belief should be such that $D(B) \geq D(A_1) + \ldots + D(A_n) - (n-1)$.

(In particular, if one knows that $A_1$ through $A_n$ together imply $B$ then one shouldn't be certain of $A_1$ through $A_n$ without being certain of $B$.) The $n = 0$ case says that if one knows [is certain] that $A$ is a logical truth, one's degree of belief in it should be 1. Of course it's an *epistemic* normativity that's at issue here.

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[6] Thanks to JC Beall for pressing me to make Constraint (*) explicit.

[7] Those who think that belief is an all or nothing matter needn't resist this: they can take degrees of belief to be confined to the values 0 and 1, where 0 represents nonbelief rather than belief in the negation. This will require allowing $D(A)$ and $D(\neg A)$ to both be 0, but I do not preclude this in what follows.

What I will now attempt to show is that any remotely similar normative role for implication is incompatible with taking implication to involve the preservation of truth in all cases (if the cases include an actual case).

This may seem puzzling, because it is apparently possible to *derive* that implication coincides with necessary truth preservation, from very minimal principles. Consider the following sequence of claims:

(1) The inference from $A_1, \ldots, A_n$ to $B$ is valid.
(2) The inference from $\text{True}(\langle A_1 \rangle), \ldots, \text{True}(\langle A_n \rangle)$ to $\text{True}(\langle B \rangle)$ is valid.
(3) The inference from $\text{True}(\langle A_1 \rangle) \wedge \ldots \wedge \text{True}(\langle A_n \rangle)$ to $\text{True}(\langle B \rangle)$ is valid.
(4) The sentence $\text{True}(\langle A_1 \rangle) \wedge \ldots \wedge \text{True}(\langle A_n \rangle) \rightarrow \text{True}(\langle B \rangle)$ is valid.

These seem equivalent: (1) to (2) by the usual truth rules, (2) to (3) by the usual rules for conjunction, (3) to (4) by the usual rules for the conditional. But validity for sentences is logically necessary truth (or necessary truth by virtue of form—I shall not distinguish), so (4) says that the inference necessarily preserves truth (by virtue of form).

This argument looks very persuasive. But it is fallacious! It turns on principles (the two truth rules and the two $\rightarrow$ rules) that are jointly inconsistent, as Curry's paradox shows. Curry's paradox involves a sentence $K$ that is in some suitable sense equivalent to

$$\text{True}(K) \rightarrow \perp,$$

where $\perp$ is some absurdity (e.g. 'I am the Pope'). As is well-known, there are natural ways of constructing such sentences. (For instance, in passing a department store window I might see a TV display, showing what I take to be my least favorite politician, leading me to say "If what that guy is now saying is true then I'm the Pope". If in fact the display is of those passing the window, then my utterance is "equivalent given the empirical facts" to the claim that if it itself is true then I'm the Pope.) Given this, there is an equivalence between

$$\text{True}(K)$$

and

$$\text{True}(\langle \text{True}(K) \rightarrow \perp \rangle).$$

But now we reason as follows: Suppose, for the sake of argument, that $\text{True}(K)$, that is $\text{True}(\langle \text{True}(K) \rightarrow \perp \rangle)$. But then **by the rule of True-Elimination** (from $\text{True}(\langle A \rangle)$ infer $A$), we infer $\text{True}(K) \rightarrow \perp$. But from this and the supposition $\text{True}(K)$, we infer the absurdity $\perp$, using **Modus Ponens, *aka* $\rightarrow$-Elimination**. So far, nothing terribly surprising: we haven't proved $\perp$ absolutely, we've just proved it from the assumption $\text{True}(K)$. But now **the rule of $\rightarrow$-Introduction** tells us that this constitutes a proof (not from assumptions) of $\text{True}(K) \rightarrow \perp$. And then **the rule of True-Introduction** (from $A$ infer $\text{True}(\langle A \rangle)$), gives us $\text{True}(\langle \text{True}(K) \rightarrow \perp \rangle)$, which is equivalent to $\text{True}(K)$. So now we've proved $\text{True}(K)$, *without assumptions*. And that together with the earlier argument from $\text{True}(K)$ to $\perp$ yields a proof of $\perp$ without assumptions. We've apparently proved that I'm the Pope, a promotion that few would have expected.

