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Logical Partisanhood

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Abstract A natural suggestion and increasingly popular account of how to revise our logical beliefs treats revision of logic analogously to the revision of scientific theories (Hjortland, Priest, Russell, Williamson, etc.). I investigate this approach and argue that simple applications of abductive methodology to logic result in revision-cycles, developing a detailed case study of an actual dispute with this property. This is problematic if we take abductive methodology to provide justification for revising our logical framework. I then generalize the case study, pointing to similarities with more recent and popular heterodox logics such as naïve logics of truth. I use this discussion to motivate a constraint—LOGICAL PARTISANHOOD—on the uses of such methodology: roughly: both the proposed alternative and our actual background logic must be able to agree that moving to the alternative logic is no worse than staying put.

Keywords Logical revision · Abduction · Non-classical logics · Recapture theorems · Anti-exceptionalism about logic

1 Introduction

An increasingly popular account of logic, *Anti-Exceptionalism*, views logic as similar to, and continuous with, other scientific theories. It thus treats revision of logic analogously to revision of scientific theories, applying familiar abductive

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standards of scientific theory choice to the case of logic.[1] We should, that is, move from one logical theory $\mathfrak{L}$ to another $\mathfrak{L}'$ when $\mathfrak{L}'$ does "better" than $\mathfrak{L}$ in terms of theoretical virtues like:

> ...simplicity, ontological leanness (Occam's razor), explanatory power, a low degree of ad hocness, unity, [and] fruitfulness. (Priest 2006: 135)

This is intended to explain *rational* change of logic; nothing so detailed is needed to explain vacillating flirtations we might have with one logic or another. *Abductive methodology* is supposed to provide justification for moving from one logic to another. One whole body of logic, that is: the particular and common version of this methodology I'm here interested in isn't aimed at settling whether we should revise any particular logical principle.[2]

Unfortunately, this approach to logical revision is problematic for insufficiently appreciated reasons. I'll highlight this by posing a challenge for this method and, in response, proposing a principled constraint on abductive approaches to logical revision. Adopting this constraint rules out many putative logical options as reasonable alternatives to classical logic, at least given their current state of development, but there remain interesting challengers to classical orthodoxy.

I focus on disputes about which logic to adopt as our most basic canon of logical implication.[3] There is little to no dispute about whether we can adopt distinct logics for instrumental purposes.[4,5] Nearly all cases of situation-specific reasoning, such as drawing out local commitments from potentially inconsistent data sets, can be treated as instrumental applications of formal methods; the most interesting questions about logical revision focus on our most general canons of implication, such as which logic should we take as the *background logic* in which to evaluate logical relations between propositions. My discussion should be understood accordingly.

[1] For the origin of this approach, see Goodman (1983) and tentative suggestions throughout Quine's corpus. Recent advocates include Bueno and Colyvan (2004), Priest (2006, 2016), Russell (2014, 2015), Williamson (2014, 2017), and Hjortland (2017).

[2] For criticism of the case-by-case reflective equilibrium methodology, proposed by Goodman (1983), see Shapiro (2000), Wright (1986) and Woods (forthcoming-a). It simplifies matters to think of full logics instead of significant fragments thereof, though my point generalizes.

[3] This question should be separated out from the question about what our actual most basic canons are like; this interesting questions, privileged by some relevance logicians, seems more a question for linguists, psychologists, and philosophers of mind than epistemologists and logicians. See Burgess (1981) and Hanson (1989) for discussion.

[4] Shapiro (2014) worries about whether there is a background logic at all; however, inspection of his discussion seems to reveal that his primary concern is with logics in their instrumental role. It's hard to deny that there are background canons of implication. For instance, his presumption that reasonable logics agree on things like *inhaltlich* mathematics already presumes quite a bit of shared background logic. We need not presume the entirety of our logical commitments in order to make the point that if part of what is up for grabs is the acceptability of using *inhaltlich* mathematics in running a comparison, then the comparison is vulnerable to cycles absent sufficient common ground. My restriction to whole bodies of logics, like classical and intuitionistic, simplifies, but isn't strictly necessary for my overall point. Thanks to Liam Kofi Bright for discussion.

[5] I use 'implication' instead of 'inference' in order to avoid engaging with the dispute over whether inference, even deductive inference, corresponds neatly with logical implication (Harman 1986).

I put aside concerns—due to Kripke, discussed in Berger (2011)—about whether the idea of adopting a logic makes sense at all; if not, then the case against abductive accounts of logical theory choice is significantly simpler. Finally, I concentrate on actual *uses* of abductive methodology in justifying choosing one logic over another; this means that we need not only that certain reasons for revision obtain, but that we support revising on their basis and that we're permitted to assert them and evidence them to those we are trying to convince. That is, I'm interested in the reasons we possess and are in a position to use in justifying revision of our most general canons of implication. There may be reasons for preferring one logic over another which are not available for use in justifying revising our logic; whether and how this might be so is a matter is far trickier than that discussed here and must await another occasion.

I'll present and analyze an actual dispute about which background logic is correct where simple applications of abductive methodology fail to provide a reasonable outcome. The case is problematic because each party to the dispute should, under reasonable assumptions about abductive methodology, claim that the other logic "scores better" than it itself does on various theoretical virtues. Some theorists have recognized the possibility of such counterexamples but they've claimed that it's difficult to find "realistic and interesting pairs" of such theories (Krämer 2014; Priest 2016).[6] I show, by detailed examination of a contemporary example, that there are examples of realistic and interesting pairs which are related in this way.

Slightly more contentiously, I argue that there's a class of recent theories similarly situated with respect to classical logic. The trouble with these examples is that applying abductive methodology in order to evaluate them with respect to an alternative results in a rational agent oscillating between the alternative and the starting logic. Since choice of logic is supposed to be a *rational* enterprise, we need to be able to avoid such oscillation, and that we can do so by endorsing a principle constraining our use of abductive methodology like:

> Logical Partisanhood: Unless the output of weighing the merits of my background logic against an alternative—on one hand by the lights of my own background logic and on the other hand by the lights of the proposed alternative—agree that moving to the alternative is no worse than staying with our current background logic, we ought to hold fast to our background logic.[7]

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[6] Others, such as Hjortland (2017) are less concerned about the non-neutrality of criteria of theoretical goodness, suggesting that we can revise logic piecemeal. Hjortland's discussion of abductive methodology is mainly at the level of entire logics; since he doesn't discuss criticism of the piecemeal approach by Wright and Shapiro, I'll put his suggestion to the side here (though see Woods (forthcoming-a)). Another consideration involves what happens when we compare more than two logics at a time and whether my proposed constraint on logical revision still holds for such cases. It seems to me that a generalization will hold, though this raises additional difficulties and disturbs the contrast with piecemeal revision, so I put it aside. I hope to address it elsewhere.

[7] Replacing 'no worse' with 'better' in logical partisanhood, we obtain a stronger principle I also favor. However, it could be that classical logic favors switching to some alternative $\mathfrak{L}$ and $\mathfrak{L}$ is neutral between classical logic and $\mathfrak{L}$. It's arguable that we should then switch from classical logic to $\mathfrak{L}$ and perhaps even to switch from $\mathfrak{L}$ to classical logic or not, as one desires. Further adjudication depends on details of our theory of rational choice which outstrip my discussion, so I focus on logical partisanhood., the weaker principle. Thanks to Jared Warren for discussion.

