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LOGICAL PLURALISM AND VICIOUS REGRESSES

by

DANIEL BOYD

A dissertation submitted to the Graduate Faculty in Philosophy in partial fulfillment of the requirements for the degree of Doctor of Philosophy, The City University of New York.

2020

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Logical Pluralism and Vicious Regresses

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Daniel Boyd

This manuscript has been read and accepted for the Graduate Faculty in Philosophy in satisfaction of the dissertation requirement for the degree of Doctor of Philosophy.

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## **Abstract**

Logical Pluralism and Vicious Regresses

by

Daniel Boyd

Advisor: Graham Priest, Ph.D.

This material in this dissertation will be divided into two parts. The first part is a preliminary discussion of vicious regress arguments in the philosophy of logic in the 20th century. The second part will focus on three different versions of logical pluralism, i.e., the view that there are many correct logics. In each case an argument will be developed to show that these versions of logical pluralism result in a vicious regress.

The material in part one will be divided into three chapters, and there are a few reasons for having a preliminary discussion of vicious regress arguments in philosophy of logic. Many vicious regress arguments have been raised in the past with the aim of making some kind of point in the philosophy of logic. Looking at some of these historical examples will serve multiple purposes. Primarily it will provide an opportunity to think carefully about the structure of vicious regress arguments. Vicious regress arguments can be distinguished in terms of what their underlying assumptions are and what they ultimately aim to demonstrate. The successfulness of a vicious regress argument will always be a function of these two things. But thinking about the structure of vicious regress arguments will also be beneficial for another reason. It will provide for a useful

comparison to see how and in what way my own arguments relate to or differ from previous vicious regress arguments. Having cases to compare and contrast will help to clarify what assumptions I am making and where it will be important to reply to objections.

In chapter one, I'll look at a vicious regress that unfolds in Lewis Carroll's dialogue "What the Tortoise Said to Achilles?" (1895). I'll look at a few different ways that people have tried to extract a moral from Carroll's text, and I'll argue that our thinking about the moral should be guided by a prior question about how to understand the nature of the regress. I'll look at some different interpretations of the regress in Carroll, and I'll comment on why some interpretations may be more plausible than others.

Chapter two focuses on a vicious regress argument that is developed in Willard Van Orman Quine's "Truth by Convention" (1936). In some ways, it is easier to see what the intended moral of Quine's vicious regress argument is because he explicitly characterizes the view he aims to criticize. He aims to criticize a conventionalist thesis about logic where logical truths are fully explained in terms of linguistic conventions for logical connectives. I'll assume that Quine took himself to be criticizing a view held by Rudolph Carnap (1934/37).<sup>1</sup> I won't focus on the exegetical question of whether Quine's interpretation of Carnap is accurate, but I will look at a few passages from the Carnap material that Quine cited in his critique. Whatever the case may be with Carnap's actual view, Quine distinguished between two forms of conventionalism about logic. He only intended his regress argument to apply to a version of conventionalism about logic where conventions are understood as being somehow *explicit*. Quine developed a separate argument for a version of the conventionalist thesis about logic where conventions are understood as *implicit*. I'll discuss both of these arguments and the operative notions of convention. Much of the discussion in this chapter will concern the nature of Quine's regress argument and the extent to which its successfulness depends on the notion of convention at play. Quine saw his regress argument as based on the same kind of considerations in Carroll's dialogue. I'll note that there are both epistemic and non-epistemic interpretations of Quine's regress argument, and I'll argue that there are reasons to prefer a non-epistemic reading. I'll also look at a view from Jared Warren who develops an implicit convention version of the conventionalist thesis about logic. Warren responds to the critique of

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<sup>1</sup>I'll also note some views that challenge this assumption.

implicit conventionalism about logic from Quine, and I'll provide some reasons for thinking that Warren's response isn't successful. I'll end the chapter by making two observations about versions of conventionalism about logic that employ a conception of conventions where they are understood as implicit. I'll suggest that these views don't obviously avoid vicious regress worries, and that they also face worries concerning underdetermination (although I'll discuss these latter two points in more detail in chapter five).

In chapter three, I'll look at an argument from Saul Kripke (1974a/74b) that is also supposed to be inspired by the regress considerations in Carroll's dialogue. Kripke's argument is directed towards Quine's own views regarding the idea that logical hypotheses can be empirically revised. I'll explain Quine's view about the empirical revision of logical hypotheses and Kripke's criticism. I'll also comment on some of the similarities and differences between Kripke's argument against Quine and Quine's argument against Carnap (largely to argue that they are based on the same kind of underlying point). I'll also argue that there are some reasons for thinking that Kripke's argument may be based on a misinterpretation of Quine. I'll look at the interpretation that is needed in order for Kripke's challenge to be successful, and I'll criticize some arguments in support of this interpretation from Romina Padro (2015).

The second part begins with chapter four where I will look at a version of logical pluralism from Jc Beall and Greg Restall (2006). Beall and Restall's version of logical pluralism is based on a case-theoretic analysis of logical validity. I'll give an exegesis of their view, and then I'll argue that it results in a vicious regress. I'll spend some time talking about how I understand the nature of vicious regresses in this chapter, and the discussion of vicious regresses will be informed by a view from John Passmore (1961). I'll give an exegesis of Passmore's view, and I'll also devote quite a bit of space to an objection and reply section. Part of the objection and reply section will contribute to making a case for the claim that the vicious regress point is a consideration in favor of logical monism. In particular, I'll respond to an objection claiming that there is an analogous regress for logical monism. Without responding to an objection like this, the ultimate objective of my thesis would be incomplete (since there is only a consideration in favor of logical monism if it doesn't face an analogous puzzle). Chapter four will end with a discussion of a view from Colin Caret (2017). Caret develops a view where the details of Beall and Restall's theory are articulated in terms of

an indexical contextualist semantic theory for expressions like “logically valid”.<sup>2</sup> I’ll argue that a vicious regress can still be developed for a view like Caret’s where the details of Beall and Restall’s theory are understood in this way.

In the fifth chapter, I’ll look at a version of logical pluralism from Hartry Field. Field’s logical pluralism is developed by conjoining a normative conception of logical validity with a relativistic conception of normativity. I’ll devote a good deal of space to explaining Field’s normative conception of validity and how his form of relativism is understood. The main upshot of combining these two components is that it results in a view where validity attributions are understood as being somehow relative to *policies*. I’ll argue that Field’s view also results in a vicious regress, and I’ll look at a few objections to my argument. The objections will mostly concern an issue about whether the regress argument only works for certain conceptions of policies (in particular whether they are conceived of as being somehow *explicit*). So the issues here will be analogous to some of the concerns that are discussed in chapter two regarding Quine’s criticism of logical conventionalism. I’ll also raise a separate puzzle for Field’s view that is based on considerations of underdetermination. The details of this argument are informed by a criticism of dispositional analyses of rule-following from Kripke (1982). So I’ll spend some time in this chapter looking at responses to Kripke’s criticism from Tomoji Shogenji (1993) and Jared Warren (2018). I’ll argue that neither of these accounts will help to dissolve the underdetermination issue. The points in this chapter (concerning regress and underdetermination) are also the ones I mentioned I would come back to in chapter two.

In the sixth chapter, I’ll look at Stewart Shapiro’s version of logical pluralism (2014). It is also based on a form of relativism, but it is distinctive in that it is developed in terms of considerations in the philosophy of mathematics. I’ll provide an exegesis of Shapiro’s view, and I’ll argue that it also faces a vicious regress puzzle. It’s worth noting that Shapiro gives a semantic characterization of his view. He gives a detailed description of his view about the meaning of logical connectives and expressions like “logically valid”. He describes his view on the semantics of expressions like “logically valid” as a form of indexical contextualism (although there are some

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<sup>2</sup>In chapters four and six, my use of theoretical labels like “indexical contextualist”, “non-indexical contextualist”, and “assessment sensitive” will follow the usage of MacFarlane (2014). Shapiro (2014) cites MacFarlane when explaining his usage of these technical terms, and Caret’s view fits the definition of indexical contextualism that is given by MacFarlane (although Caret cites other sources when developing his view).

important qualifications to this claim which I'll discuss). I'll argue that a vicious regress can be developed for Shapiro's view even when the details of his indexical contextualist semantic theory are taken into consideration. I'll also argue that Shapiro's view faces an underdetermination puzzle. These points about underdetermination will be similar to what is discussed in the chapter on Field, but the argument will concern details that are specific to Shapiro's semantic theory. A key point of focus will be Shapiro's view of contexts and the role that contexts are supposed to play in his indexical contextualist theory of "logically valid".

# Preface

Logical monism is the idea that there is only one correct logic. This idea is somewhat vague because there isn't a single way to articulate what a logic is or what it means for a logic to be correct. But vagueness aside, logical monism is the orthodox view. According to one way of making the idea more precise, a logic can be understood as a formal theory of logical consequence.<sup>3</sup> Then the logical monist position can be characterized as the view that there is a single correct theory of logical consequence.

One challenge to the orthodox view is known as logical pluralism. This is the idea that there is more than one correct logic.<sup>4</sup> In what follows, I'll look at a few different versions of the logical pluralist thesis and I will argue that there are theoretical considerations in favor of logical monism. I won't be raising much of an issue about what exactly it takes for something to count as a form of logical pluralism. My plan will be to focus on some influential versions of the idea, and to argue that there are theoretical difficulties in each case. More specifically, I'll argue that certain versions of logical pluralism result in a vicious regress. To the extent that my arguments are successful, it should show, at least in the cases where I focus my attention, that there is a theoretical consideration in favor of logical monism. I'll say more about the details of this below, but first I'll say something about why any of this might be worth thinking about at all.

Unless there is some story about why logic matters, it's not clear why anyone would care

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<sup>3</sup>This invites a question about what logical consequence is. Logical consequence is typically characterized as a kind of necessary truth preservation relation that holds in virtue of "logical form". There's no single way of understanding the notion of logical form, or what the relata of logical consequence are supposed to be. But for the reader who is interested in hearing more about the philosophical issues regarding logical form, see MacFarlane (2000). For more on the issue of how to understand the relata of logical consequence, see Russell (2008).

<sup>4</sup>See Haack (1978) for a historically influential discussion of logical pluralism, and Russell (2019) for an overview of many different versions of the idea.

about the dispute between logical monists and logical pluralists. So why would anyone care about logic, or more specifically about logical consequence? There are different approaches for how to answer this more general question about why logic matters, but a standard story is that logic matters because of a connection that it has with reasoning.<sup>5</sup>

Reasoning matters because it is pervasive in just about everything that we do.<sup>6</sup> We reason in everyday ordinary affairs, and reasoning is also an integral part of our scientific, political, and legal practices. From a more philosophical perspective, reasoning matters because we have an interest in understanding the world around us and how we fit into it. A natural (although not unquestionable) assumption is that we cannot understand the world and our place in it without first understanding our own perspective and how it is structured. But our understanding of ourselves and how we form a perspective of the world will be painfully empty without an understanding of how we reason. It is through reasoning that we form our view of the world, so without an understanding of reasoning, it's not clear how stereotypical aspirations of philosophy could ever be met.