Short of very desperate measures, it seems that the fallacy in this argument must lie in the use of one of the four bold-faced rules: the introduction and elimination rules for 'True', and the introduction and elimination rules for the conditional. At least one of these rules needs restriction; opinions can differ on which one or ones is the culprit. But all four of the rules were used in the argument that validity coincides with logically necessary truth-preservation! The argument that everything that preserves truth by logical necessity is valid uses the two truth rules plus modus ponens; the argument that every valid inference preserves truth by logical necessity uses the two truth rules plus $\rightarrow$-Introduction.[8]

(I've heard it claimed that on Tarskian theories one can maintain the truth-preservingness of all four rules, but this is incorrect. The best that a theory with a single Tarskian truth predicate can do is say that if $\Gamma$ classically implies $B$, *and $B$ and the members of $\Gamma$ don't contain 'true'*, then if all members of $\Gamma$ are true, so is $B$. But since the Tarskian employs classical logic in connection with sentences that *do* contain 'true', this is not nearly general enough. Tarskian theories with a sequence of predicates 'true$_\alpha$' fare no better: the best the Tarskian can do is say, for each ordinal notation $\alpha$, that if $\Gamma$ classically implies $B$ (or implies it in the Tarskian theory) *and $B$ and all members of $\Gamma$ are in a sublanguage in which 'true$_\alpha$' isn't definable*, then if all members of $\Gamma$ are true$_\alpha$, $B$ is true$_\alpha$. The totality of these claims as $\alpha$ varies covers each instance of the naive claim about implication, but there is no way to maintain the claim about implication with a single truth predicate. And indeed, the "truth-preservation" of modus ponens for sentences of level $\alpha$ must be stated in terms of a broader truth-predicate truth$_{\alpha+1}$, and the "truth-preservation" of truth$_{\alpha+1}$-introduction for sentences of level $\alpha$ must be stated in terms of a still broader truth-predicate truth$_{\alpha+2}$.)[9]

So far I've argued that *the derivation* of the equivalence of implication to truth-preservation by logical necessity requires principles that can't be right. But I claim something stronger: the *conclusion* of this alleged derivation can't be right; that is, the normative criterion and the necessary truth-preservation criterion are bound to come apart.

Indeed, for *nearly* every way of dealing with the truth-theoretic paradoxes, **it is inconsistent to hold that the logic one accepts *actually* preserves truth.** By 'the logic one accepts' I mean the logic that one thinks should normatively govern one (e.g. govern one's inferential practice, or the interrelations among one's beliefs, in the sense sketched at the start of Section 5.2). And I mean to include in this logic not just the logic of the usual sentential connectives and quantifiers, but also the logic of the truth predicate. At least in this broad sense of logic, virtually no way of dealing with the paradoxes can consistently take its own logic to be truth-preserving.[10]

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8 Stephen Read pointed out to me that one might take the transition from (1) to (2) as a primitive form of inference, not based on T-Introd and T-Elim. (T-Introd and T-Elim would still be needed for the inference from (2) to (1).) However, that primitive form of Inference entails a limited form of T-Introd, namely that theorems are closed under T-Introd (take the case where $n = 0$); and this limited form is all that was actually used in the Curry paradox.

9 I've assumed in this paragraph that the Tarskian theory takes $\text{True}_\alpha(\langle A\rangle)$ to be *meaningless* when $A$ contains 'True$_\beta$' for $\beta \geq \alpha$. Similar points hold for the variant view that takes $\text{True}_\alpha(\langle A\rangle)$ to be *false* in those circumstances, but the details differ slightly.