LOGICAL PARTISANHOOD, so-called because it gives us permission to remain partisan even when there's reason from our perspective to switch, blocks these oscillations. Since our opponent cannot construct a case for us switching by their own lights—just as we would be unable to recreate our reasons to switch if we did—we ought to hold fast. I now turn to developing the problem case that leads to adopting this constraint.

2 Theoretical virtues for logics

We presume, for simplicity, that there is a (perhaps trivial) logic-neutral translation of the language of each of the pair of logics in dispute into a common language from which we can arbitrate the dispute.[8] We treat a logic as a set of operational and structural rules. The former consist of rules governing particular constants like conjunction and the conditional. The latter include things like weakening, contraction, and cut. We write $\Gamma \vdash_{\mathfrak{L}} \varphi$ for the claim that we can derive φ from Γ by repeated application of the rules of $\mathfrak{L}$. We define the closure $cl(\mathfrak{L})$ of a logic $\mathfrak{L}$ as the set of pairs $\langle \Gamma, \varphi \rangle$ such that $\Gamma \vdash_{\mathfrak{L}} \varphi$. We also presume one logical alternative $\mathfrak{L}'$ is a sublogic of the other $\mathfrak{L}$ in the sense that $cl(\mathfrak{L}') \subseteq cl(\mathfrak{L})$ (this simplifies the discussion considerably). With these simplifications, we might take the following two criteria to guide our choices in deciding whether to accept a logic:[9]

- *Strength* One logic $\mathfrak{L}$ is *stronger* than another $\mathfrak{L}'$ when it can prove more. That is, when the closure of $\mathfrak{L}'$, over the target domain, is a proper subset of the closure of $\mathfrak{L}$—$cl(\mathfrak{L}') \subsetneq cl(\mathfrak{L})$.
  - This might seem trivial, given our presumption about one logic being a sublogic of another, but it isn't. In practice we only care about strength with respect to a target domain like mathematics and a sublogic can agree with its superlogic on this target domain. We don't usually care if one logic can prove pointless disjunctions like $\varphi \vee \top$ and another cannot. We thus assume a target domain that we clearly care about: thoroughly uncontentious mathematics. For instance, we want to be able to derive large fragments of arithmetic from the *PA* axioms. We'll then say that $\mathfrak{L}$ is *stronger* than $\mathfrak{L}'$ when the former can derive more uncontentious mathematics from the relevant axioms. Take this caveat as read below.[10]

[8] We could frame this point in terms of a choice of translation principles from one language into another, but it suffices to presume a common language; the key thing to avoid is cases where we invert the expressions of a language, using 'or' for 'and' and conversely, but where the inversions of the usual rules hold of the inverted expressions. Presumably two such languages do not disagree. See Woods (forthcoming-b) for a syntactic—and thus metatheory-light—account of theoretical equivalence between logics. Thanks to Jared Warren for discussion.

[9] Thanks to helpful referees for useful discussions of these criteria, closure operations, and the nature of a logic.

[10] See Shapiro (2014) for useful discussion and insistence on the non-negotiability of recovering uncontentious mathematics.

- *Informativeness* One logic $\mathfrak{L}$ is more *informative* than another $\mathfrak{L}'$ when the informational upshot of $\mathfrak{L}$-provability is greater than the informational upshot of $\mathfrak{L}'$-provability.
  - Informativeness captures one sense in which provability can be explanatory: by revealing detailed information about *why* the conclusion follows from the premises. Intuitively, informativeness of a logic is a matter of informativeness of the proofs involved, but there are cases where any proof of a claim in some logic—such as a constructive logic—guarantees some amount of information that a proof in a supralogic like classical logic does not. So a sublogic of $\mathfrak{L}$ can be more informative than $\mathfrak{L}$, even though provability in the former counts as provability in the latter because the fact that some claim is derivable in the weaker logic *guarantees* information that the fact that some claim is derivable in the stronger logic doesn't. In such a case, we'll say that, at least in this respect, the weaker logic is more informative. Informativeness is still somewhat vague here, but our use of it below is sufficiently clear as to be unproblematic.[11]

Strength and informativeness, unlike more metaphysical criteria like Occam's razor, admit of clear application to evaluating the goodness of a logic. Given some set of criteria like these, we can arbitrate some disputes between logics. When $\mathfrak{L}'$ dominates $\mathfrak{L}$ by the lights of them, we should adopt $\mathfrak{L}'$. When it doesn't, then we should stay put. We could develop the evaluation and weight of these criteria in more detail, but it wouldn't matter for the general point made here. Likewise, we will assume that strength and informativeness are the salient criteria for choosing between logics since other criteria could be chosen without damage to our cases at the cost of some complexity.[12]

3 Assessment and a *précis* of the problem

There's an immediate problem with application of abductive methodology to logic; it's not entirely clear which logic we should use to assess how well $\mathfrak{L}$ or $\mathfrak{L}'$ meet the above criteria. Should we use what we conceive of as our actual background logic, say $\mathfrak{L}$? If we do, then if we revise to $\mathfrak{L}'$, we may change the evaluation of our logical

[11] It's often clear in practice that certain types of proofs are more informative than others. For example, intuitionistic proofs of existentially quantified claims require explicit constructions of witnesses when we don't know in advance that the claim is determinate (and when we do know in advance that a claim is determinate, there's a recipe for providing an explicit witness). Intuitionistic proofs of $\exists x\varphi$ not only show that *there is* an object that has property $\varphi(x)$, it provides an explicit example. Intuitionistic provability thus guarantees access to information that classical provability doesn't guarantee.

[12] Other natural criteria here involve various forms of proof-complexity—length of proofs, complexity of concepts involved, etc—as well as expressive strength (the ability to express various concepts). The former criteria seem less central than strength and informativeness, the latter will more or less coincide with my version of strength in the particular target domain under consideration. Since my goal here is to construct problem cases, not give a detailed account of the criteria for logical revision *tout court*, I'll put these alternatives to the side.

options by the lights of these criteria. Presumably, we should then rerun the comparison. Here's Priest, making a similar point:

> However, it remains the case that logic (arithmetic) is deployed in the choice process, and we may end up choosing a logic (arithmetic) different from the one we currently employ. If we do so, then the choice-computation will have to be redone after the new theory is adopted (Priest 2016: 17)[13]

However, since our evaluation may have changed, we may need to backtrack on our choice. In cases like those I'll shortly describe, adopting the simple choice principle implicit above:

> SIMPLE ABDUCTION: Choose whichever theory does better, *vis-á-vis* informativeness and strength, by the lights of our current logic

results in a 2-cycle, choosing $\mathfrak{L}'$ over $\mathfrak{L}$, then needing to revert to $\mathfrak{L}$ as it scores higher than $\mathfrak{L}'$ by $\mathfrak{L}'$'s lights. We could presumably adopt a more complicated choice principle, but it's entirely unclear how this would help.