However we understand the importance of reasoning, a striking feature of it is that instances of reasoning can be categorized as good or bad. And this is a place where logic can be cited as relevant to reasoning. In some way, logic is part of the story of what makes good reasoning good.<sup>7</sup>

This gives us some idea of why logic might matter, but none of this is to say that the connection between logic and good reasoning is entirely straightforward. If we want to further our understanding of logic and how logic relates to other things (like good reasoning), then we have to ask questions about what logic is. And this provides us with one avenue for thinking about why the monism-pluralism dispute might be worth thinking about. If some version of the logical

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<sup>5</sup>Even for those who are skeptical about the existence of an interesting connection between logic and reasoning (e.g. see Harman (1986)), the nature of logical consequence should still be understood as a philosophically significant issue. There are philosophically significant ideas that only have substance relative to a theory of the nature of logical consequence. For example, the logicist thesis in philosophy of math (i.e. the view that mathematics in some way reduces to logic) will have a different content relative to different views about the nature of logic. MacFarlane (2000) cites Curry as making this point (about how the sense of views in philosophy of mathematics can depend on a prior understanding of logic).

<sup>6</sup>When discussing reasoning, I mean to describe something that includes the activity of deductive inference. I don't mean to assume that this is the only thing that goes into human reasoning; human reasoning is a highly complex affair. I also don't mean to assume that it is obvious what deductive inference consists in, although I will discuss a few different ways of thinking about this in chapter one.

<sup>7</sup>I don't mean to say that deductive logic is uniquely relevant in an account of good reasoning. Good reasoning involves many things and deductive validity is at most only part of the story.

pluralist thesis were true, this could have ramifications for how we understand the nature of logic and the relationship between logic and good reasoning. For example, it's natural to think that if logical pluralism were true, there would be more room for permissiveness in regards to what counts as good reasoning.<sup>8</sup> None of this is to say that there aren't other questions about logic that are of interest when it comes to the connection between logic and good reasoning. Any number of questions might matter. Is logic a product of the mind? Is it objective? Is it a matter of language? Can it be understood in material terms? Ultimately the monism-pluralism question has to be situated within a nexus of other issues that will intersect in interesting ways. While investigating any of these issues might be beneficial for our understanding of the nature of logic, it's easy to see how the monism-pluralism issue could be relevant to questions about the nature of logic and the story of how logic relates to good reasoning.

Whatever may be said about the importance of the monism-pluralism issue in logic, it has generated a lot of popular interest in the last 20 years. Much of this recent interest is due to the work of Jc Beall and Greg Restall (2006). But there are also earlier interesting precursors that influence contemporary discussion of the topic. Rudolph Carnap's tolerance principle (1934) is a case in point. Since Beall and Restall's work, many different versions of the logical pluralist thesis have been developed and much ink has been spilled arguing for or against various versions of the view. In the following, I will look at a few of these more recent versions of the logical pluralist thesis in order to argue that they face vicious regress worries. Whatever one's stance on the issue, my hope is that the following will provide an opportunity for careful thought about the nature of logic.

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<sup>8</sup>I don't mean to imply that any version of logical pluralism will imply a greater amount of permissiveness in regards to what counts as good reasoning. The devil will always be in the details of how a version of logical pluralism is formulated. For example, some versions of logical pluralism would have it that different logics apply to different domains. For those views, there may be no sense in which there is more than one right way to reason about a given subject matter. But this is all the more reason for thinking about different versions of logical pluralism and whether plausible versions of the view have interesting consequences for our understanding of the theoretical roles that logic is thought to play.

*Parmenides*: I fancy the consideration which leads you to imagine the existence of these various unitary forms is to this effect: when you have judged a number of things to be large, you presumably pronounce, in a review of them all, that they present one and the same pattern, and this is why you regard the large as one thing.

*Socrates*: Precisely so.

*Parmenides*: But what of *the* large and other large things? When you pass them all mentally in review in the same fashion, must this not again give rise to the appearance of a single large something, in virtue of which they all appear large?

*Socrates*: Presumably.

*Parmenides*: Consequently a second form of magnitude will present itself, distinct alike from *just* magnitude, and from the things which participate of magnitude. On a fuller view of all these cases, we shall discover yet a further form, in virtue of which they will all be large; thus, you see, every one of your forms will no longer be one, but an indefinite plurality.

Plato, *Parmenides*, translation: A. E. Taylor.

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I would also like to thank my committee members: Michael Devitt, Hartry Field, and Gary Ostertag. I am grateful to each one of them for their willingness to support me in this project. Each of them provided feedback and comments at various stages of the writing process, and I cannot thank them enough for their attention, insight, and encouragement along the way. They are responsible for so many improvements to the material here, and without them this dissertation would not have been possible.

## Part I

# Vicious Regress Arguments in 20th Century Philosophy of Logic

# Chapter 1

## The Regress in Lewis Carroll's

### Dialogue

In this chapter, I'll examine a vicious regress that is developed in Lewis Carroll's "What the Tortoise Said to Achilles" (1895). Carroll writes in dialogue format and the regress is something that manifests in a conversation that takes place between the characters in his story. I'm taking Lewis Carroll as a starting point because it is a major influence on discussions of certain vicious regress arguments in philosophy of logic in the 20th century. Chapters 2 and 3 will involve a discussion of vicious regress arguments from Quine and Kripke and they both refer back to Carroll as a source of influence on their arguments.

In section 1.1, I'll give an exegesis of Carroll's dialogue, and the discussion that follows will be guided by a question about what the moral of the dialogue is supposed to be. I'll comment on some of the issues about how to understand the moral in 1.2, and I'll suggest that how we think about the moral should depend on how we think about the nature of the regress in Carroll's dialogue. I'll also look at two different ways to think about the nature of the regress. In section 1.3, I'll look at an interpretation of the regress where it is understood as something that concerns causal necessity. In section 1.4, I'll consider an interpretation where the regress is understood as something that concerns epistemic justification. I'll conclude by noting that while the epistemic interpretation may be more prevalent, neither interpretation is ruled out. All of this should set a

proper baseline for part one because it provides a good example of how we can think about the nature of vicious regress arguments (and how they can be characterized in terms of their underlying assumptions and ultimate aim).

## 1.1 Lewis Carroll's Dialogue

There are two characters of interest in Lewis Carroll's dialogue.<sup>1</sup> One of them (a tortoise) and the other (named "Achilles") engage in a back and forth where a regress unfolds. The points of exegetical interest, for our purposes, begin when the characters consider a sequence of claims concerning Euclidean geometry.

(A) Things that are equal to the same are equal to each other.

(B) The two sides of this triangle are things that are equal to the same.

(Z) The two sides of this triangle are equal to each other. (Carroll 1895: 691)

Upon considering the sequence, the tortoise says something that appears to be partly a question and partly a claim about how readers of Euclid would understand certain logical relationships that hold between the claims *A*, *B*, and *Z*.

Readers of Euclid will grant, I suppose, that *Z* follows logically from *A* and *B*, so that anyone who accepts *A* and *B* as true, *must* accept *Z*, as true? (Carroll 1895: 691)

There are a couple of things worth noticing about the tortoise's comment. It invokes a notion of one thing *following logically* from something else, as well a notion of *necessity* (expressed by the italicized "*must*"). Some of the main interpretive questions center on how to understand figures of speech like this, but I'll return to this point after looking at the main points in the dialogue.

The tortoise and Achilles both consider two possible attitudes that a reader of Euclid could take towards the sequence. First, they grant that a reader might take the sequence to be valid, but not accept *A* and *B* as true. This can be seen in the following piece of dialogue:

“ And if some reader had *not* yet accepted *A* and *B* as true, he might still accept the *sequence* as a *valid* one, I suppose? ”

---

<sup>1</sup>Carroll "What the tortoise said to Achilles", *Mind* 4. (14): 278-280 (1985)

“ No doubt such a reader might exist. He might say ‘I accept as true the Hypothetical Proposition that, *if*  $A$  and  $B$  be true,  $Z$  must be true ; but, I *don't* accept  $A$  and  $B$  as true.’ Such a reader would do wisely in abandoning Euclid, and taking to football.” (Carroll 1895: 691)

This is the first time that the word “*valid*” appears in the dialogue, but it appears to be used in a way that corresponds to the previous use of “follows logically”. The passage also makes reference to a conditional claim “*if*  $A$  and  $B$  be true,  $Z$  must be true”. This conditional is referred to as a hypothetical proposition, and it receives the name “ $C$ ” later in the dialogue. On one way of understanding this hypothetical, it can be seen as merely giving voice to the idea that the sequence is valid.<sup>2</sup>

The tortoise and Achilles also suppose that a reader could accept  $A$  and  $B$  as true, but nevertheless reject the hypothetical proposition.

“ And might there not *also* be some reader who would say ‘ I accept  $A$  and  $B$  as true, but I *don't* accept the Hypothetical’ ? ”

“ Certainly there might. *He*, also, had better take to football.” (Carroll 1895: 692)

At this point in the dialogue, the tortoise and Achilles assume that something holds for both types of people. They assume that neither of them is under any “logical necessity” to accept  $Z$ .

“ And *neither* of these readers,” the Tortoise continued, “is *as yet* under any logical necessity to accept  $Z$  as true ? ”

“ Quite so,” Achilles assented. (Carroll 1895: 692)

There are questions about how to understand this notion of logical necessity (just as there are questions about how to understand the previously mentioned italicized “*must*”), but I will come back to this after saying a bit more about how the dialogue develops into a regress.

What is important now is that the tortoise offers a challenge for Achilles. He tells Achilles that he wants him to imagine that the tortoise is like the second type of person. That is, he wants Achilles to imagine that the tortoise accepts  $A$  and  $B$ , but doesn't accept the hypothetical proposition  $C$ . But the tortoise doesn't only want Achilles to imagine this. He wants Achilles to imagine this *and then* “logically force” the tortoise to accept  $Z$  as true.

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<sup>2</sup>The hypothetical could also be interpreted as stating a consequence of the fact that the initial sequence is valid.

“ Well, now, I want you to consider *me* as a reader of the *second* kind, and to force me, logically, to accept *Z* as true.” (Carroll 1895: 692)

There is a question at this point about what it means to be logically forced to accept *Z*. But whatever it means to be so logically forced, we do know something about how the tortoise and Achilles think the logical force can be escaped.<sup>3</sup> Recall that they think there are at least two ways: (1) not accepting *A* and *B*; (2) not accepting the hypothetical *C*.

It has already been granted that the tortoise will accept *A* and *B*. Given this, Achilles thinks, that in order for there to be any logical force upon the tortoise to accept *Z*, the tortoise will have to accept *C*. Achilles says

Then I must ask you to accept *C*. (Carroll 1895: 692)

This is all very well with the tortoise as he is perfectly happy to accept *C*. He asks Achilles to write down *C*, and then they consider a new sequence, exactly like the first, except that *C* has been added between *B* and *Z*.

(*A*) Things that are equal to the same are equal to each other.

(*B*) The two sides of this Triangle are things that are equal to the same.

(*C*) If *A* and *B* are true, *Z* must be true.