10 One can get at least a slightly weaker form of this conclusion by reflecting on Gödel's second incompleteness theorem. For if a theory could prove that all of its rules of inference preserve truth (including as a degenerate case that all its axioms are true), it would almost certainly be able to prove that all its theorems were true, and from this derive its own consistency; so by the second incompleteness theorem, it would have to be inconsistent. (The two assumptions buried in the 'almost certainly' are that the theory allows the truth predicate to appear in inductions and that it implies the nontruth of at least one sentence.)

With a small additional assumption (one that allows us to use the truth predicate to finitely axiomatize), the argument extends to show that for any consistent theory there are specific rules that it can't show to be truth-preserving. See Field (2008, Section 12.4).

Even then, this conclusion is weaker than the one in the text: it involves the unprovability of a claim, not that adding the claim to the theory yields inconsistency. However, the argument applies in particular to theories that contain the claim that all their rules preserve truth: such theories must be inconsistent. This doesn't directly show that you get inconsistency from a theory $T$ by adding

This sounds very dramatic, but it *needn't* really be so bad, because on some solutions the logic has a kind of *restricted* truth-preservation property. Though I won't go into details, I'll illustrate this below.

Before getting to the illustrations, I want to emphasize that I'm not denying that one could stipulate that 'valid' is to mean 'necessarily truth-preserving'. Of course one could. The claim is only that if one does so, then one loses the natural tie between validity and inferential practice—roughly, the normative component of the notion, the connection between validity and what governs our degrees of belief. The question of whether to tie the word 'valid' to this normative component or to unrestricted truth-preservation is obviously verbal.

Here, briefly, are a couple of illustrations.

**A.** There are many resolutions of the semantic paradoxes that

(i) accept reasoning by modus ponens (the inference from $A$ and $A \to B$ to $B$), but

(ii) don't accept all instances of $\text{True}(A) \wedge \text{True}(A \to B) \to \text{True}(B)$.[11]

Most resolutions that employ nonclassical logic fall into this category, as do some classical-logic resolutions.

**A1.** Most nonclassical-logic resolutions restrict classical logic so as to attain the complete intersubstitutivity of $\text{True}(\langle p \rangle)$ with $p$; so if they are to satisfy (i) and (ii), they must accept modus ponens while not accepting the schema

(iii) $A \wedge (A \to B) \to B$.

This "law" follows from modus ponens if you accept $\to$-Introduction (and $\wedge$-Elimination), but these theories reject $\to$-Introduction. They accept modus ponens but take (iii) to fail (along with many other classical laws involving nested conditionals in which the same sentence letter has multiple occurrences that are nested within conditionals to different degrees).

**A2.** Classical-logic resolutions, of course, must accept the schema (iii), but they must also reject the complete intersubstitutivity of $\text{True}(\langle p \rangle)$ with $p$; so classically, the acceptance of (iii) doesn't automatically entail the acceptance of $\text{True}(A) \wedge \text{True}(A \to B) \to \text{True}(B)$, and some classical-logic resolutions exploit this gap.

Most theories of type **A** are compatible with a kind of *restricted truth preservation*: though they don't accept that modus ponens preserves truth in general, they do accept that it preserves truth among "normal" sentences, and it is only "normal" sentences that they are committed to accepting. For instance, a paradigm case where modus ponens is accepted but $\text{True}(A) \wedge \text{True}(A \to B) \to \text{True}(B)$ is suspect is when $B$ is an obvious falsehood such as 'I am the Pope' and $A$ is the corresponding Curry sentence; in this case, the views in question don't allow for the acceptance of the premises of the modus ponens, so the failure to regard modus ponens as preserving truth in this case doesn't undermine the utility of modus ponens. (It might be thought that because of this, an

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to it the claim $B$ that all rules *of* $T$ preserve truth, for that becomes a new axiom (degenerate rule) of the resulting theory $T+B$: to argue for the inconsistency of $T+B$ by the second incompleteness theorem you need that $\text{True}(\langle B \rangle)$ is a theorem of $T + B$. However, in theories that allow for the inference from $A$ to $\text{True}(\langle A \rangle)$, this loophole is closed.