Can we resolve this problem by moving to an alternative view of which logic to use while applying abductive methodology? The obvious suggestions all seem rather problematic. We could simply pick one or the other logic to use when applying abductive methods, but then it looks like we aren't truly advocating revising our logic at all since using any non-arbitrary choice of a logic to *decide* between logical options suggests a principled justification for already using it as our background logic.[14] If, say, $\mathfrak{L}'$ is preferable for carrying out abductive justifications, why wouldn't $\mathfrak{L}'$ be preferable as a background logic in other contexts?

If we demand that all criterial evidence for changing our logic be assessed from the standpoint of the *weaker* logic, we demand that we ignore evidence when we start with a stronger logic as our background logic. This is epistemically problematic and, moreover, its natural motivation seems to better motivate LOGICAL PARTISANHOOD. Presumably the motivation is that reasons to switch—though, in deference to the first point, not reasons *not to switch*—be acceptable from both sides. We've assumed we're comparing a logic with a sublogic, so this is guaranteed by working in the weaker logic. It is, however, too strong a constraint.

LOGICAL PARTISANHOOD requires that both parties to a dispute assess the dispute in their own terms, but that we should only switch when they agree that one of the two logics is better. The crucial case, for reasons to switch, is when both alternatives agree we should switch, but where they have distinct cases for this. For example, $\mathfrak{L}$'s verdict might be to switch and $\mathfrak{L}'$'s verdict a stronger version of the same since not all of $\mathfrak{L}'$'s reasons are accessible from the standpoint of $\mathfrak{L}$. In such a case, plausibly, we *ought* to switch. Given this, requiring that we work entirely in the

[13] This raises another issue I want to put aside. How long we can try to recapture classical results before we have to redo the comparison? Surely we get some leeway, surely we can't take forever. I can avoid this here since the cases below involve logics where we're unlikely to be able to recreate the classical results due to medical limitations. See below for details.

[14] And surely randomly picking $\mathfrak{L}$ or $\mathfrak{L}'$ to arbitrate isn't rational.

weaker logic seems too strong—LOGICAL PARTISANHOOD does better. We will thus put aside the simplistic idea that we should assess the merits of switching to an alternative weaker logic in the weaker logic.

We could use the intersection of the alternatives, but if the logics are not sublogics of one another, then we will typically have insufficient resources in the intersection to build any kind of justification of the abductive superiority of one logic to another on this basis (Priest 2006: Sect. 12.9). This leaves the case where the intersection of the two logics is plausibly sufficient to generally carry out abductive justifications. However, the problems for this approach are exactly analogous to those arising for working with the weaker of the alternatives, detailed above.

Some theorists have favored the idea that we should assess the merits of a theory by *its own* lights.[15] Hartry Field makes the point well, in recent work, where he notes:

> if you take 'logically valid' to obey a logic weaker than classical, you shouldn't ultimately be satisfied with developing your theory of that logic using inferences that are merely *classically* valid... (Field 2017)

Field is suggesting that, when assessing how well a logic meets a criterion, we should use that logic itself. So, in particular, we would assess the strength of $\mathfrak{L}$ by looking at what we can $\mathfrak{L}$-prove and the strength of $\mathfrak{L}'$ by looking at what we can $\mathfrak{L}'$-prove. Unfortunately, this approach seem to merely move the bump in the rug, as whether we can $\mathfrak{L}$-derive φ from Γ is itself a relation which can be assessed differently by different logical theories, as our case study will shortly demonstrate.

This means that there are two roles logic plays in assessing how well a target logic meets criteria of theoretical goodness. There is what we might call the *derivability* role, captured by the occurrence of '$\mathfrak{L}$' in claiming φ—which itself might be a claim about provability or entailment—is $\mathfrak{L}$-provable or $\mathfrak{L}$-proved. There is also what we might call the *working* role, captured by the logical resources we employ in *justifying* claims like these. Putting this more clearly, let's write $Q \vdash_{\mathfrak{P}}$ "$\vdash_{\mathfrak{L}} \varphi$" for the claim that the logic $\mathfrak{P}$ and Q, a collection of axioms governing syntax and inductive definitions, proves that φ is $\mathfrak{L}$-provable.[16] $\mathfrak{P}$ is serving here in the derivability role. Now consider the warrant with which we assert a claim like $Q \vdash_{\mathfrak{P}}$ "$\vdash_{\mathfrak{L}} \varphi$". If our warrant is that we have a C proof that Q $\mathfrak{P}$-proves that φ is $\mathfrak{L}$-valid, or more generally if C is the logic governing our application of evidence to warranting the claim, then C is occupying the working role. These two roles can come apart.

For instance, Bacon (2013) recently showed that we can generate basic metatheoretic results for even classical logic in a non-classical metatheory.

[15] See Krämer (2014) where this approach is argued for in detail, though in a slightly different context.

[16] Presumably Q should be a suitably weak theory; it would be ad hoc to stuff so much into our syntax axioms that the weakness of a logic is overcome. See discussion of Field*'s picture below.

However, his proof proceeds classically, showing that in a *formalized* non-classical metatheory M, using only M-acceptable methods, we can develop a semantics for a logic $\mathfrak{L}$. That is, that classical logic (and arithmetical axioms playing the Q role) proves "$M \vdash_{M} \varphi_{\mathfrak{L}}$" for various metatheoretic results $\varphi_{\mathfrak{L}}$ about $\mathfrak{L}$. Plausibly, we also have, depending on whether we can classically show that M suffices for developing a reasonable theory of syntax from Q, a *classical warrant* for asserting that $Q \vdash_{M}$ "$M \vdash_{M} \varphi_{\mathfrak{L}}$".[17] These are lovely results, but our *warrant* for accepting them is still classical since classical logic is playing the working role,[18] even though M can play the derivability role (if we can develop an adequate theory of syntax in M).[19] There is still a residue of classicality in these results.

Field's quote can thus be interpreted in terms of the non-classical logic playing the derivability role or the working role. In the former interpretation, we use our actual working logic to assess what's $\mathfrak{L}$-provable, using $\mathfrak{L}$ in the derivability role—i.e. we use $\mathfrak{L}$ to assess what's in $cl(\mathfrak{L})$. In the latter, we also use $\mathfrak{L}$ for our working logic as well. But the latter idea runs into the same difficulties discussed above about working in the weaker of two logics—insofar as it differs from LOGICAL PARTISANHOOD, it does so in a way which seems epistemically problematic. We will thus put the latter interpretation aside and consider as a reasonable alternative to using our working logic for the derivability role, using $\mathfrak{L}$ in generating $cl(\mathfrak{L})$.

These two approaches—using our actual working logic and using a logic's own logic in the derivability role—to assessing theoretical virtues are the natural approaches; dissenters must therefore justify proceeding in an alternative fashion.[20] Henceforth, then, we will presume that one of these two ways of assessing the overall theoretical goodness of a logical theory is correct. As will be seen shortly, our example yields significant problems for both approaches.