(*Z*) The two sides of this Triangle are equal to each other. (Carroll 692)

Achilles thinks the tortoise must, at this point, accept *Z*. But the tortoise asks why.

If you accept *A* and *B* and *C*, you *must* accept *Z*.”

“And why *must* I ?” (Carroll 1895: 692)

Achilles responds to this question by pointing out the fact that *Z* follows logically from *A*, *B*, and *C*.

Because it follows *logically* from them. If *A* and *B* and *C* are true, *Z must* be true. You don't dispute *that*, I imagine? (Carroll 1895: 692)

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<sup>3</sup>There's a question about how to understand the relationship between all these things (the “*must*”, the logical necessity, and logical force). I take it that the force and some uses of the “*must*” come to much the same thing, and that they are somehow tightly linked with the notion of logical necessity.

Here we see an italicized “*must*” again. But this time it is not explicitly about what anyone must accept (given that they make certain assumptions). It is rather about what must be true (given that some other things are true). Perhaps Achilles is just giving voice to a necessary truth preservation account of logical consequence. But regardless of what is going on here, the tortoise locks on to the latter statement about truth preservation, and notes that it is a hypothetical. The tortoise says that if he doesn't accept this hypothetical, then he might accept *A*, *B*, and *C* without accepting *Z*. Then Achilles grants that this is possible.

“ If *A* and *B* and *C* are true, *Z must* be true,” the Tortoise thoughtfully repeated. “ That's *another* Hypothetical, isn't it ? And, if I failed to see its truth, I might accept *A* and *B* and *C*, and *still* not accept *Z*, mightn't I ? ”

“ You might,” the candid hero admitted ; “ though such obtuseness would certainly be phenomenal. Still, the event is *possible*. (Carroll 1895: 692)

In light of this, Achilles thinks he needs to ask the tortoise to grant the following hypothetical: if *A*, *B*, and *C* are true, *Z* must be true. This hypothetical is named “*D*”, and the tortoise also grants it as well.

So I must ask you to grant *one* more Hypothetical.”

“ Very good. I'm quite willing to grant it, as soon as you've written it down. We will call it

(*D*) If *A* and *B* and *C* are true, *Z* must be true. (Carroll 1895: 692)

At this point Achilles thinks the tortoise will accept *Z*, but the tortoise is not so sure.

Now that you accept *A* and *B* and *C* and *D*, *of course* you accept *Z*.

“Do I ? ” said the Tortoise innocently. “ Let's make that quite clear. I accept *A* and *B* and *C* and *D*. Suppose I *still* refused to accept *Z*? ” (Carroll 1895: 692)

Achilles still seems to think that he has succeeded at this point. He says that logic would somehow force the tortoise at this point and that the tortoise would have no choice.

“Then Logic would take you by the throat, and *force* you to do it ! ” Achilles triumphantly replied. “ Logic would tell you ‘ You ca'n't help yourself. Now that you've accepted *A* and *B* and *C* and *D*, you *must* accept *Z* ! ’ So you've no choice, you see.” (Carroll 1895: 693)

The tortoise at this point thinks that a new hypothetical has entered the discussion: If  $A$  and  $B$  and  $C$  and  $D$  are true,  $Z$  must be true. The tortoise thinks that if he doesn't grant this hypothetical, he needn't grant  $Z$ .

( $E$ ) If  $A$  and  $B$  and  $C$  and  $D$  are true,  $Z$  must be true. Until I've granted *that*, of course I needn't grant  $Z$ . (Carroll 1895: 693)

It's worth noting that Achilles hasn't actually stated the conditional  $E$  explicitly. In  $E$ , there is no explicit reference to what anyone must accept (it only explicitly states something about what must be true, given the truth of the antecedent). But if you look at the second to last passage above, Achilles uses a hypothetical that explicitly refers to conditions under which  $Z$  must be accepted. In the last passage above, the tortoise says that he needn't accept  $Z$ , even when he has granted  $A$ ,  $B$ ,  $C$ , and  $D$ , as long as he doesn't accept the hypothetical  $E$ . True to form, Achilles agrees.

"I see," said Achilles (Carroll 1895: 693)

At this point, it should be obvious to the reader of the dialogue that this process can go on forever. Before the dialogue ends, it skips to a point in the future where Achilles and the tortoise have gone through more than 1000 steps of the reasoning exhibited in the initial dialogue. So how should the moral of the story be understood?

## 1.2 Understanding the Dialogue and Deriving a Moral from the Story

There are a few main interpretive questions I'd like to address.<sup>4</sup> The main question is what the moral of the story is supposed to be. But how someone understands the moral will depend on how they understand the regress (and whether it is vicious).<sup>5</sup> So what is the nature of the regress? Is it a regress of belief? Is it a regress of inference? Does it somehow concern epistemic justification or rationality? Before we answer any of this, we need to ask what exactly the tortoise is demanding when he challenges Achilles to logically force him to accept the claim  $Z$ . We also need to ask what the conditions are for when the logical force is supposed to be in play (according to the characters in the dialogue).

As I read the dialogue, the tortoise never grants that any logical necessity is in play. There are several places in the dialogue where Achilles thinks that the logical force is present, but the

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<sup>4</sup>There is a theme in commentary on Carroll's dialogue where it is assumed that a mistake is being made, but can be corrected if certain distinctions are made and attended to. A good example of this is Russell (1903: 35-37). He distinguishes between a notion of "therefore" and "implies" which is based on a distinction between asserted and merely considered propositions (where assertion and consideration are not construed psychologically). I won't go further into the details of Russell's view because he does not explicitly relate it to the details of Carroll's text, although it is worth mentioning that he expresses some hesitancy about his view saying there are "grave difficulties in forming a consistent theory on this point". Hesitancy aside, this kind of strategy has been taken up by others as well.

Rhees (1951) for example argues (through a dialogue with his own tortoise and Achilles) that the regress can be resolved through certain key distinctions. In his dialogue, it is granted that  $C$  is needed, but not needed "on the same footing as  $A$  and  $B$ ". (1951: 243) The idea is that  $D$  is an illicit conditional because it treats  $C$  as a premise (i.e. on the same footing as  $A$  and  $B$ ). A preferred conditional is described where "instead of saying that, if one accepts  $A$  and  $B$  and  $C$  one must accept  $Z$ , we must now say that, *if* one accepts  $C$ , *then*, if one accepts  $A$  and  $B$ , one must accept  $Z$ ." (1951: 244) The tortoise is willing to accept this preferred conditional (and this is called accepting  $C$  as a "meta-premiss").

I think there are two main questions for approaches of this kind. On the one hand, is the supposedly key distinction really defensible (or clear in the first place)? At least Russell seems to grant that this is a worry in his own case. On the other hand, has it really been clarified how the distinction is supposed to help? I think this latter issue is especially pressing in the case of Rhees. For example, why would adding  $C$  as a meta-premiss matter? And why wouldn't further meta-premisses be needed? Rhees's Achilles says that "if one such meta-premiss is true, the premisses referred to *in* that meta-premiss are in fact *sufficient* to imply a conclusion. Any additional meta-premisses therefore are *quite* superfluous." (p. 246). But  $A$  and  $B$  by themselves were already sufficient to imply  $Z$ , and the tortoise already accepted those before  $C$  was ever mentioned or accepted as a meta-premiss. So if further meta-premisses are superfluous, it would seem that accepting  $C$  as a meta-premiss is superfluous as well.

<sup>5</sup>In later chapters (especially in chapter 4) I will go into detail about how I understand what it is for a regress to be vicious. In some of the discussion in this chapter, I'll discuss viciousness as something that could be based on the idea that there is a problem with an individual having infinitely many explicit beliefs. But I'll also sometimes discuss viciousness as something that is based on the idea that a proposed solution (or explanation) would re-introduce a worry that it was originally brought in to resolve. This latter way of thinking about vicious regresses is in line with a view from Passmore (1961) and it is consonant with the understanding of viciousness that is operative in chapters 4-6.

tortoise always convinces him otherwise. The tortoise seems to think the logical force can always be avoided as long as certain hypotheticals are rejected (which link the tortoise's assumptions with  $Z$ ). It seems like the moral of the story is that some kind of mistake is being made.<sup>6</sup> But what exactly is the mistake? To answer this, we should first turn to the question of how to understand the logical necessity that is referenced in the dialogue.

Recall the place in the dialogue where the tortoise asks Achilles to suppose that the tortoise is like the “second” kind of reader of Euclid, i.e., that he accepts  $A$  and  $B$ , but doesn't accept the hypothetical “If  $A$  and  $B$ , then  $Z$ ”. Under this supposition, the tortoise asks Achilles to force him by logic to accept  $Z$ .

Well, now, I want you to consider *me* as a reader of the *second* kind, and to force me, logically, to accept  $Z$  as true. (Carroll 1895: 692)

What is the tortoise demanding that Achilles do here? I think there are two main interpretations to be considered. In 1.3, I'll discuss an interpretation of the logical force where it is understood as something that would somehow causally guarantee that the tortoise draws an inference.<sup>7</sup> In 1.4, I'll consider an interpretation of logical force where it is understood as something that would make the tortoise epistemically justified in inferring  $Z$ .<sup>8</sup>

### 1.3 An interpretation of logical force where it is understood as a kind of Causal Necessity

On one interpretation, the tortoise is asking Achilles to find conditions that would, given that the tortoise accepts  $A$  and  $B$ , guarantee that the tortoise infers  $Z$ . This would be an understanding of “force” that looks to be somehow causal. And it would give an interpretation of the italicized “*must*” that corresponds to a kind of causal necessity.

If this is right, it cannot be just any kind of causation that we are interested in, because

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<sup>6</sup>Or perhaps the moral may be that the tortoise is not even a possible agent.

<sup>7</sup>This is on the assumption that the tortoise doesn't already accept  $Z$ .

<sup>8</sup>To be more specific, it might be something that concerns a kind of epistemic obligation. Talk of justification can be interpreted as being more like a permission than an obligation, and force is more naturally understood in terms of obligation.

the tortoise is not looking for just any conditions where he would transition from  $A$  and  $B$  to  $Z$ . For example, given that the tortoise accepts  $A$  and  $B$ , there might be certain ways of bumping the tortoise on the head that would result in the tortoise being caused to accept  $Z$ . Or there might be some way of priming the tortoise so that he transitions from  $A$  and  $B$  to  $Z$  as a result of some merely associative process. So the causation would have to be some kind of “non-deviant” causation.<sup>9</sup> Perhaps then, on this analysis, the tortoise’s challenge can be understood in the following way. He is asking for conditions where he would *infer*  $Z$  from  $A$  and  $B$ , as opposed to conditions that would merely cause him to accept  $Z$  as a result of an associative transition (or bump on the head etc.).<sup>10</sup>

If this interpretation is right, then we can ask what Achilles thinks is necessary in order for an inference to be drawn. At a certain point in the dialogue, Achilles thinks that if there is going to be any logical force at all, then the tortoise will have to accept the hypothetical  $C$ . The hypothetical  $C$  says that  $A$  and  $B$  provide conditional support for  $Z$ . On the presently considered interpretation, this means that Achilles initially assumes the following as a necessary condition for the inference to take place. He thinks that in order for the tortoise to infer the conclusion, the tortoise will have to recognize that the premises support the conclusion (and that is just what accepting  $C$  amounts to).<sup>11</sup>

Given this set up, a regress can unfold in the following way. Suppose that accepting a claim like  $C$  is a necessary condition for an inference to be drawn from  $A$  and  $B$  to  $Z$ . If  $C$  is being

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<sup>9</sup>See Davidson on non-deviant causation in intentional action in “Actions, Reasons, and Causes” (1963), and “How is Weakness of the Will Possible?” (1969).