11 Indeed, adding all such instances would make the resolutions inconsistent. However, not all of these theories accept *reductio* reasoning, and in those that don't, the negation of these instances won't be part of the theory.

alternative account of valid inference would be available: a form of inference is valid if and only if it preserves truth *in all applications to normal premises*. Unfortunately, the idea is ultimately unworkable: if one tries to characterize 'normal sentence' precisely, one either ends up with a characterization that is unduly restrictive or one ends up unable to accept the claim that modus ponens preserves truth among normal sentences.)

Again, one can if one likes declare that according to theories of this sort, modus ponens isn't valid, because it isn't unrestrictedly truth-preserving. But theories of this sort do give modus ponens a privileged place regarding normative commitment: on these theories, a commitment to "the Curry sentence is true" and "if the Curry sentence is true then I'm the Pope" normatively requires a commitment to "I'm the Pope", even if one can't express this commitment by saying that if the first two are true then so is the third. Once one grants that the normative commitment and the truth-preservation come apart, it's a matter of uninteresting verbal legislation which to mark with the term 'valid'.

**B.** A second illustration—more popular, though far less to my liking—is a resolution of the paradoxes known as the Kripke–Feferman theory, or KF. This is a theory that employs classical logic, but says that some of its inferences don't preserve truth. Indeed, *it accepts classical theorems that it says aren't true*, such as $\mathrm{True}(L) \vee \neg\, \mathrm{True}(L)$, where $L$ names the Liar sentence. This strikes me as extremely weird. Note that on this view, there's no way to remove the counterintuitiveness by a notion of restricted truth preservation: the distinction of restricted truth-preservation from full truth-preservation applies only to inferences with premises, not to theorems.

But weird or not, this is a popular view, and the issue about 'valid' arises for it. Usually, KF is described as taking excluded middle to be *valid despite having untrue instances*. But Tim Maudlin accepts KF, but identifies validity with necessary truth preservation, so he describes his view as one on which classical reasoning is *invalid but should be accepted*.

More fully, Maudlin *believes* $\mathrm{True}(L) \vee \neg \mathrm{True}(L)$, while regarding it untrue. (Indeed, he believes $\neg \mathrm{True}(L)$, while regarding it untrue.) More generally, *his degrees of belief accord with classical logic*, in the sense defined earlier. On the normative criterion, he's a classical theorist.

And to return to views of sort **A** that accept reasoning via Modus Ponens despite holding that it doesn't preserve truth: the advocate of such a view is likely to have degrees of belief governed by the rule

$$D(B) \geq D(A) + D(A \rightarrow B) - 1,$$

so that if they believe $A$ and $A \rightarrow B$ each to degree $\geq 1 - \in$ they will believe $B$ to degree at least $1 - 2 \in$. That holds even when $A$ and $A \rightarrow B$ are "abnormal" sentences like the Curry sentence. I think it's reasonable to regard this constraint on degrees of belief as the most central component of the notion of validity.

**§6. Normativity.** Where are we? With regard to the Beall and Restall view of logical pluralism, I've suggested that

(i) it isn't that interesting to show that 'implies' or 'valid' can mean different things—even different things of the general form "preserves truth in all cases";
(ii) this is especially so since the normal meaning of 'implies' doesn't seem to be of form "preserves truth in all cases".

But I think that there is more promise of achieving an interesting pluralism by another route.

A key idea behind this alternative route is one I've already raised in the discussion of Beall and Restall: it's that logical validity has a normative component. Given this, mightn't we get an interesting logical pluralism by arguing for normative pluralism? It is, of course, *epistemic* normativity that is tied to the notion of logical validity. Is pluralism about epistemic normativity a believable doctrine?