[17] Note that this is non-trivial given the use of arithmetic as a syntax language; we need to reproduce all the standard coding and representability results using only M-acceptable methods. One way to do it is to give a classical proof of a "recapture result" showing that more usual methods are non-classically acceptable for proving *these* standard results. On this move and constraints on it, see below.

[18] Bacon is well aware of this—see fn. 12 where he points out that he is presuming that syntax admits of classical reasoning. The question remains whether he is entitled to that presumption without showing rigorously and non-classically that syntax is a legitimate context in which to reason classically. See the discussion of Field* below.

[19] A similar complaint holds for Mares's admirably explicit development of a non-classical theory of probability and application of such to choice of logic (Mares 2014). The background metatheory used in this construction (the working logic) is thoroughly classical, so the reasons generated on the basis of it will be cold comfort to those seeking a thoroughly non-classically acceptable justification of their favored non-classical logic.

[20] A burden that has been shouldered before; Bob Meyer (1985) argues that there is no real need to develop relevance metasemantics for relevance logic, *even though* he held our actual logic is non-classical. Of course, this view is contentious and, more importantly, his argument depends on viewing relevance logic as an empirical claim about the correct interpretation of our actual inferential practice. Given this, we put this case aside.

4 Case study: Tennant *v* Burgess *in re* cut[21]

Having introduced the target anti-exceptionalist approach to logical revision, I now turn to developing our problem case where two logics each reckon the other superior to themselves. My aim isn't just to demonstrate the theoretical possibility of such a case, but to show that actual disputes about which logic to adopt have this problematic feature. Graham Priest (2016) and Stephen Krämer (2014) have noticed the abstract possibility of such a case, but they shrug it off, claiming that the actual existence of natural cases of this is unlikely:

> Note that [Krämer's favored principle of commitment] does of course render some pairs of theories incomparable. Trivial examples are provided by theories that explicitly make competing claims about what sentences their rivals' best extensions include or fail to include. It seems plausible, however, that these cases at least are cases of genuine incomparability, and that few if any realistic and interesting pairs of theories are thus related. (Krämer 2014: 2163)

> A worst-case scenario is one where we simply flip back and forth between two logics (arithmetics), each of which is better according to the other! It's hard to come up with realistic examples of this sort of situation, and, therefore, to pursue a realistic discussion of how to proceed under such circumstances. (I can't think of any historical examples of this kind of situation). But, presumably, the fact that we are in such a loop would itself be new information to be fed into the decision process. It exposes some kind of incoherence in the theories at hand, and we might be best off looking for a new theory which is not subject to this kind of incoherence (Priest 2016: 17).[22]

However, we can find such cases. My main example is a relatively recent dispute between John Burgess and Neil Tennant over Tennant's version of relevance logic.[23] We will use $\vdash_{T}$ to indicate the derivability relation of Tennant's classical core logic and $\vdash_{C}$ for classical derivability.[24] *Prima facie*, adopting classical core logic would be a disaster for mathematics since it's not monotonic—$\Gamma, \varphi \vdash_{T} \psi$ does not follow from $\Gamma \vdash_{T} \psi$—and it's not transitive—$\Gamma \vdash_{T} \varphi$ does not follow from $\Gamma \vdash_{T} \psi$ and $\psi \vdash_{T} \varphi$.

But monotonicity and transitivity are pervasive in ordinary mathematical proof. If Tennant's view is to be reasonably close to adequate with respect to ordinary mathematics—if it's to do well on the strength criterion—we need some reason to

[21] I trust the homage here is apparent.

[22] As will become clear, I agree that this information should inform our overall choice.

[23] See Tennant (1987, 2002) for Tennant's arguments in favor of his view, Burgess (2005) for the feasability-based objection detailed here, and Tennant (2006) for Tennant's initial rejoinder. Thanks to a helpful referee for pointing out infelicities in my earlier discussion and useful discussion of Tennant's results.

[24] For ease of exposition, we will work with Tennant's classical core logic instead of his more extreme core logic.

think that non-pathological ordinary mathematical proofs have Tennant-acceptable proofs. And, somewhat surprisingly, we do have this sort of reason:

> Cut-Elimination for T (cet): There's an effective operation [..., ...] from ordered pairs of T-proofs to T-proofs such that, given a T-proof Π of φ from Δ and T-proof Σ of ψ from Γ and φ, $[\Pi, \Sigma]$ is either a T-proof of $\bot$ or of φ from premises in $\Delta \cup \Gamma$ (Tennant 2014: Th. 1)

We then have a corollary:

> Classical Recapture for T (crt): If $\Delta \models_{C} \varphi$, then for some $\Gamma \subseteq \Delta$, $\Gamma \models_{T} \varphi$ or $\Gamma \models_{T} \bot$. (Tennant 2014: Th. 4)

crt says, roughly, that if $\Gamma \models_{C} \varphi$ and Γ doesn't contain pathology, we've a T-proof of the same result from strictly relevant premises. What is more, the result is constructive; we can effectively transform non-pathological classical proofs into T-acceptable proofs, roughly by repeated applications of [..., ...]. T-proofs are guaranteed to be *compact* in the sense of containing only those premises strictly necessary for their proof and make no epistemically dubious transitions through pathology. This seems like a significant epistemic gain over classical logic and thus T seems more informative.

On the other hand, there's a problem with our grasp of this epistemic gain. Tennant's proofs of cet and crt don't obviously avoid use of the monotonicity and transitivity properties which they're supposed to show can be eliminated. For instance, Tennant (2015) doesn't give an explicit account of his background syntax theory or which theory of inductive definitions he's making use of. He defines a complexity measure without showing that his defined version has all the typical properties. Proof is typically by inspection, but it isn't shown that what's obvious by inspection conforms to T-acceptable scruples. And so on.

So whether Tennant's (2012, 2015) proofs—of cet and thereby crt—are T-acceptable is contentious. What he's done is provided a constructive recipe for generating the values of [..., ...]. Given a weak theory of inductive definitions (required for defining [..., ...]) and syntax (required for proving facts about the shape and availability of various proofs and sentences), we can effectively merge together two T-proofs, one ending in φ, the other using φ as an auxiliary premise, to obtain a T-proof "cutting out" φ. This is an excellent result. However, in my view, it doesn't yet T-prove cet or T-prove that there is a T-proof of cet.

To give a particular example of the sorts of problems I have in mind, let's grant that T suffices to prove the relevant results about syntax and his inductive definition mentioned above.[25] There's good reason for this: from a classical point of view,

[25] A helpful referee suggests that Tennant can take a proof-theoretic account of inductive definitions, analogous to the account given by Martin-Löf (1971), for his Q and prove these results directly. I have several concerns about this. First, it doesn't solve the problem with a theory of syntax; there are still implicit metacuts which arise in making use of banalities of syntax theory. Second, this style of account of inductive definitions, as Tennant notes in (2006: 1158), has been used for a foundation of *intuitionistic mathematics*. The version of Tennant's core logic we're inspecting here, though, is relevant, not intuitionistic. It's an open question how natural this account of inductive definitions is once we move to

Tennant has given excellent reason, in terms of a cogent proof of CRT, that these results can be obtained. Nevertheless, *Tennant* hasn't obtained them, *using T*, in his (2015). What we have instead is presumably the presumption of a proof elsewhere of the relevant facts, then the tacit use of them as undischarged assumptions (masked by the "don't mention it if it's obvious" norm of ordinary classical and intuitionistic informal proof). This amounts to an *implicit* metacut, since the relevant transitivity and monotonicity properties can't simply be assumed.