<sup>10</sup>It’s worth noting that term “inference” is used in so many ways that it’s not always obvious how to isolate the relevant object of inquiry. Here I’m assuming a notion of inference where it is the kind of thing that could transmit epistemic justification for belief (or perhaps be the kind of thing that someone can be held responsible for). So it may not be a notion of inference that could be performed by certain less sophisticated animals (e.g. mosquitos) or powerful computer programs (e.g. AlphaGo). I won’t try to give an analysis of what inference is, and I won’t try to settle difficult questions about whether or in what way inferences can happen unconsciously. That being said, at the end of the day, a story about the conditions under which inference occurs will probably have to include information about memory, attention, and many other things.

<sup>11</sup>Paul Boghossian, in recent work, argues that a condition like this is necessary for an inference to be drawn. He says that if an inference from  $A$  to  $B$  is to occur, this must happen partly because the agent takes  $A$  to support  $B$ . He calls this the “taking condition”. See his “What is Inference” (2014), and “Inference, Agency, and Responsibility” (to appear in Balcerak Jackson (ed.): OUP Volume on Reasoning). But it’s a question of interest what the attitude of “taking” would ultimately have to amount to. Boghossian considers doxastic as well as intuition based accounts of the taking condition, and he ultimately prefers an account that is given in terms of a specific notion of rule following. All of these proposals are important for understanding a certain kind of response to the Carroll dialogue. If the moral isn’t to reject recognition of  $C$ , then recognition of  $C$  must be understood in a way that does not generate a regress. For a critical discussion of Boghossian’s view, see Mark Richard’s “Is Reasoning a form of Agency” (to appear in the same volume), and McHugh and Way “Against the taking condition”. (2016)

added as a *further premise*, and inference requires the recognition of a support relation between premises and conclusion, then it will also be necessary for the tortoise to recognize that  $A$ ,  $B$ , and  $C$  support  $Z$ . In other words, the tortoise would have to recognize a further hypothetical. And if this further hypothetical is added as a premise, then we can continue this reasoning to show that the tortoise would have to accept an infinite number of hypotheticals in order to draw the initial inference. This is already a vicious regress.<sup>12</sup>

There is another way of seeing what may be the other side of the same coin. If we focus our attention on the question of what role the recognition of  $C$  would play in the generation of the inference, we can ask what the recognition of  $C$  would add that wasn't already present in the recognition of  $A$  and  $B$ . If the recognition of  $A$  and  $B$  wasn't by itself sufficient to generate the inference, then why would the recognition of  $C$  be one of the important missing ingredients in the story of how the inference gets generated? One idea is that the addition of  $C$  sets the tortoise up with three premises ( $A$ ,  $B$ , and  $C$ ) of a logically valid argument. In fact, when the tortoise asks Achilles why he must accept  $Z$  (given his acceptance of  $A$ ,  $B$ , and  $C$ ), Achilles says something to just this effect.

Because it follows *logically* from them. If  $A$  and  $B$  and  $C$  are true,  $Z$  *must* be true. You don't dispute *that*, I imagine ? (Carroll 1895: 692)

But the problem with this should be obvious. Before the recognition of  $C$ , the tortoise already accepted the premises of a logically valid argument with  $Z$  as the conclusion. If the tortoise didn't infer at the stage before the recognition of  $C$ , it's not clear why the tortoise would infer at any later stage through the acceptance of  $D$ ,  $E$ ,  $F$ , etc.. Put differently, if the recognition of  $C$  were useful on the grounds that it provided a premise for a logically valid argument, it would only be adding

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<sup>12</sup>One way of thinking about why this would count as vicious is because it would require an individual to have an infinite number of explicit beliefs in order to draw an inference. Since explicit beliefs take up storage space in the mind, this would make inference impossible. These kinds of issues are familiar from the "infinetist" responses to Agrippa's trilemma. One way of trying to make sense of the idea that a person could have infinitely many beliefs is to understand beliefs as things that can be held somehow implicitly. Perhaps then someone could suggest that infinitely many hypotheticals could be accepted implicitly (where this is explained in terms of dispositions to accept explicit claims). That might remove some worries about computational overload, but it would introduce other problems. If an agent accepts a hypothetical implicitly, it's not clear why they would count as accepting the hypothetical as a premise. It's also unclear how tacitly accepting infinitely many premises is supposed to be relevant in an explanation of why the tortoise would draw an inference. Saying the tortoise is disposed to accept  $Z$  is not the same thing as saying that the tortoise infers  $Z$  (since we have to distinguish inference from merely associative transition). For a defense of the infinitist perspective in the case of epistemic justification and helpful critical discussion see Klein (2003, 2006, 2007) and Wright (2013).

something that was already present in the acceptance of  $A$  and  $B$  by themselves.<sup>13</sup>

So we have an interpretation where a regress is generated on the basis of assumptions about the necessary conditions for an inference to be drawn. Even though this interpretation is not focused on the issue of whether an inference is epistemically justified, this doesn't mean that the moral of Carroll's story is epistemologically insignificant. It would actually be very significant because it would tell us something about the nature of inference, and inference is central to the story about how we form justified beliefs about the world around us.

So what is the moral of the story according to this interpretation? There is more than one way to respond to the dialogue. Probably the most natural response would be to say that in order for a deductively valid inference to be drawn (like the one from  $A$  and  $B$  to  $Z$ ), it cannot be necessary for an agent to accept a claim like  $C$ . In other words, in order to draw an inference, it

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<sup>13</sup>There is a regress here in the following sense. If something further is required at the first stage (with  $A$  and  $B$  by themselves), then something further will be required at the second stage as well (because the second stage didn't add anything of significance that wasn't already present at the initial stage). But similar reasoning will tell us that something will be required at every further stage as well. However, if the recognition of  $C$  isn't adding anything new that would be of use (that wasn't already present in the acceptance of  $A$  and  $B$ ), then it's not clear how anything of use would be added by the recognition of  $D$ ,  $E$ , etc.. So we never arrive at a stage where something useful has been added.

It's important to note that a key point regarding inference can be made without mentioning any infinite regress at all. Independently of any regress that would emerge, we can point out that there doesn't seem to be any significant difference between the stage prior to adding  $C$  and the stage where  $C$  is added. If it wasn't necessary for the tortoise to infer at the first stage, it won't be necessary for the tortoise to infer at the second stage because the new stage adds nothing of significance that wasn't already present at the first stage. That provides some motivation for thinking that the recognition of  $C$  can't be necessary for the inference. And that is enough to show, without mentioning a regress, that there is an issue with explaining the relevance of accepting claims like  $C$  in a story about what guarantees inference.

It may be the case that Kripke (1974a/1974b) interprets Carroll's dialogue along these lines (i.e. in a way where focus is placed on the issue of how a claim like  $C$  would be of any help for an inference to be drawn). He criticizes an aspect of Quine's view that logic is empirically revisable, and he says that his argument is inspired by the Carroll dialogue. I'll look more closely at Kripke's criticism in chapter 3, but one of the main upshots is supposed to be that if an agent was unable to draw an inference by universal instantiation, then the acceptance of certain claims that would mandate the inference (e.g. "All universal claims imply each of their instances") could not help the agent to draw the inference. The reasoning is as follows. The agent already accepted some universal claim (e.g. All cows are four legged). If the agent wasn't able to infer by UI in the first place, then why would it help to accept another universal claim (in this case "All universal claims imply each of their instances")? Kripke thinks that if accepting this claim were to be of any help, then the agent would have to use it as a premise in an inference by UI. But we already know that the agent was unable to infer anything from the first universal statement. So it's not clear why the second universal statement would be of any help.

I'll come back to Kripke's criticism of Quine, but it is straightforward to see how someone might read the moral of Carroll's dialogue in this way. The tortoise does not infer  $Z$  from  $A$  and  $B$  at the first level. So why would the tortoise be helped to infer  $Z$  merely by the addition of another premise  $C$ ? It might be supposed that the only way  $C$  could help is if it is used as a premise in a modus ponens inference. But if the tortoise fails to draw the inference at the first stage, it's not clear why he would be able to draw a modus ponens inference at the second stage. This being said, it is worth noting that the first stage in Carroll's dialogue is not actually an instance of modus ponens. It may involve modus ponens in some way, but it seems to involve quantification (although this may be an unnecessary feature of Carroll's set up.)

cannot be mandatory for an agent to accept that their premises support their conclusion. Another idea would be to say that an agent *does* have to accept a claim like  $C$ , but that their acceptance has to be understood in some attenuated way. For example, rather than understanding the acceptance of  $C$  as a belief state, it may be understood in terms of an intuition, or some kind of non-doxastic awareness.<sup>14</sup> Another idea would be to say that it cannot be mandatory that a claim like  $C$  be accepted *as a further premise* in order for an inference like that from  $A$  and  $B$  to  $Z$  to be drawn.<sup>15</sup>

Whatever the ultimate moral may be on this interpretation, there will still be many important questions about inference that are unresolved. For example, if the tortoise needn't recognize a claim like  $C$  in order to draw an inference from  $A$  and  $B$  to  $Z$ , then what would make the tortoise count as inferring as opposed to merely accepting  $Z$  as a result of an associative transition? If the tortoise needs to accept  $C$  in some attenuated way, how should this attenuated acceptance be understood? And if the tortoise needs to accept  $C$  but not *as a premise*, what role is the acceptance playing in the generation of an inference? I will not try to resolve any of these issues, but I will turn to another reading of Carroll's dialogue where the logical "force" is given an epistemic interpretation.

## 1.4 An interpretation of logical force where it is understood as a kind of Epistemic Necessity

On another way of interpreting Carroll's dialogue, expressions like "force" and "logical necessity" will concern features that make an inference epistemically justified.<sup>16</sup> This provides a different way

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<sup>14</sup>See Bonjour (1998/2014) and Fumerton (2015). Their views are given as a response to the question about what would make an inference epistemically justified, but elements from their views could be incorporated into a view where non-doxastic states are seen as necessary for an inference to occur. This is noted in Rosa (2017). Also, as mentioned previously, Boghossian (2014) discusses a theory of inference where notions of rule-following might be cited to give a non-doxastic account of what it is for an agent to take their premises to support their conclusion.

<sup>15</sup>See Valaris (2014) for a view like this where acceptance of a claim like  $C$  is constituted by the agent's inference (rather than something that is part of the causal conditions for the inference to occur). See Dogramaci (2016) and Rosa (2016) for a response. See Valaris (2016) for a reply to Dogramaci.