Quite independent of logic, I think there are strong reasons for a kind of *antirealism* about epistemic normativity: basically, the same reasons that motivate antirealism about moral normativity, or about aesthetic goodness, extend to the epistemic case. (For instance, (i) the usual metaphysical (Humean) worry, that there seems no room for "straightforward normative facts" on a naturalistic world-view; (ii) the associated epistemological worry that access to such facts is impossible; (iii) the worry that such normative facts are not only nonnaturalistic, but "queer" in the sense that awareness of them is supposed to somehow motivate one to reason in a certain way all by itself.) And while there are different ways in which one might articulate an antirealism about normativity, I think most of them involve ideas that are at least *in some sense* pluralist.

In what follows, I will give a quick sketch of a kind of antirealism about epistemic normativity that I've articulated elsewhere (e.g. Field, 2009a), and sketch the sense in which it makes room for epistemological pluralism; then I will focus in particular on pluralism about logic.

**§7. Relativist expressivism and pluralism.** The kind of normative antirealism I advocate might be called 'relativist expressivism'. It includes the following:

- There are all sorts of different possible overall norms for forming, evaluating, and revising beliefs.
- It's natural to idealize and say that a person employs one of these overall norms in forming, evaluating, and revising his or her beliefs, though a somewhat less idealized picture would be that there's a bunch of overall norms that the person vacillates between.
- A person will typically also have views about "how good" various norms are.
- These views are resultants of two things: one's beliefs about what the results of using the norms are likely to be, and one's attitudes toward such results.

As an example of the last point, we think that reasoning by counterinduction is likely to be bad, because we think it will lead to massive error, and we negatively evaluate having massively erroneous beliefs. Of course, the conclusion that it will lead to massive error is, like all of our beliefs, based on using the norms we use—the unique norm, on the highly idealized story, and the various different ones on the less idealized story.

So the picture is: we use our norms to evaluate both our own norms and other norms.[12] If it happens that on our evaluation, our own norms come out better than their competitors, there's no pressure to change them. If we get an evaluation on which another norm comes out better, there is some pressure to change (though also some pressure to look at the evaluation more closely—there is no simple general answer about what the outcome will be in this situation). There are arguments purporting to show that such situations where

[12] Obviously we don't choose an inductive method ab initio by deciding which best is likely to satisfy our goals: we couldn't, since we'd need an inductive method to decide that. We don't choose an inductive method ab initio at all, we acquire one by genetics and training; goals, and the evaluation of methods, come later.

we rationally evaluate a norm other than the one we employ as better than the one we employ can never arise, but they rest on quite contentious premises. (See Section 11 of Field, 2009a.)

From this perspective, it doesn't make much sense to talk of a norm being *correct* or *incorrect*. It does make sense to talk of one norm as *better* or *worse* than another; but this is far different than talk of correctness and incorrectness of norms, for two reasons.

First, judgments of one norm being better than another are relative to our goals and to our evaluations of the possible trade-offs among them. Presumably for epistemic norms, the only goals of interest concern attaining truth, avoiding falsity, and so forth. But it isn't a simple matter of trading off the value of achieving truth versus avoiding falsehood. For instance, we need to take account of the value we put on *approximate* truth, on achieving truth *quickly*, and so forth; and we regard truths about some matters as far more important than truths about others. There are a vast number of different truth-oriented goals that we have, and trade off among; and different trade-offs among the truth-oriented goals are likely to support different comparative evaluations of competing epistemic norms.

Second, and more important: even relative to our epistemic goals and a specification of their weights, there's no reason to think that there's a uniquely best norm. For instance, there might be a sequence of better and better norms for achieving the goals; in addition, there might be ties and/or incomparabilities "arbitrarily far up".

The upshot of this is a kind of normative pluralism: there are lots of possible norms, and we can rate them as better or worse (relative to our epistemic goals), but there's no reason to think there's a uniquely best one. And talk of correctness of epistemic norm just doesn't seem to make sense.