Of course, I grant we could avoid this problem at the cost of proof length. The problem is the nagging worry that a fully explicit proof would be infeasibly long—the number of uncited banalities concealed by norms of informal proof isn't at all trivial and these norms aren't clearly cogent once we've moved away intuitionistic or classical mathematics.[26] I'd be happy to be proven wrong about the matter, but if I'm right about the lacunae, we won't have a *T*-acceptable justification of CRT unless or until this is done. So it seems reasonable to wonder whether a *T*-acceptable proof of Tennant's result really has been given and not unreasonable to hold that it hasn't.

It's worth stressing, though, how close Tennant's proof is to what I'd regard as a *T*-acceptable proof—Tennant is one of a handful of non-classical theorists who dot their 'i's and cross their 't's when proving non-classical metatheoretic results. Moreover, Tennant might object that the required weak theories of inductive definitions and syntax can be straightforwardly and feasibly developed in a *T*-acceptable fashion. I doubt it will be both straightforward and feasible, but my discussion survives being wrong. I'm developing historically interesting problem cases for anti-exceptionalism about logic and arguing on their basis that proponents of logical revision should obey LOGICAL PARTISANHOOD. If Tennant's right, then we just need to look back a few years, prior to his *T*-cogent proof. Assuming his current proof is *T*-cogent or near enough, Tennant's actually resolved his own problem case by proving CET and CRT *T*-acceptably, thereby satisfying LOGICAL PARTISANHOOD (and, incidentally, generating a troubling anti-exceptionalist argument for adopting *T* to classically-minded theorists like myself).

So, either as a historically interesting example, or, as I reckon, a currently pressing one, we have two competing logics, *T* and *C* and two theoretical virtues: strength, cashed out as the ability to recapture uncontentious mathematical reasoning, and informativeness, cashed out in terms of how much information is guaranteed by some claim's provability. By *C*'s lights, *T* and *C* are roughly equivalent with respect to strength (given CRT) and *T* scores higher than *C* on informativeness. However, by *T*'s lights, *C* dominates *T* with respect to strength

Footnote 25 continued
the relevantized system. At best, there's a gap to be filled. See also the discussion of Field* below for worries about taking too much for our *Q*.

[26] The key worry has to do with the fact that the *canonical* transformation of ordinary to cut-free proofs involves exponential speed-up. See Burgess (2005) for reasons for thinking that Tennant will not be able to produce such a proof. Tennant (2006) correctly notes that exponential speed-up is the worst case scenario and that it's entirely possible that there will be a feasible proof. This is right, but it should be noted that one which is clearly T-compliant has not yet emerged.

(since CRT isn't available to *T*) and, say, *T* still scores higher than *C* on informativeness.

Applying the simple decision rule suggested above, we obtain one version of the two-cycle by presuming our *current* logic plays the derivability role. We write $cl_{\mathfrak{B}}(\mathfrak{L})$ for the closure of $\mathfrak{L}$ with $\mathfrak{B}$ playing the derivability role. Then $cl_C(T) \subsetneq cl_C(C)$, but the difference in strength is close—the cases where *C* exceeds *T* are relatively unimportant when it comes to workaday mathematics, given (the classically legitimate) CRT. This means that, given our hedge about strength, *C* and *T* are roughly equivalent. *T* dominates *C* with respect to informativeness. So, starting with *C*, we ought to adopt *T*. Once we've adopted *T*, the score changes. $cl_T(T) \subsetneq cl_T(C)$, but the gap in strength is wide since CRT isn't available from the perspective of *T*. We thus have no reason (yet!) to think that *T* really does entail sufficiently much workaday mathematics (and thus, by the lights of *T* that we can see and make use of, the gap is wide). *T* is still plausibly more informative than *C*, but we have a wide enough gap in strength that summing again suggests switching back to *C*. And away we go.

Alternatively, using $\mathfrak{L}$ in the derivability role for $\mathfrak{L}$, we face a second version of the two-cycle, though now one focused on what we are entitled to *assert*. Our working logics, from both perspectives, may not suffice to legitimate claiming that *T* and *C* *T*- and *C*-prove various claims. After all, *C* justifies the claim that a weak theory of syntax and inductive definitions suffices to show that CRT is *T-valid*—since there is a classical proof of this fact and there is thus no barrier to using CRT in our justification that CRT is *T*-derivable. *T*, however, does not justify the *T*-provability of CRT since there is as yet no *T*-proof that CRT is *T*-provable from weak theory of syntax and inductive definitions.

That is, we are plausibly not *T*-justified, though we are *C*-justified in asserting $\vDash_T$"$Q \vDash_T$ CRT", as we have no proof that *T*-proves CRT from *Q*. This means that what we are rationally entitled to assert about the criteria of theoretical goodness shifts with our background logic. Starting with classical logic, we are entitled to assert that $cl_T(T) \subsetneq cl_C(C)$, but that the difference is close, similar to above. Moving to Tennant's logic, we are not so entitled; the most we can assert is that $cl_T(T) \subsetneq cl_C(C)$, and that the difference, for all we know, is fairly wide. Note that we are using $\mathfrak{L}$ in both cases to inform the closure operator for our assessment of $\mathfrak{L}$; we are just now constraining what we may assert by use of our background logic.

As long as we think that our justification for asserting that a criterion is met goes by the lights of our current background logic, we can't assert the superiority of Tennant's logic to classical logic once we adopt Tennant's logic as our own. Whereas, from the point of view of classical logic, it's clear that Tennant's logic really does have the desirable property CRT. So, when assessing how well a logic meets the criteria by its own lights, we also face the problem that adopting a logic may change our assessment of how well we can say it meets various criteria of theoretical goodness.

This presumes that rationality requires that we assert no more than what our background logic licenses, but this seems entirely reasonable. Why should our standards of justification allow us to explicitly assert the merits of a logical option when we cannot prove that it has these merits with the logic that we actually endorse? This logic *may* have these merits, but it seems unacceptably permissive to allow us to use this fact as we cannot see it from our current perspective. There very well might be cases where other sources of information, besides proof, licensed this assertion, but the justificatory burden is clearly on the theorist claiming the existence of such justification.

The case of *Tennant v Burgess in re Cut* establishes that simple applications of abductive methodology in logic actually face the problem that Priest and Krämer suggested was only an abstruse possibility. Below, I generalize the point, then draw some general lessons for revising logic.