<sup>16</sup>As mentioned before, it may be more appropriate to talk in terms of epistemic obligation, but I'll continue to talk in terms of epistemic justification in the main text. There are questions about how to understand the notion of epistemic justification. How is it to be distinguished from practical justification? Does it involve reliability? Will epistemically justified inference require an agent to antecedently see that the premises support or give reason for inferring the conclusion? Related to the idea of the tortoise needing to recognizing that they have a reason to infer  $Z$ , there is an informative dialogue from Simon Blackburn (written in honor of the 100th anniversary of Carroll's dialogue). The tortoise in Blackburn's dialogue finds a lack of motivation to infer  $Z$  even in spite of the fact that

of thinking about the tortoise's challenge. Here, the tortoise is understood as asking for conditions that would make him epistemically justified in inferring  $Z$  from  $A$  and  $B$ .<sup>17</sup> On this interpretation, it may be that a person can be justified in drawing an inference even though they do not in fact draw the inference. So perhaps the tortoise can be justified in inferring  $Z$  from  $A$  and  $B$  without in fact inferring  $Z$  from  $A$  and  $B$ .

But on this reading, what are we to make of the tortoise's view regarding the necessary conditions for an inference from  $A$  and  $B$  to  $Z$  to be epistemically justified? We can gain some insight into this by considering what the tortoise and Achilles say about readers of Euclid. Recall that they consider two kinds of readers of Euclid. The second kind that they consider accepts  $A$  and  $B$ , but doesn't accept the hypothetical  $C$ .

“ And might there not *also* be some reader who would say ‘ I accept  $A$  and  $B$  as true, but I *don't* accept the Hypothetical’ ? ”

“ Certainly there might. *He*, also, had better take to football.” (Carroll 1895: 692)

Regarding this kind of reader (as well as the first kind who accepts the hypothetical but doesn't accept  $A$  and  $B$ ), they say there would be no logical necessity to accept  $Z$ .

“ And *neither* of these readers,” the Tortoise continued, “is *as yet* under any logical necessity to accept  $Z$  as true ? ”

“ Quite so,” Achilles assented. (Carroll 1895: 692)

But then Achilles thinks that all he needs to do is to get the tortoise to accept  $C$ . The claim  $C$  expresses a conditional support relation between the first two claims ( $A$  and  $B$ ) and  $Z$ . Some philosophers have noted that  $C$  looks like an “internalist” requirement about epistemic justification.

For example, Paul Boghossian (2001) sees the Carroll dialogue as something that can be read as suggesting a powerful argument against an internalist epistemology of justified inference.<sup>18</sup> He

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Achilles gets him to accept that it would be rational to infer  $Z$  and that the tortoise ought to do what he thinks is rational.

<sup>17</sup>There is another issue about whether the property of justification applies to mental states or propositions. Likewise, on this interpretation, you can ask whether the property of being justified applies to inferences themselves or sequences of premises and conclusions themselves, but I'm not sure whether much hangs on this in the discussion at hand.

<sup>18</sup>See also Patrice Philie (2007) and Fumerton (2015) for responses to the idea that Carrollian regress issues present a difficulty for internalist theories.

cites William Alston (1986) as an example of a philosopher who considers the idea that justified inference does require that the agent have an appreciation of a support relation between the initial premises and the conclusion.

If “logical necessity” is construed in terms of epistemic justification, then this is not an unnatural way of thinking about a requirement that Achilles seems to assume. Achilles thinks he needs to get the tortoise to accept  $C$ , and  $C$  does express a conditional support relation between the first two claims ( $A$  and  $B$ ) and  $Z$ . And we can easily imagine Achilles thinking something like, “gosh, if I can just get the tortoise to appreciate that the premises  $A$  and  $B$  logically imply  $Z$ , then surely they will see that they are justified in accepting  $Z$ . Here is Boghossian’s characterization of Alston:

William Alston considers, without fully endorsing, a version of the following account (I have modified it in small ways).  $S$ ’s belief that  $p$  *confers* warrant on his belief that  $q$  just in case:

- (A)  $S$  is justified in believing the premises,  $p$ .
- (B)  $p$  and  $q$  are logically related in such a way that if  $p$  is true, that is a good reason for supposing that  $q$  is at least likely to be true.
- (C)  $S$  knows, or is justified in believing that the logical relation between  $p$  and  $q$  is a specified in (B).
- (D)  $S$  infers  $q$  from  $p$  because of his belief specified in (C). (Boghossian 2001: 24)

The main point is that Boghossian sees Carroll’s dialogue as including a moral against a theory like this. In particular, he sees the dialogue as including a moral against the condition “(C)” in the Alston passage above. He says

A second, more severe, problem is suggested by Lewis Carroll’s observations in his note “What the Tortoise Said to Achilles.” There are a number of ways of reading that famous argument, of course, and it is not clear which, if any of them, Carroll actually had in mind. But on one suggestive reading, its moral is precisely that condition (C) is too strong, if there is to be any such thing as transfer of warrant by argument. (Boghossian 2001: 25)

Boghossian notes that other philosophers have taken this to be the moral of Carroll’s dialogue as well (e.g. James Van Cleve (1984)), and he explains in detail why someone might take this to be the moral. Alston’s condition (C) stipulates that an agent must know (or be justified in believing)

that there is a logical support relation that is characterized in condition (B). Boghossian notes a couple ways of regimenting the idea that premises logically support their conclusion. He focuses on a regimentation labeled “(1)” in the following paragraph where he discusses the “propositional view” (i.e. the view that Alston considers).

According to the propositional picture, one can only be justified in inferring a given conclusion from a given premise according to a given rule R, if one knows that R has a particular logical property, say that it is truth-preserving.

So, for example, no one simply reasoning from the particular proposition p and the particular proposition ‘if p, then q’ to the proposition q could ever be justified in drawing the conclusion q; in addition, the thinker would have to know that his premises necessitate his conclusion. Let us suppose that the thinker does know this, whether this be through some act of rational insight or otherwise. How should we represent this knowledge? We could try:

Necessarily :  $p \rightarrow ((p \rightarrow q) \rightarrow q)$  (1) (Boghossian 2001: 25)

So how could knowledge of a claim like (1) have any epistemic relevance? As Boghossian describes the idea behind the propositional view, knowledge of a claim like (1) is supposed to be significant because it allows an agent to recognize that the premises support the conclusion.

However the knowledge in question is represented, how does it help justify the thinker in drawing the conclusion q from the premises with which he began?

The answer might seem quite simple. Consider (1). Doesn't knowledge of (1) allow him to appreciate that the proposition that q follows logically from the premises, and so that the inference to q is truth-preserving and so justified? (Boghossian 2001: 26)

A key point is that Boghossian does not think that knowledge of (1) will allow the agent to appreciate these things *immediately*. He thinks that appreciation of these things will only happen as a result of (1) being used *as a premise in an inference*. On top of this, the needed inference will be of the same type as the inference that was originally in need of justification. He notes this in his response to the question that occurs in the previous passage.

In a sense, the answer is obviously ‘Yes’, knowledge of (1) does enable an appreciation of just that fact. But it doesn't do so automatically, but only via a transition, a transition, moreover, that is of a piece with the very sort of transition it is attempting to justify.

1.  $p \rightarrow ((p \rightarrow q) \rightarrow q)$
2. p

3.  $(p \rightarrow q) \rightarrow q$
4.  $p \rightarrow q$
5. Therefore,  $q$

As is transparent, any such reasoning would itself involve at least one step in accord with modus ponens. (Boghossian 2001: 26)

Notice the last comment in the above passage. It means that in order to justifiably infer 5 from 2 and 4, you would first have to accept 1, and infer 3 from 1 and 2. But according to Alston's picture, in order for this inference from 1 and 2 to 3 to be justified, condition (C) would have to be met. In other words, the agent would have to have a further justified belief that 1 and 2 support 3. But how could this justified belief be relevant in the story of how the agent infers 3 from 1 and 2? The justified belief will have relevance *as a premise* in another inference; so the regress moves along. Every justified inference needs a justified belief (as mandated by condition (C)). But every one of these justified beliefs will be relevant only on the grounds that they are used as premises in further justified inferences (which require more justified beliefs, and so on). Boghossian sums this up in the following passage.

If, therefore, we insist that the original inference from  $p$  and  $p \rightarrow q$  to  $q$  was unjustified unless supported by the propositional knowledge represented either by (1) or by (2), then we commit ourselves to launching an unstoppable regress. Bringing any such knowledge to bear on the justifiability of the inference would itself require justified use of the very same sort of inference whose justifiability the general knowledge was supposed to secure.

What this Lewis Carroll-inspired argument shows, it seems to me, is that at some point it must be possible to use a rule in reasoning in order to arrive at a *justified* conclusion, without this use needing to be supported by some knowledge about the rule that one is relying on. It must be possible simply to *move* between thoughts in a way that generates justified belief, without this movement being grounded in the thinker's justified belief about the rule used in the reasoning.

Condition (C), we are agreed then, must go. (Boghossian 2001: 26)<sup>19</sup>

In this passage, Boghossian makes it clear what he thinks the moral of the story is supposed to be. (C) cannot be a necessary condition on a theory of justified inference.<sup>20</sup> This would give an

<sup>19</sup>It's worth noting that there is a question about whether the transition from (1) and (2) to (3) would be (in some sense) of a piece with the transition it is trying to justify. Perhaps here both transitions are modus ponens inferences, but as noted before, in the Carroll dialogue, there is some quantificational structure that is relevant in the transition from  $A$  and  $B$  to  $Z$ .

<sup>20</sup>In more recent work, Boghossian treats a condition that is similar to Alston's (C) as a necessary requirement for inference to take place (set aside the further question about what makes an inference epistemically justified). As noted before, he opts for a non-doxastic construal of what he calls the "taking" condition.

analysis of what went wrong in the dialogue. The tortoise treated  $C$  as something further that was needed, but if Boghossian is right, it cannot be correct in general to think that justified deductive inference requires belief in a claim like  $C$  that concerns a support relation between premises and a conclusion.

There are other ways to respond that are analogous to the options discussed in the previous section. For example, someone might say that a claim like  $C$  *does* need to be accepted for the inference to be justified, but that the acceptance of  $C$  cannot be understood in terms of a belief.<sup>21</sup> Alternatively, someone might say there is no problem with treating a belief in (something like)  $C$  as a requirement for justified inference *as long as*  $C$  is not treated as a further premise.<sup>22</sup> However the moral of the regress in Carroll is understood, all of these responses provide an example of how the points in Carroll's dialogue can be understood in epistemic terms. This is perhaps the most common way of thinking about the meaning of terms like "force" in the dialogue.<sup>23</sup>

## Concluding Segment

I'm not sure whether Carroll intended one of these specific readings (causal vs. epistemic) with his use of phrases like "*must*", "logical necessity", and "force". As far as I can tell, he could have had both of these notions in mind or some other notion that I have not considered. Perhaps the text doesn't determine an answer for these questions, and for all I know, Carroll may not have found it important to sharply distinguish between these meanings in his enjoyable dialogue. Nevertheless, the expressions he uses (e.g. "force" and "*must*") can be given causal or epistemic interpretations.

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<sup>21</sup>See Bonjour (1998/2014) and Fumerton (2015)

<sup>22</sup>See Rhees (1951) for a view that may fit into this category, and the previous footnote which discusses some issues for Rhees's view.