Such a normative pluralism seems to me overwhelmingly plausible in the case of inductive norms. No one knows how to formalize a plausible candidate for an inductive norm that anyone actually follows. Still, the crude models that have been formalized seem a reasonable guide. And in all these models, there is a reliance either on adjustable parameters (as in the Carnap and Hintikka continua of inductive methods), or on a simplicity ordering of hypotheses with few clear constraints, or some such thing. Obviously some of these methods are a whole lot better than others: some are bad because they learn too slowly, others on the opposite ground that they jump too quickly to conclusions; some are bad because they are unable to take certain features of our evidence into account, others on the opposite ground that they take seriously features of evidence (such as time of day when the observation was made) when there is no special reason to do so. One wants methods that avoid these extremes, that are somewhat in the middle on the scales (rather vaguely) suggested. But is there any reason whatever to think that there's a uniquely best point, for example a uniquely best value of the adjustable parameters? Or worse, to think that one value is not only uniquely best, relative to whatever goals we may have, but uniquely correct? I'd think clearly not.

**§8. Applying relativist expressivism to logic.** But what about the case of deductive logic? I've argued that logical implication is best thought of as a notion with a normative component. If so, then it would seem that a relativist expressivist view of norms generally should hold for logic in particular. And basically, I think it does—though with a difference.

The basic idea of relativist expressivism as applied to logic is

- There are different possible logics.
- We can evaluate these possible logics for how well use of them would satisfy various goals. In the evaluation we of course use a logic, in analogy with what

happens in the inductive case, but there is no good argument that in using a logic **L** to evaluate itself and other logics, **L** will always come out best in the evaluation. Indeed, there are clear cases to the contrary. For instance, we may use standard logic to discover the semantic paradoxes and to reason about how they are best resolved, and this resolution may well involve a weakening of standard logic.[13] (Of course, we don't "choose an initial logic" by deciding which one best meets our goals: we couldn't, since we'd need a logic to decide that. Rather, we start out with whatever our genetics and training build into us; goals come in only at a more reflective stage, for instance when we consider making revisions in the logic we are initially endowed with.)

- Whether because of a detailed evaluation or simply by intuitive assessment, we regard some logics as better than others (for a given goal).[14] We certainly don't regard all logics as equally good (any more than we regard all inductive methods as equally good): relative to almost any goals one might have, a logic that allows you to affirm the consequent is a bad logic, in that it will have a deleterious effect on achieving those goals.
- But it isn't obvious that there need be a uniquely best logic for a given goal, much less that we should think of one logic as "uniquely correct" in some goal-independent sense.

The last point gives the sense in which the relativism involves a kind of logical pluralism.

However, I think that there's an important difference between the case of logic and the case of other epistemic norms, which is that the goals that most of us advocate do very much more to settle the logic. While some (e.g. relevantists) may advocate additional goals, I take it that for almost everyone logic is supposed to preserve truth, *at least in application to premises we accept*, and to do so by logical necessity. Indeed, all else being equal we'd presumably *like* a logic that preserves truth in complete generality (and to do so by logical necessity). Unfortunately, we saw in Section 5.2 that this more general goal is one we can't consistently believe our logic to meet, unless we are to unduly restrict what counts as logic. (On a Tarskian view of truth, one can't even *think* the claim that our logic meets this goal; on views that allow truth predicates that are unrestricted and unstratified, one can think it but can't consistently believe it.)

But though there seems to be no interesting way to get a logic for which we can consistently hold that it preserves truth with complete generality, we *can* get a logic in which all the theorems are (necessarily) true, and I think we should want this since it's a special case of the goal of wanting the inferences to preserve truth when applied to true premises. *And relative to this goal, many logics will genuinely disagree*: one logic will contain theorems whose truth will be rejected by proponents of the other logic.