5 Generalizing: recapture theorems

The case of *Tennant v Burgess in re Cut* isn't unique. Many heterodox logics, especially recent attempts to defend naïve theories of truth,[27] use theorems like CRT to compensate for logical weakness. For example:

> The nonclassical logician needn't doubt that classical logic is "effectively valid" in the part of set theory that doesn't employ the term 'True'; and this part of set theory suffices for giving a model-theoretic account of validity for the logic that is at least extensionally correct...We may indeed suppose that the advocate of the non-classical logic explicitly adopts as non-logical axioms all instances of $A \vee \neg A$ for any A in the language of set theory (or at least, in that part of set theory employed in the definition of validity). (Field 2008: 108–109)

In the presence of excluded middle, taken as a non-logical fact about a particular domain like mathematics, consequence relations like Field's "collapse" to classical consequence.[28] These "recapture" results show that in non-pathological contexts we recapture our classical toolbox by means of non-logical presumptions of classical validities.

If we interpret Field—calling the interpreted Field 'Field*'—as claiming that we should adopt a non-classical logic like those he's recently explored as our background logic,[29] then he *needs* to establish a recapture result in order to justify his use of classical metatheory.[30] Field thus needs to show or have shown (using $\models_{\mathfrak{F}}$ for Field's consequence relation and $\vdash_{\mathfrak{F}}$ for a corresponding proof-relation):

[27] Theories of truth that vindicate the intersubstitutivity of φ and ''φ' is true' in extensional contexts.

[28] The technique here is well-known. Restall (1993) cites it as a way for the logical deviant to escape from the charge that they must abandon most of mathematics.

[29] This is a non-trivial assumption; still, it's an interesting interpretation of Field's project that deserves scrutiny even if Field himself doesn't ultimately interpret his project in this way.

[30] The proof also requires defending that excluded middle holds for every relevant property invoked in metatheoretic reasoning (Field 2003). It's reasonable to wonder whether this can be done once we restrict ourselves to Field*-acceptable resources.

$$(RE_F)\, Q \vdash_{\mathfrak{F}} \forall \varphi, \Gamma \text{ ``}\Gamma \models_{\mathfrak{C}} \varphi\text{''} \rightarrow \text{``}A \vee \neg A, \ldots, \Gamma \models_{\mathfrak{F}} \varphi\text{''}$$

But his proof of RE_F uses a classical theory of syntax.[31] Field has thus only proved

$$(RE_C)\, Q \vdash_{\mathrm{C}} \forall \varphi, \Gamma \text{ ``}\Gamma \models_{\mathfrak{C}} \varphi\text{''} \rightarrow \text{``}A \vee \neg A, \ldots, \Gamma \models_{\mathfrak{F}} \varphi\text{''}$$

which is insufficient to justify his use of classical reasoning.[32]

Note that the problem here isn't with the presumption of excluded middle for all the relevant predicates appearing in a bit of classical reasoning that we want to preserve once we've adopted Field*'s perspective. This, of course, is problematic; Field views all non-semantic expressions as classical, in the sense of obeying excluded middle, but carving the semantic from the non-semantic is non-trivial. We can, though, spot Field* this assumption without losing the force of the above point. Even with the spot, Field* owes us a rigorous F-adequate proof of RE_F to *use* the instances of excluded middle. It's this which makes his position vulnerable to cycles like those of *Burgess v Tennant in re Cut*.

Field* could avoid this problem by boosting up the power of Q, adding in, say, all the metatheoretic instances of RE_F necessary to trivialize the proof of RE_F. This, however, seems like an unacceptable bootstrap; what justification could we have for simply taking for granted these sorts of principles given that other reasonable logics take these sorts of linking principles to be proved; Field's (2003) rough sketch of a more standard proof itself testifies to his agreement that this method is unacceptable.[33]

For those still tempted by moves like this, note that we could add into our criteria other intuitive theoretical virtues which would close off this bootstrap. For instance, it seems a virtue of a logic if it's able to base the required claims about its own metatheory on a minimal set of basic and obvious principles governing that domain. Once we've added such virtues, given plausible weightings of these new criteria, we can rejigger the above examples to reproduce the cycle.[34]

[31] There are other problems here. For instance, his proof invokes a form of mathematical induction that he rejects. See the proof sketched in endnote 9 of (Field 2003) which seems to invoke the principle form of mathematical induction, though the proof is difficult to parse. The substitution theorem there proved is essential in his later justification of classical methods in arithmetical contexts.

[32] Note that I am not arguing that there is no justification for this practice; I just claim that Field's proof of RE_C provides no direct warrant. Moreover, I'm not claiming such proofs are useless; they simply aren't sufficient to ward off the sort of worries I raised above. Halbach and Horsten (2006), for instance, give a similar proof that their preferred non-classical logic for truth—PKF—collapses to classical logic on sentences expressible in a mock-up of a language containing typed truth-predicates up to type ω^{ω}. Though they use a classical syntax language to prove this, they explicitly do not take this result to assuage worries about moving to such a weak logic as a background logic.

[33] His proof still makes use of classical logic, but it doesn't paper over the problems in the way we're currently considering.

[34] Thanks to Carlo Nicolai and a helpful referee for useful discussion of this point. Carlo Nicolai has pointed out (personal communication) a similar strategy for strong Kleene-logics which involves taking all the identity axioms for granted in the background syntax theory. As he noted, this violates the spirit of the strong Kleene approach, which seemingly should allow that identity axioms themselves aren't determinately true or false.

Field* could dig his heels in here, claiming that it's obvious that we can use classical logic in non-truth-involving contexts like the metatheory in which we develop rigorous semantics and proof-theory for a logic. Alternatively, he could claim empirical justification for the use of classical logic in non-truth-involving contests, arguing that a classical proof of something, in this particular context, is evidence of a corresponding non-classical proof. But it's really not obvious, given the weaknesses of Field*'s logic, that we have such justification. Presuming standard treatments of empirical justification misunderstands the radical nature of non-classical logic. Confirmation theory, probability, and the like depend on background entailment relations; our working understanding of such presumes principles of classical logic that non-classical logicians reject.[35] Even non-classical probability theories are typically developed in a classical meta- and working-theory, so they're of no dialectical use in a context when we require justification according to our working logic.[36]

Tendencies to heel-dig testify to the compellingness of reasons based on classical logic, even when given and assessed by non-classical logicians. If the question is what our background logic should be, then it's bizarre to let reasons only appreciable from our opponent's point of view—or unassimilated aspects of our point of view which were themselves informed by our opponent's point of view—inform our choices. Meadows and Weber gloss the intuitive point thus:

> However much one can say in reply to this [that the justification *for* a theory isn't acceptable as a justification *in* that theory], it certainly *seems* bad. A classical meta-theorist simply does not take non-classical logic as seriously as the rhetoric elsewhere insists one should. (Meadows and Weber 2016: 10)

It's similarly bizarre to employ reasons that would disappear as soon as we modified our background logic in accord with the output of our exercise of abductive methods (on which problem, see below and Woods (2018)).

Anyway, as we presumed above that the two reasonable approaches to working out how well a logic meets our abductive criteria—using our current background logic in the derivability role and using the logic under consideration in the derivability role—both allow the possibility of revision cycles, I'll put this possibility to the side. If there's justification for digging in one's heels on this issue, the burden of justification lies on the side of the heel-digger. If the case of recapture theorems is analogous to the case of *Tennant v Burgess in re Cut*, as it seems to be, then the problem I am pointing to is widespread and rather pressing. How should we proceed in assessing the merits of various logics given that our abductive methodology allows the possibility of such cycles?