<sup>23</sup>There are other places where someone might look for an epistemic regress as well. Even if the tortoise did need to believe a claim like  $C$  in order to be justified in inferring  $Z$ , there is another question about how  $C$  could be justifiably believed in the first place. The tortoise grants  $C$  in the context of the dialogue, but if we ultimately want a story about how  $C$  can be justified in a way that isn't problematically circular and isn't based on some kind of foundational or non-inferential assumptions, then a regress will ensue. For example, if a claim like  $C$  can't ultimately be based on something like testimony, intuition, or conceptual competence, then inference may be the only leftover way for  $C$  to be justified. But if  $C$  is inferred from some other premises, how are those further premises justifiably acquired? More inferences? At every stage you can ask how the further premises are justifiably acquired, and unless you appeal to some kind non-inferential acceptance (based in intuition or otherwise), there will be a regress of inference. Wright (2001, 2014) describes regresses that have a structure like this.

In spite of the fact that I have some reservations about arguing for a definitive interpretation of the dialogue, I can say some things about readings of the regress that place a focus on the question of how a claim like  $C$  is supposed to be applied. Both Kripke's argument (see footnote 13 above) and Boghossian's (2001) argument place an emphasis on how a claim like  $C$  would be applied, given that one accepts it. Kripke looks at how acceptance of claims like  $C$  would be relevant for explaining how an inference is drawn in the first place, and Boghossian (2001) looks at how acceptance of claims like  $C$  would be relevant in explanations of how an inference is justified. They both discuss a role that claims like  $C$  would purportedly play when they are used as a premise. But as far as I can tell, there is nothing in the Carroll dialogue itself that discusses how the tortoise would actually use  $C$ . The tortoise doesn't raise any issue about how  $C$  is supposed to be applied. Upon accepting  $C$ , the tortoise simply goes on to say that if the hypothetical  $D$  isn't accepted, nothing will logically force acceptance of  $Z$  (even when  $C$  is accepted).

## Chapter 2

# Quine's Regress Argument against Logical Conventionalism

One of Willard Van Orman Quine's main goals in "Truth by Convention" is to argue against the idea that logic is grounded in linguistic convention. He explicitly focuses on the idea that *logical truth* is based in linguistic convention, and he argues that it cannot solely be a matter of the conventions of language that logical truths are true. Quine's criticism bifurcates into two separate points depending on the notion of convention that is employed in the logical conventionalist thesis. He focuses on one version of the logical conventionalist thesis where conventions are understood as *explicit*, and another version of the logical conventionalist thesis where conventions are understood as *implicit*. The central idea in Quine's criticism of explicit conventionalism about logic is that it would result in a vicious infinite regress, and his argument is supposed to be based on the regress in Lewis Carroll's dialogue.

In the following, I will look at Quine's argument against logical conventionalism in detail, and I'll also discuss some related interpretive and philosophical issues. In section 2.1, I'll simply describe the view that Quine aims to criticize with his regress argument. So I will describe how Quine understands the idea of linguistic conventions and how he understands the thesis that the truth of a claim (logical truth or otherwise) might be based in *explicit* linguistic conventions. I'll assume that Quine intended at least some of the arguments in "Truth by Convention" to be a

criticism of ideas held by Rudolph Carnap.<sup>1</sup> There is some thesis that Carnap holds (in various incarnations) that can be described as a view according to which logic is a matter of convention, but it is not my aim here to address the exegetical point about whether or not Carnap really did hold a view of the form that Quine was criticizing. So I will not be giving an exegesis of what Carnap thought, but rather an exegesis of the view that Quine took Carnap to believe. That being said, I will look at some passages from Carnap so that they might be used as a guide in the exegesis of Quine. Once these details are set into place, I'll reconstruct Quine's regress argument in section 2.2.

In section 2.3, I'll discuss an interpretive issue regarding whether or not Quine's regress argument should be understood in epistemic terms. I'll look at an epistemic interpretation of Quine's argument in section 2.3.1 (which includes an unnumbered subsection concerning the evidence for this interpretation). In section 2.3.2, I'll discuss a non-epistemic interpretation of Quine's argument.

In section 2.4, I'll discuss Quine's criticism of implicit conventionalism about logic. Quine did not see his regress argument as applicable to this form of conventionalism about logic, and he rejected it on independent grounds. His main reason for rejecting implicit logical conventionalism was that the notion of implicit conventions would be explanatorily useless. So I'll look at the details of these point from Quine regarding explanatory usefulness.

In section 2.5, I'll look at a version of implicit conventionalism about logic from Jared Warren. Warren defends a view where implicit conventions for logic can be understood in terms of inferential rules (and where inferential rules are understood by analogy to syntactic rules that are posited by linguists in explanations of linguistic phenomena). In section 2.6, I'll look at a response to Quine's critique of implicit conventionalism from Warren, and in section 2.7 I'll provide a few arguments for thinking that an implicit conventionalism about logic can't be sustained.

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<sup>1</sup>Quine cites Carnap's *Logical Syntax of Language* in "Truth by Convention". The German edition of *Logical Syntax of Language* was originally published in 1934, and the English edition was originally published in 1937. I'll be citing a reprint of the English translation when discussing Carnap's text, but I won't be citing any of the passages that are additions to what was published in the original German. It's worth noting that it is arguable whether Quine's criticism was intended to be an attack on any view held by Carnap. I won't focus on this issue, but see Creath (1990) and Ebbs (2011) for a defense of the view that Quine's argument was not so intended.

## 2.1 Quine's Understanding of the View that Logical Truths are True by Convention

There are two main points to be taken up regarding Quine's understanding of the thesis that logical truths are true by convention. The first point concerns the nature of the conventions that Quine had in mind, and the second point concerns how it is that something might be true in virtue of conventions (whatever they turn out to be). While the present aim is not to give an exegesis of Carnap's actual view, I will cite some passages from Carnap throughout the following discussion so that they might act as a guide for understanding the view that Quine is focused on.

When Quine talks about conventions, he is talking about linguistic conventions. More specifically, he has in mind something that concerns the meaning of linguistic expressions. So when Quine discusses the thesis that logical truths might be true by convention, he has in mind a view where logical truths are true somehow wholly on the basis of the meaning of linguistic expressions.

When Quine talks about the conception of linguistic meaning that he has in mind, he isn't talking about associative connections or the connotation that a word might have. By the meaning of a word, Quine is specifically talking about something that concerns the conditions under which the word is truly applied. He discusses the notion of meaning as one where an expression's meaning is somehow determined on the basis of the contexts in which the expression truly or falsely applies.

[I]n point of meaning, however, as distinct from connotation, a word may be said to be determined to whatever extent the truth or falsehood of its contexts is determined (Quine 1936: 260)

When Quine discusses this conception of linguistic meaning, he may have in mind passages from *Logical Syntax of Language* that indicate a view where the meaning of logical connectives can be determined by a (somehow arbitrary) choice of postulates or rules.

[L]et any postulates and any 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 (Carnap 1934/37: xv)<sup>2</sup>

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<sup>2</sup>Carnap talks about meaning in terms of rules of inference here, but he also discusses meaning in terms of truth tables. I'll say more about how Carnap understands the relationship between rules and truth tables below.

Quine also describes the view he is interested in as one where an expression's truth in a context can be determined by "fiat" (see the second passage cited from p. 60 below for Quine's exact wording). When Quine describes the view in this way, he may have in mind Carnap's idea that there is an element of free choice involved in the selection of logical postulates. But there is another important point about the notion of convention that Quine has in mind. When Quine talks about logical truths being true by convention, he is interested in the thesis that those conventions are somehow given explicitly. This may be in line with passages in Carnap where inference rules are discussed as things that can be laid down.

We also shall lay down rules of inference (that is to say, the definition of 'directly derivable') and set up primitive sentences for our object-languages. (Carnap 1934/37: 29)

However this idea might be understood in Carnap, for Quine, the explicitness of a convention can be understood in terms of stipulative definitions. A stipulative definition can be formulated by a person explicitly declaring the situations in which an expression truly or falsely applies. In particular, Quine discusses the idea that we might determine a meaning for a new expression by specifying the contexts in which the expression is true (or satisfied). He says

But the alternative is open to us, on introducing a new word, of determining its meaning absolutely to whatever extent we like by specifying contexts which are to be true (Quine 1936: 260)

So, according to the view that Quine has in mind, someone can generate a meaning for an expression by simply stating the conditions in which the expression will count as truly applying. And it is true that new words can be introduced into a language in just this way. This may be in line with Carnap's talk of postulation, but it's worth noting that Carnap emphasizes the idea of inference (or "transformation") rules as determining meaning. This can be seen in the following passage where Carnap discusses the use of truth tables as a preliminary way of explaining meaning in the case of logical connectives (or "junction symbols").

The one-termed or two termed junction symbols are used to construct a new sentence out of one or two sentences respectively. In a strictly formally constructed system, the meaning of these symbols—as will be discussed more fully later—arises out of the rules of transformation. In order to facilitate the understanding of them, we shall provisionally

explain their meaning (and similarly that of other symbols) by less exact methods; first, by an approximate translation into words of the English language, and secondly, with more precision, by means of the so-called truth-value tables. (Carnap 1934/37: 18)

Given Quine's understanding of the notion of convention, we can now look at how he understands what it would mean for something to be true in virtue of convention. In an explicitly given convention, a meaning is provided for a linguistic expression. Given the notion of meaning that Quine has in mind, this amounts to an act where someone stipulates the circumstances in which the expression truly applies. So, truth would be based in explicit convention in the sense that someone can stipulate the circumstances in which an expression truly applies.<sup>3</sup> A sentence would count as true wholly on the basis of convention to the extent that it is stipulated to hold in every situation.<sup>4</sup> In regards to the idea that we might stipulate the circumstances in which an expression truly applies, Quine characterizes it as an act whereby things become true by "fiat" or "linguistic convention".

Since all contexts of our new word are meaningless to begin with, neither true nor false, we are free to run through the list of such contexts and pick out as true such ones as we like; those selected become true by fiat, by linguistic convention. (Quine 1936: 260)

After introducing the idea of truth by fiat, Quine considers the possibility that it might be applied to the case of logical truth. This is the view that Quine aims to criticize. But he notes that logical connectives are not new words. They are words that already have an established meaning or pre-existing usage. So he asks us to imagine a scenario where primitive logical expressions (e.g. "not", "or") are washed of any antecedent meaning, and where we give them meanings by stipulating the situations in which they are supposed to truly apply.

Now suppose in particular that we abstract from existing usage of the locutions 'if-then', 'not', (or '∼'), and the rest of our logical primitives, so that for the time being these become meaningless marks, and the erstwhile statements containing them lose their status as statements and become likewise meaningless, neither true nor false; and

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<sup>3</sup>Someone may worry that the truth would need to be a *consequence* of the stipulation. If so, they may be anticipating some of the worries that Quine ultimately raises for the view that logical truths are a matter of convention. If the truth (which is supposedly based in convention) somehow presupposes the notion of consequence, it doesn't look like logical truth is being explained in terms of convention.