One place this disagreement comes out is in treatments of the paradoxes. Let $L$ name the Liar sentence, and consider the sentences

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13 Indeed, if we construe 'standard logic' generously, to include a pre-theoretic "logic of truth" in which True($\langle p \rangle$) is always equivalent to $p$, then we *must* revise logic: the only question is whether the revision is to be in the logic of truth or in the logic of the sentential connectives (or both).

14 The term 'goal' here might suggest a very local goal, so that one logic might satisfy the goals we have for reasoning in one area while another logic satisfies our goals for reasoning in another area. An analogous possibility arises with inductive methods in place of logics. But what I primarily have in mind is more global: a goal for an *all-purpose* inductive method or logic.

(5) True($L$) $\vee \neg$ True($L$)

(6) True($L$) $\wedge \neg$ True($L$).

The classical logician, dialetheist, and paracomplete theorist differ *in part* in their views about the *implication* relations these sentences enter into: that is, over whether (5) is *implied by* everything and whether (6) *implies* everything. But these theorists *also* disagree in their *ground-level attitudes* toward the sentences: the classical theorist accepts (5) and rejects (6), the dialetheist accepts both, and the paracomplete theorist rejects both. There's nothing normative about sentences (5) and (6), so this is a genuine disagreement at the ground level (barring the implausible view that there is sufficient "difference of meaning" in their connectives to undermine the apparent disagreement). And relative to the minimal goal that logical theorems be true, this disagreement over logic is straightforwardly factual.[15] So some logical disagreement is genuine.

Does this eliminate all role for logical pluralism? No. In particular, I've given content to the conflict being genuine only in cases where the logics disagree in their theorems—that is, where one logic has a theorem that not only isn't a theorem of the other but that proponents of the other will disagree with.[16] But there are examples of pairs of logics that agree in their theorems but disagree in their valid inferences. Nonetheless, the interest of logical pluralism is vastly reduced when confined to such examples.

Indeed, in many of the cases when logics have different valid inferences but the same theorems, it may be that proponents of the two logics differ only verbally. Above, I suggested the following connection between implication and degrees of belief:

(I) If one knows [is certain] that $A_1$ through $A_n$ together imply $B$ then one's degrees of belief should be such that $D(B) \geq D(A_1) + \ldots + D(A_n) - (n - 1)$.

But it is possible to imagine someone proposing a different connection between implication and degrees of belief, with the result that *despite different claims about "implication"*, he accepts *precisely the same constraints on degrees of belief*. When that happens, I suggest, the proponents of the different logics don't genuinely disagree.

As a possible example, suppose someone accepts not only (I), but also

(II) If one knows [is certain] that $A_1$ through $A_n$ together imply $B$ then one's degrees of belief should be such that $D(\neg B) \leq D(\neg A_1) + \ldots + D(\neg A_n)$.

(II) is equivalent to (I) in a classical theory of degree of belief, where $D(\neg A)$ is just $1 - D(A)$. But in standard treatments of degree of belief for nonclassical logic, this equation is rejected and (II) and (I) are inequivalent. In particular, in logics that restrict excluded middle but disallow the acceptance of contradictions, one typically requires only that $D(\neg A) \leq 1 - D(A)$. In that case, (II) adds nothing to (I) *in the case of theorems*: (I) requires that $D(B) = 1$, and (II) that $D(\neg B) = 0$, but the latter follows from $D(B) = 1$ plus $D(\neg B) \leq 1 - D(B)$.[17] But *for inferences with at least one premise*, imposing (II) as an additional requirement beyond (I) makes a genuine difference.

[15] I suppose one could extend the view that implication is normative to the view that the ground-level connectives are normative. But that would be an extraordinarily radical view—for instance, it would make even the laws of physics normative.

[16] Cases where one logic contains a theorem that a proponent of another logic doesn't *accept as a theorem*, but also doesn't *disagree with*, tend not to be terribly interesting from the point of view of pluralism: they are simply cases where the proponent of the second logic imposes demands beyond truth that the proponent of the first logic doesn't impose.

[17] I'm assuming that the allowable range for values of $D$ is the interval from 0 to 1.