35 See Sects. 4.1–4.2 of Woods (2018) for exploration of this general point.

36 See Meadows and Weber (2016) for related worries about (and limitations of) non-classical theories of computation.

6 Some responses

One response to the above puzzle claims that inability of a logic to justify the reasons for adopting it in its own terms is a badmaking feature which should be fed into the overall abductive assessment. A related response, suggested in the quote from Priest above, is that vulnerability of certain logics to this type of cycle indicates underlying incoherence in the logics under consideration. We will take these suggestions in reverse order.

The logics chosen here aren't bizarre or intuitively incoherent, as inspection of some of the work done *in* each will reveal. If there's underlying incoherence here, it's unclear what it's supposed to be. Note also that given the previous section, we can substitute many non-classical logics for Tennant and Field's logics, such as traditional relevance logics and many recent non-transitive and non-contractive logics viewed as *background logics*.[37] Each employs recapture theorems to legitimate using classical reasoning in certain contexts; each proves this recapture theorem classically.[38] It isn't clear what the underlying incoherence of each would be, other than that they're very weak when stripped of their respective recapture theorems. We can also substitute a wide variety of logics for classical logic in the above example; for example, comparing Tennant's classical core logic with intuitionistic logic or any of the intermediate logics between intuitionistic and classical logic would plausibly serve to generate an example of a two-cycle given the criteria above. Given the replicability of this situation for a wide variety of logics, presuming that logics vulnerable to the problems I've raised contain unnoticed incoherencies would be a pyrrhic save, at best.[39]

Turning now to the first suggestion, we need to distinguish it from LOGICAL PARTISANHOOD. If this inability isn't weighted, then it seems a *strengthening* of LOGICAL PARTISANHOOD and one which runs afoul of case where each logic gives conclusive reasons by its own lights to adopt a particular logic. If we fix this last worry, the difference remaining between it and LOGICAL PARTISANHOOD is one of formulation: should we pitch, as I have, this restriction as a constraint on abductive methodology or as a non-negotiable unweighted criterion *within* abductive methodology? As you like.

If the inability is a *weighted* criterion, outweighable by others, then we should worry that cycles will reemerge once we find the proper set of explanatory criteria. After all, why couldn't a logic have many advantages, so many so that *from* a classical perspective, this particular defect is outweighed? Even if not, there is a lingering sense of dissatisfaction. We would like an *explanation* of why abductive

[37] The caveat is important since one perspective on non-classical logics, recently explored by Ripley (2013) and Cobreros et al. (2012), takes classical logic as our background logic and the failure of transitivity to be a feature of an *extension* of classical logic involving semantic or vague vocabulary. Such an approach isn't as obviously open to my criticisms. I hope to address this perspective in future work.

[38] There are occasional welcome exceptions. See Weber (2012), Weber et al. (2015), and Rumfitt (2015) for virtuous examples of appropriate care in non-classical metatheorizing.

[39] It also isn't clear that it's legitimate to develop bespoke criteria mid-application of abductive methodology because of the inability of the methodology to render a coherent decision.

methodology is a useful method for justifying revising our logic. If the response is that given the logics under consideration, the inability to give all the reasons favoring the weaker logic in its own terms doesn't cause cycles, this seems like a lucky accident and that, by itself, would seem to undermine the ability of the abductive comparison to provide justification.

We have restricted ourselves to logics where one is a sublogic of the other. Removing this restriction, it's plausible that there are virtues and vices of even classical logic that can only be seen an independent perspective. The ability of a logic to preach its own virtues is thus itself plausibly *theory-dependent* in the sense suggested above. Better to take LOGICAL PARTISANHOOD seriously, pointing out agnosticism will be a defect *because* it corresponds to the inability of a weaker logic to make a case for itself that's analogous in strength to the case in favor of the weaker logic by the stronger logic. When we have the inability to preach, we should suspect abductive methodology won't yield a reasonable outcome and thus, by LOGICAL PARTISANHOOD, will not confer justification for revising our logic.

Jared Warren suggests a different reason to stay at *T*—since *T* has proof by cases and excluded middle, if we presume enough syntax, *T* can show that *C* proves that *T* proves CRT. *T*-proponents can then argue as follows: either $\vdash_{\mathrm{T}}$ CRT or $\nvdash_{\mathrm{T}}$ CRT. If the former, then we shouldn't move to *C* since the difference in strength isn't sufficiently large. If the latter, then since *C* proves CRT, *C* is unsound as it proves false things about *T*. Either way, we should not move back to *C*.

There are two problems with this. First, whether *T* proves CRT is potentially a logic-dependent matter. If we presume a classical background, there is simply a fact about whether *T* proves CRT (it does). But since we don't, it's possible that *T* and *C* differ about *T* entailing CRT; at least nothing we have said so far rules this out.[40] That *C* proves *T* proves CRT doesn't yet decide whether it's proving something false about *T*—this depends on whether *T* proves CRT when we condition the relationship of provability by *T*.

A less contentious response (also suggested by Jared Warren) notes that *T* already views *C* as unsound; after all, the whole point is to reject certain classically acceptable proofs. We can hardly figure use this fact as part of our decision as to whether to revise—this would be tantamount to internal *T*-criticism of what you can do with various classical principles instead of a language-level comparison of the virtues of the two logics. We might as well point out that since we've presumed that *T* doesn't prove CRT, moving to *C* allows us to avoid the now-unquestionable crippling weakness of *T*. Anyway, this argument does not seem to undermine the conclusions above, but emphasizes the nature of whole-logic abductive comparisons like those under consideration.[41]

Finally, it has been suggested that when we find ourself in such a cycle, we need to break it by arbitrarily choosing a logic, analogously to how we break ourselves

[40] Whether this situation is possible is a matter beyond this paper as it concerns questions about logical realism and the relationship of logic to actual reasoning practices.

[41] Thanks to Jared Warren for very useful discussion of this point and the general situation described here.

out of Buridan's ass-type cases.[42] However, in Buridan-style cases, we have evaluation of reasons of roughly equal strength. Buridan-type solutions seem more reasonable for cases where abductive methodology provides an balanced evaluation of the reasons in favor of alternative logics. For example, this seems to be the case when comparing the virtues of dropping *modus ponens* and dropping conditional proof to rescue a naïve theory of truth. Yet even in these cases, and certainly in our cyclic cases, LOGICAL PARTISANHOOD already provides a principled resolution of what seems an epistemically problematic choice situation; don't move unless you need to. If you (epistemically) need to, do what you gotta do.[43]

7 Wrapping up: a plea for neutrality

The whole point of using abductive methodology was conferring rational justification for moving from one logic to another. Given this fact, the existence of cases of revision cycles is rather pressing; we cannot simply ignore alternatives like Tennant's to classical logic as untenable. After all, by our very own lights, Tennant's logic looks superior to classical. We are not, if we are rational, entitled to simply ignore the competition without good reason. That is, we can't simply pretend Tennant doesn't exist or hope we don't run into him at a conference.