<sup>4</sup>The classic criticism of this idea is well known. For an example, consider an instance of the law of excluded middle "The Eiffel tower is on earth or not". Whatever the conventions are for "or" and "not", it is natural to think of this sentence as being true partly on the grounds that the Eiffel tower is on earth (so that it is not wholly a matter of linguistic convention). For examples of this style of argument in the literature, see Boghossian (1996), Burge (2003), and Sider (2003). For a useful analysis of this criticism see Warren (2014).

suppose we run through all those erstwhile statements, or as many of them as we like, segregating various of them arbitrarily as true. To whatever extent we carry this process, we to that extent determine meaning for the initially meaningless marks 'if', 'then', ' $\sim$ ', and the rest. Such contexts as we render true are true by convention. (Quine 1936: 260)

The passages where Quine discusses the idea that logical truth depends on convention can be compared with passages in Carnap where he discusses the relationship between meaning and logic. (Although as noted before, Carnap (1934/37) places focus on transformation rules rather than statements of truth conditions or truth tables.)

We shall see that the logical characteristics of sentences (for instance, whether a sentence is analytic, synthetic, or contradictory; whether it is an existential sentence or not; and so on) and the logical relations between them (for instance, whether two sentences contradict one another or are compatible with one another; whether one is logically deducible from the other or not; and so on) are solely dependent upon the syntactical structure of the sentences. (Carnap 1934/37: 1-2)

[I]t depends entirely on the formal structure of the language and of the sentences involved, whether a certain sentence is analytic or not; or whether one sentence is deducible from another or not. (Carnap 1934/37: 6)

However Quine's conception of logical conventionalism relates to the ideas in Carnap, there is an important thing to note. If the logical truths are stipulated to have a truth value one at a time, it would require infinitely many stipulations. This observation is the starting point of Quine's regress argument.

## 2.2 Quine's Regress Argument against the View that Logical Truths are True by Convention

So how exactly does Quine's regress argument work? Quine notes that we can give only finitely many stipulative definitions. And given that there are infinitely many logical truths, this means that we cannot stipulate a truth value for each logical truth individually. Quine says this means, for the form of conventionalism about logic under consideration, that a finite number of stipulations will have to somehow determine truth values for an infinite number of logical truths.

[W]e must not be deceived by schematism. It would appear that we sit down to a list of expressions and check off as arbitrarily true all those which, under ordinary usage, are true statements involving only our logical primitives essentially; but this picture wanes when we reflect that the number of such statements is infinite. If the convention whereby those statements are singled out as true is to be formulated in finite terms, we must avail ourselves of conditions finite in length which determine infinite classes of expressions. (Quine 1936: 261)

Noting that the conventionalist view will have to make do with finitely many conventions, Quine gives a description of how someone might motivate the idea that an infinite class of truths could be based in a finite number of conventions. The idea is that someone might use general conventions. A general convention is supposed to cover an infinite class of statements, and Quine gives a few examples of what general conventions would look like.<sup>5</sup> Before giving the first example, he highlights the following schema.

(1) If if  $p$  then  $q$  then if if  $q$  then  $r$  then if  $p$  then  $r$  (Quine 1936: 262)<sup>6</sup>

The first general convention, which Quine names “(I)”, makes reference to this schema and states that every instance of the schema is true. A key idea is that this convention is supposed to secure infinitely many truths since there are infinitely many instances of (1). Quine describes how this is supposed to work:

One infinite part of our program of assigning truth to all expressions which, under ordinary usage, are true statements involving only our logical primitives essentially, is thus accomplished by the following convention:

(I) Let all results of putting a statement for ‘ $p$ ’, a statement for ‘ $q$ ’, and a statement for ‘ $r$ ’ in [(1)] be true. (Quine 1936: 262)

Granting that this were successful, it would still only secure the truth of a fragment of the logical truths that we are interested in. So Quine provides two more general conventions that would purportedly be needed. They are listed as follows:

(II) Let any expression be true which yields a truth when put for ‘ $q$ ’ in the result of putting a truth for ‘ $p$ ’ in ‘If  $p$  then  $q$ .’ (Quine 1936: 262)

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<sup>5</sup>Quine is following Lukasiewicz (1929/63) here.

<sup>6</sup>In the original text, Quine does not label the schema with a “(1)”. In the following, I’ll use the expression “(1)” surrounded by brackets to clarify that this is my own labeling.

(III) Let all results of putting a statement for ' $p$ ' and a statement for ' $q$ ', in 'If  $p$  then if  $\sim p$  then  $q$ ' or 'If if  $\sim p$  then  $p$  then  $p$ ,' be true. (Quine 1936: 263)

The grand idea is that the truth of all propositional logical truths can be based in these three conventions (or some enlarged but still finite set of conventions).

Quine says this idea would be illustrated by providing derivations of logical truths from the conventions. He gives an example of how a particular logical truth could be derived when the conventions (I)-(III) are taken as premises. It will be worth quoting an extended passage where Quine explains how a derivation like this is supposed to work. His example involves the logical truth "If time is money then time is money". Part of the interest in showing a derivation for this instance of the principle of identity is that it is an "if-then" statement (without any negations) that cannot be inferred on the basis of convention (I) and (II) alone.

[T]here are truths of that description which though lacking the not-idiom, are reached by (I)-(III) and not by (I) and (II). This is true, e.g., of any instance of the principle of identity, say

(2) 'If time is money then time is money.'

It will be instructive to derive (2) from (I)-(III), as an illustration of the general manner in which truths are generated by those conventions. (III), to begin with, directs that we adopt these statements as true:

(3) 'If time is money then if time is not money then time is money.'

(4) 'If if time is not money then time is money then time is money.'

(I) directs that we adopt this as true:

(5) 'If if time is money then if time is not money then time is money then if if time is not money then time is money then time is money then if time is money then time is money.'

(II) tells us that, in view of the truth of (5) and (3), this is true:

(6) 'If if if time is not money then time is money then time is money then if time is money then time is money.'

Finally (II) tells us that, in view of the truth of (6) and (4), (2) is true. (Quine 1936: 264)

In the above derivation of the logical truth "if time is money, then time is money", the

conventions are treated as initial premises. According to the conventionalist view of logic that Quine is considering, a similar derivation can be given for every logical truth. The idea is that if this were right, it would illustrate how all logical truth could be a matter of convention.

But Quine thinks this won't work. He makes a comment on derivations of logical truths from conventions like (I)-(III). He says that any such derivation requires "logical inference" and therefore results in an infinite regress.

In the adoption of the very conventions (I)-(III) etc. whereby logic itself is set up, however, a difficulty remains to be faced. Each of these conventions is general, announcing the truth of every one of an infinity of statements conforming to a certain description; derivation of the truth of any specific statement from the general convention thus requires a logical inference, and this involves us in an infinite regress. (Quine 1936: 270)

Clearly Quine is correct that a derivation from the conventions (I)-(III) to a particular logical truth is going to involve logical inference. To explain how this leads to an infinite regress, Quine examines a section of the previous derivation in more detail. He says that the derivation of (6) from (3) and (5) happens "on the authority of (II)". In his characterization of the derivation, he does not describe it as a single step inference where (3) and (5) are the sole premises. He says the derivation will involve (II), and the ascription of truth to (3) and (5).

[I]n deriving (6) from (3) and (5) on the authority of (II) we *infer*, from the general announcement (II) and the specific premiss that (3) and (5) are true statements, the conclusion that

(7) (6) is to be true. (Quine 1936: 270)

It may seem as though Quine is characterizing the transition from (3) and (5) to (6) as involving much more detail than one would ordinarily assume is needed.<sup>7</sup> Perhaps the idea is that if explicit conventions like (II) are going to play a role, then they must do so explicitly as premises. That would help to explain why Quine treats the ascription of truth to (3) and (5) as something that is needed. Given that convention (II) is playing a role in the derivation as a premise, and given that convention (II) makes use of the truth predicate, the application of (II) would require us to represent not just (3) and (5), but also the ascription of truth to (3) and (5).<sup>8</sup> Regardless

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<sup>7</sup>For example, why should so many premises be needed? And if the conclusion is (7) (i.e. the ascription of truth to (6)), won't Quine's characterization of the derivation require a further inference to get to (6)? Why can't the conventionalist just infer (6) from (3) and (5) without any intermediate steps or extra premises?

<sup>8</sup>This explanation can be questioned. Even if we grant that (II) is being treated as a premise, perhaps it is

of whether these intermediate steps are necessary, and regardless of Quine's reasons for thinking they are needed, it is important to note them because they are part of Quine's characterization of the derivation and they play a role in Quine's story about how the regress is supposed to be generated.

When Quine explains how the regress emerges, he says it will be simpler to look at a slightly reconfigured version of convention (II). He restructures convention (II) so as to make it a universally quantified statement that explicitly binds three variables for statements that occur in a conditional. He names the reconfigured convention (II').

An examination of this inference will reveal the regress. For present purposes it will be simpler to rewrite (II) thus:

(II') No matter what  $x$  may be, no matter what  $y$  may be, no matter what  $z$  may be, if  $x$  and  $z$  are true [statements] and  $z$  is the result of putting  $x$  for ' $p$ ' and  $y$  for ' $q$ ' in ' $p$  then  $q$ ' then  $y$  is to be true. (Quine 1936: 271)

Quine describes the derivation in a way where it explicitly concerns the structure of (5). In particular, it is supposed to be relevant that "(5) is the result of putting (3) for ' $p$ ' and (6) for ' $q$ ' in ' $p$  then  $q$ '."

We may also grant it as known that (5) is the result of putting (3) for ' $p$ ' and (6) for ' $q$ ' in ' $p$  then  $q$ ' (Quine 1936: 271)

There is a reason why Quine thinks this claim about the structure of (5) is relevant in the derivation. It has something to do with the fact that (II') is a premise in the derivation. (II') concerns not only truth ascriptions, but also structural facts about truth ascriptions. So in Quine's characterization of the derivation, this claim about the structure of (5) will be the right conjunct of a premise labeled "(8)". (8) bundles together the claim about the structure of (5) with the ascription of truth to (3) and (5):

(8) (3) and (5) are true and (5) is the result of putting (3) for ' $p$ ' and (6) for ' $q$ ' in ' $p$  then  $q$ '. (Quine 1936: 271)

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possible to transition from (II), (3), and (5) to (6) without any intermediate steps. A mathematical inference need not correspond to a long-winded derivation in the *Principia Mathematica*. Whether or not this extra detail is required, it's worth emphasizing that there is a question about what constitutes a derivation of the requisite sort needed for illustrating the view that logic is true by convention.

Quine notes that from (II') and (8), you can infer (7). But he says that this inference will involve the use of (II').

From these two premisses we propose to infer (7). This inference is obviously sound logic; as logic, however, it involves use of (II') and others of the conventions from which logic is supposed to spring. (Quine 1936: 271)

Quine thinks that the derivation of (7) from (II') and (8) will require intermediate steps. To see why Quine thinks the use of (II') is involved, it will be helpful to point out the fact that (8) is the antecedent of a conditional that results from three applications of universal instantiation on (II'). In discussing the derivation of (7) from (8) and (II'), Quine mentions a convention "(IV)" for removing wide scope universal quantifiers and uniformly replacing previously bound variables with "designators".