For instance, consider the instance of modus ponens that takes us from $K$ and $K \rightarrow \bot$ to $\bot$, where $K$ is the Curry sentence considered above. Presumably $D(\bot)$ is 0 and $D(\neg\bot)$ is 1; and for a view that accepts the equivalence of True($\langle K \rangle$) to $K$, $K$ will be equivalent to $K \rightarrow \bot$ and so $D(K \rightarrow \bot)$ should be $D(K)$. A typical proponent of a logic without excluded middle would want $D(K)$ and $D(K \rightarrow \bot)$ to be 0, and would also want $D(\neg K)$ to be 0. That is compatible with modus ponens (and the closely related 1-premise inference from $K \wedge (K \rightarrow \bot)$ to $\bot$) being valid, if one accepts only Constraint (I); but if one adds Constraint (II), modus ponens will be declared invalid. It seems to me that someone who declares modus ponens invalid *because he accepts Constraint (II) as well as Constraint (I)* is in merely verbal disagreement with someone who accepts only Constraint (I) and regards modus ponens as valid.[18]

I do not claim that in *every* case where two logics differ in their validities but not their theorems, proponents of the logics merely differ verbally. Indeed, one can imagine the same modus-ponens-restricting logic advocated by two people, one of whom imposes constraint (II) and the other who doesn't: the first proponent might differ merely verbally from someone who accepts modus ponens, while the second would be suggesting a genuinely different logic.

The best case for a significant pluralism in logic lies with examples that (a) don't seem like verbal variants, and where in addition (b) the logics disagree only in inferences that have at least one premise, not in their theorems. When they disagree in their theorems (or at least, when one has theorems that the proponent of the other can be expected to disagree with), the dispute between them seems a clearly factual one. But even in cases for pluralism that meet conditions (a) and (b), it isn't always obvious that there's no genuine conflict or disagreement between proponents of the logics.

One point that I will merely mention but not pursue is that advocates of different logics might agree on truth-related goals more subtle than having true theorems, and each might hold that the practice of constraining their degrees of belief by means of their favored logic is more conducive to meeting those truth-related goals than the practice of constraining degrees of belief by the alternative logic would be. If so, there would seem to be a factual issue between them.

A second point is that even when *factual* disagreement is absent, *normative* conflict or disagreement can remain. In particular, even in those relatively unusual situations where there is no factual disagreement between advocates of different logics (and where the difference isn't verbal), they disagree about how to constrain their degrees of belief. Perhaps in some of these cases, the advocate of one logic might recognize the other as a permissible alternative. This certainly happens with induction: the advocate of one inductive method is likely to think that someone with a method that differs only in that some adjustable parameter takes on a slightly different value isn't substantially in error. When the differences are more substantial, however, there is a clear sense that the alternative recommendations are in serious disagreement, even though the disagreement is about policy rather than fact. It would seem to be stretching the notion of pluralism to call an epistemological expressivist

[18] Another way that the difference might be purely verbal is if the people verbally accept the same connection between logic and degrees of belief, but have different understandings of 'degree of belief', in a way that allows the apparent conflict in their recommendations about degrees of belief to be merely verbal. This strikes me as a serious possibility in some cases (for instance, when one person's D($A$) is another's $1 - \mathrm{D}(\neg A)$), but I will not consider it further.

"pluralist" with regard to counterinductive methods, or a moral expressivist "pluralist" about whether genocide is a good thing.

In the case of deductive logic, there may be cases where the alternative logics disagree only as regards rules of inference and not about theorems; don't differ in factual respects with regard even to shared goals that go beyond truth; are not verbal variants; and where the advocate of one logic doesn't regard the other logic as totally beyond the pale. Cases meeting these conditions would be the cases most naturally thought of as substantiating a significant "pluralism". But I think that cases like this will be rare.

In short: I've left room for a certain degree of pluralism about logic, but it is a pluralism of very limited scope.

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