On the other hand, we should avoid getting into decision-theoretic cycles like those we'd get from running abductive methodology on classical and Tennant's logic. It's possible, but not obvious, that if we compared a wider class of logics we wouldn't have a problem. Perhaps one logic would obviously be superior by all the relevant lights. However, given the advantages of non-classical logics like Tennant's and Field's—*in the presence of recapture results*—it's hard to see this situation arising. Likewise, it's possible, but not obvious, that we would get a better result if we expanded the relevant criterial base to include other theoretical virtues. Just adding in virtues that would fix the problem, however, would be extremely ad hoc.

Even if these patches fixed the problem given the actual slate of logics on offer, two problems remain. First, this solution is ultimately unsatisfying—the mere possibility of these decision-theoretic cycles is already disturbing enough and points to a deep problem with applications of abductive methodology in the case of logic. Second, presumably one can cook up bespoke logics which cause such cycles for plausible choices of theoretical virtues; we could screen these off on grounds of something like unnaturalness, but this itself would call for the application of a constraint like the one I suggest below and one which is beholden to all kinds of background worries. After all, what is natural from one point of view might be completely unnatural from another.

Summarizing the general problem, implicitly discussed above, is easy enough. Since a background logic plays a role in fixing our evaluation of how well a logic

[42] Thanks to Oystein Linnebo and Kit Fine for discussion.

[43] See also fn. 7 above.

meets a certain criterion and, additionally, is part of what is up for grabs, we run into the problem that shifting our logic may shift our evaluation of how well a logic meets various criteria. This problem isn't *unique* to logic—a version of it has been discussed in terms of inductive methodology itself (Lewis 1971)—and it's natural to see how other fundamental bits of theory, such as arithmetic, low-level fragments of set theory, and the like can play a role in our evaluation of how well a theory meets some criterion or other.[44] Still, as logic is plausibly used in every way of constructing abductive justifications and the sort of mathematics needed to run abductive arguments is closely related to logic, this is a case of central importance.

Given how pervasive the problem is for actually existing heterodox logics, especially if recapture theorems themselves provide a case, a solution seems required. My suggestion is that we adopt:

> Logical Partisanhood: Unless the output of weighing the merits of my background logic against an alternative—on one hand by the lights of my own background logic and on the other hand by the lights of the proposed alternative—agree that moving to the alternative is no worse than staying with our current background logic, we ought to hold fast to our background logic.

to solve these kinds of problems. This dissolves the problem cases above since the logics in contrast do not agree about which logic is superior and, as we've seen, it explains many of the otherwise *ad hoc* fixes we explored above.[45] However, we might worry that adopting logical partisanhood rules out interesting cases of logical disputes which could be resolved on abductive grounds. Let me disabuse you of this concern.[46]

Consider intuitionistic and classical logic. There is enough overlap between these two logics that (a) a theory of syntax and inductive definitions can be formulated in the weaker intuitionistic logic, (b) each can be proven to have desirable properties, such as completeness, by commonly acceptable methods,[47] (c) the recapture

[44] See Woods (2018) for lessons we can draw from this fact in the case of skeptical arguments against realist theories of logic and mathematics.

[45] Additional complications arise when we relax the presumption that we're merely weighing two logics against each other in isolation of other alternatives. Since the sort of problems which cause these cycles typically have to do with a logic being too weak to develop an adequate metatheory in its own terms, it's extremely unlikely that we'll get interesting 3- or n- cycles if we don't already have 2-cycles. So logical partisanhood seems both sufficient to rule out the really troublesome cases and, moreover, is philosophically motivated. I hope to develop this discussion in detail elsewhere (but see Woods (forthcoming-a) for some initial considerations).

[46] Another worry with logical partisanhood is that it says that if we start in Tennant's position, that we should stay there. This seems right to me; by the intuitive picture motivating Tennant's position, moving to classical logic would involve more bad than good, including the ability to self-justify our move! Luckily, we tend to start, rationally or not, by accepting something far stronger than Tennant or Field's logic, so there's typically little worry here. We can also have justification for switching logics that doesn't arise from abductive theory comparison, such as the broadly pragmatic (arational) sort of justification discussed in Russell (2015). Anyway, the aim of epistemology is more staying out of tarpits than getting out of a tarpit we might be so unfortunate to start in. Thanks to Timothy Williamson for discussion.

[47] Though it should be noted that some theorists, such as McCarty (2002), claim with some plausibility that the standard proof of the completeness of intuitionistic logic in minimal logic (a logic weaker than

theorem connecting special cases of intuitionistic logic with classically acceptable reasoning—that decidable relations can be reasoned about classically without violating intuitionistic scruples—can itself be proved by entirely intuitionistically acceptable methods. This means that there is quite a bit of common ground for formulating criteria acceptable to both sides and where both sides can agree on how well one or the other meets these criteria. If there is enough common ground, then plausibly abductive methodology can be fairly applied in this case and render a definite verdict. Nigh-classical logics, such as super- and sub-valuational logics are likewise strong enough to allow reasonable comparisons between them and classical or intuitionistic logic.

If the heterodox logics discussed above can do a similar thing, then they would be on the same footing with intuitionistic logic *vis-á-vis* competition with classical logic. There are reasons to suspect they won't be able to do so, however. The logics under consideration, such as Field's favored and Tennant's classical core logic, are weak. It's only in the presence of the recapture theorems discussed above that they look to be serious competition for classical or intuitionistic logic. The recapture theorems themselves, however, have canonical classical proofs whose natural transformation into the heterodox setting is likely to be astronomically long and/or complicated. In the case of Tennant, a cut-free proof of the recapture theorem may not be produced for reasons of time and mortality. In the case of Field's favored, it's simply not obvious what the natural proof would be or, in fact, whether a reasonable theory of syntax and inductive definitions could be developed Field-accepatably.

So I close on a suggestion for the application of abductive methods as deliberative methodology in choosing a logic. If we are to take seriously the idea that we should choose a logic the way we would choose any scientific theory, and if we view this method as a way of constructing justifications for revision, then we need to be able to run the abductive comparison without serious risk of decision cycles. One way, the best way, of doing this is to have a common base from which we can assess the merits of each, such as we have with classical and intuitionistic logic. So, the best way for the non-classical logician to proceed when pushing their logic as a *background* logic, not a logic for applications, is to develop their justification of the merits of their logic in terms that both they and target logical orthodoxy can accept.

In the case of Tennant, this requires proving CRT in T. Similarly for Field* and RE_F. And, if not this, at least these theorists should develop a theory of syntax and inductive definitions in their favored background logic so as be able to a point to a reasonable proof-theory which is acceptable by their own lights. Until and unless these theorists do this, it seems entirely permissible to dismiss their abductive case in favor of their own logic out of hand. That is, it seems entirely reasonable to adopt LOGICAL PARTISANHOOD as a constraint on the use of abductive methodology in choosing a logic. That is, it seems entirely reasonable to be a logical partisan.

Footnote 47 continued
intuitionistic) is intuitively intuitionistically unacceptable. I put this concern aside as I am interested in showing that there *can* be interesting cases of logical disputes which can be arbitrated on abductive grounds. If this requires a slightly unnatural formulation of the justifications for intuitionism, so be it.

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