[S]uppose that (IV) entitles us in general to drop the prefix 'No matter what  $x$  [or  $y$ , etc.] may be' and simultaneously to introduce a concrete designation instead of ' $x$ ' [or ' $y$ ', etc.] in the sequel. (Quine 1936: 271)

This convention would allow us to remove expressions like "no matter what  $x$ ,  $y$ ,  $z$ , etc. may be" that occur in convention (II') and then uniformly replace the variables with the designators "(3)", "(5)", and "(6)". In particular, Quine says that by applying (IV) three times, we can infer the following statement (9) from (II').

(9) If (3) and (5) are true and (5) is the result of putting (3) for ' $p$ ' and (6) for ' $q$ ' in 'If  $p$  then  $q$ ' then (6) is to be true. (Quine 1936: 271)

This provides us with one sense in which (II') is used. It is used as a premise in an instance of reasoning that happens on the authority of convention (IV). But what we ultimately arrive at with (8) and (9) are premises that would set someone up for a modus ponens inference to (7). And Quine says that this is a place where (II') is needed. He says:

But this is an inference of the kind for which (II') is needed (Quine 1936: 271)

We can give the following explanation for why (II') is supposed to be needed. The move from (8) and (9) to (7) is of exactly the same form as the move from (3) and (5) to (6). If the conventionalist about logic needs a convention like (II') to play a role in the transition from (3) and (5) to (6),

then presumably they will also need convention (II') to play a role in the transition from (8) and (9) to (7). To put it contrapositively, if we don't need (II') to play a role in the move from (8) and (9) to (7), then we shouldn't have needed (II') to play a role in the move from (3) and (5) to (6) in the first place. But given the way that Quine understands the conventionalist picture of logic, it is part of the conventionalist view that (II') does play a role, as a premise, in the derivation from (3) and (5) to (6).

At this point, there is a natural way of seeing how an infinite regress is generated. If (II') is going to play a role in the derivation of (7) from (8) and (9) in the same way that it was supposed to play a role in the derivation from (3) and (5) to (6), then truth will have to be ascribed to (8) and (9). Moreover, (9) will need to be represented as the result of putting (8) for ' $p$ ' and (7) for ' $q$ ' in 'If  $p$  then  $q$ '. Quine explicitly bundles these claims into the claim (10), and says "we are to infer (7)" from (10) "with help of (II')". He says

[F]rom the fact that

(10) (8) and (9) are true and (9) is the result of putting (8) for ' $p$ ' and (7) for ' $q$ ' in 'If  $p$  then  $q$ '.

we are to infer (7) with help of (II'). (Quine 1936: 271)

How is (II') supposed to help here? Exactly as it was supposed to help before. This means that we would apply convention (IV) to convention (II'), but this time replacing the variables with designators "(8)", "(9)", and "(7)". This results in a conditional that has (10) as an antecedent. In other words, we have arrived once again at a place that is a set up for a modus ponens inference. We are in a position that is exactly analogous to the one we were in before. And Quine points out that the move from (10) and (II') to (7) is analogous to the "original task" of getting from (8) and (II') to (6).

But the task of getting (7) from (10) and (II') is exactly analogous to our original task of getting (6) from (8) and (II'); the regress is thus under way. (Quine 1936: 271)

This is how the regress is generated. The idea is that each modus ponens inference that happens on the authority of convention (II') is going to put you in a position that requires another modus ponens inference that happens on the authority of (II'). Since the position you end up in

is structurally identical to the one in which you started, the reasoning can be repeated so on to infinite.

## 2.3 The Nature of Quine's Regress Argument

There is more than one way to understand the nature of the regress that Quine aims to generate for the conventionalist view of logic. But however the regress is ultimately understood, Quine did think of his regress argument in terms of Carroll's dialogue. He cites Carroll's dialogue in a footnote appended to the previous passage where he says "the regress is under way". And later, in "Carnap and Logical Truth", he says

For it is impossible in principle, even in an ideal state, to get even the most elementary part of logic exclusively by the explicit application of conventions stated in advance. The difficulty is the vicious regress, familiar from Lewis Carroll, which I have elaborated elsewhere. Briefly the point is that the logical truths, being infinite in number, must be given by general conventions rather than singly; and logic is needed then to begin with, in the meta-theory, in order to apply the general conventions to individual cases. (Quine 1960a: 357)<sup>9</sup>

But even granting that Quine is right to think of his argument as analogous to Carroll's dialogue, it's already been noted that there are multiple avenues for interpreting the significance of the regress in Carroll. So, below, I'll look at two different interpretations of the regress in Quine (one epistemic interpretation and one non-epistemic interpretation). After discussing these two interpretations, I'll make a comment about how the regress is best understood.

### 2.3.1 Is the regress epistemic?

Quine's talk of inferences happening on the "authority" of conventions could be interpreted in terms of epistemic justification.<sup>10</sup> Consider the initial step from (3) and (5) to (6). Quine says,

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<sup>9</sup>I've removed superscripts from the original passage indicating footnotes where Carroll and "Truth by Convention" are cited.

<sup>10</sup>As mentioned in the previous chapter (in section 1.4), there are various notions of justification (e.g. epistemic vs. practical). Even when a specific notion, like epistemic justification, is focused on, there are still various theories about what it is supposed to involve. I'll sometimes just use the word "justification" in this section, but I intend to be talking about a notion of epistemic justification, and I don't want to invoke a specific theory about what exactly that is supposed to amount to.

that for the conventionalist about logic, this has to happen on the authority of (II'). On one way of understanding Quine's use of the term "authority", he is interpreting the conventionalist about logic as making a claim about what is required for justified inference. I.e., in order for an inference from (3) and (5) to (6) to be justified, there has to be antecedent justification for accepting a general claim like (II').

If this were right, we could see Quine's regress argument as being directed towards this claim about what is required for justified inference. The aim of the regress argument would be to show that this requirement is entirely useless. At least if we follow Quine's characterization of the regress, (II') is only useful on the grounds that we can apply convention (IV) to (II') so as to derive (9).<sup>11</sup> (9) is a conditional claim, and we might grant its antecedent (8). But that would mean that we have ended up in a position which is structurally identical to the one that we started out with regarding the move from (3) and (5) to (6). So, if we needed (II') for the initial transition from (3) and (5) to (6) to be justified, we are also going to need (II') for the subsequent transition from (8) and (9) to (7) to be justified. But what role will (II') play in the transition from (8) and (9) to (7)? We would need to apply (IV) to it to derive a further conditional claim where (10) is the antecedent. This reasoning can be repeated infinitely, so to the extent that it is fair, it would show a pretty serious problem for the claim about what is required for justified inference. If it was right, a justified inference by modus ponens would require infinitely many antecedent justified applications of modus ponens. This wouldn't be acceptable.<sup>12</sup>

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<sup>11</sup>Someone might also try to find a regress that emerges in this transition from (II') to (9), but this isn't the regress that Quine focuses on. That is not to say that Quine thinks there is no difficulty for the conventionalist in regards to deriving (9) from (II') on the basis of (IV). He explicitly says that the transition from (II') and (IV) to (9) results in a similar difficulty.

Incidentally the derivation of (9) from (II') by (IV), granted just now for the sake of argument, would encounter a similar obstacle; so also the various unanalyzed steps in the derivation of (8). (Quine 1936: 271)

But this isn't the regress he focuses on, and he doesn't go into further detail about it.

<sup>12</sup>It's perhaps more natural to reconstruct the argument in terms of the idea that justified acceptance of (9) (rather than (II')) is being treated as a requirement for justified inference. Supposing we justifiably accept (9) (and its antecedent), the most natural idea about how this would be relevant is if (9) and its antecedent are used as premises in an inference. But since this is an inference by modus ponens, it should also require antecedent justification for a claim where (10) is the antecedent. But this reasoning can be repeated infinitely, so it would mean that we would need to accept infinitely many conditional claims in order to be justified in inferring (6) from (3) and (5).

### Is there any evidence for thinking that Quine thought of the regress in epistemic terms?

Aside from the possibility that “authority” might be read in an epistemic way, is there any reason for thinking that Quine understood the regress in epistemic terms? It’s clear that Quine’s essay is written with epistemic issues in mind. He explicitly discuss the relevance of the distinction between a priori and a posteriori claims.

Still, there is the apparent contrast between logico-mathematical truths and others that the former are *a priori*, the latter *a posteriori*; the former have “the character of an inward necessity”, in Kant’s phrase, the latter do not. (Quine 1936: 270)

Also, Quine’s paper is widely understood as an attack on the idea that the conventionalist program might explain the a priori in terms of analyticity.

This, in a nutshell, was one of the central arguments of Quine’s paper, “Truth by Convention,”...Although not fully appreciated right away, it eventually became a classic, and is now widely known for its powerful critique of the program of grounding a priori knowledge in knowledge of meaning. (Soames 2003: 265)

It’s also the case that Quine compares his regress argument to the regress that features in Lewis Carroll’s dialogue (see p. 271). As noted in the previous chapter, many have interpreted the regress in Carroll’s dialogue as having an epistemic upshot. But even if this is the case, I don’t know of any evidence that Quine understood Carroll’s dialogue as having an epistemic moral, and even if he did, it doesn’t follow that he saw this feature as applicable to his own regress argument against the conventionalist view of logic.

As for the fact that Quine explicitly discussed epistemological matters in “Truth by Convention”, is this evidence that Quine had something epistemic in mind with his regress argument? If so, it doesn’t seem to be a very strong piece of evidence. What it seems to show is that Quine is sensitive to epistemic issues that are related to the conventionalist program. He certainly did know that part of the promise of conventionalism was that it might be an alternative to rationalist theories of the a priori, but this doesn’t show that his regress argument is directed towards anything that specifically concerns epistemic justification.<sup>13</sup> He also must have been aware that Carnap was

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<sup>13</sup>And even if Quine’s regress argument isn’t understood in terms of epistemic justification, that doesn’t mean

prone to a kind of dismissive attitude about the idea that logic needs to be “justified”.

The fact that no attempts have been made to venture still further from the classical forms is perhaps due to the widely held opinion that any such deviations must be justified—that is, that the new language-form must be proved to be ‘correct’ and to constitute a faithful rendering of ‘the true logic’. (Carnap 1934/37: xiv)

For language, in its mathematical form, can be constructed according to the preferences of any one of the points of view represented; so that no question of justification arises at all, but only the question of the syntactical consequences to which one or other of the choices leads, including the question of non-contradiction. (Carnap 1934/37: xv)

Passages like these may provide some reason for thinking that Quine would be hesitant to place too much emphasis on notions of justification in his regress argument.<sup>14</sup>

There are also passages where Quine says that his point can be framed in terms of communication. He says

the difficulty which appears thus as a self-presupposition of doctrine can be framed as turning upon a self-presupposition of primitives. It is supposed that the *if*-idiom, the *not*-idiom, the *every*-idiom, and so on, mean nothing to us initially, and that we adopt the conventions (I)-(VII) by way of circumscribing their meaning; and the difficulty is that communication of (I)-(VII) themselves depends upon free use of those very idioms which we are attempting to circumscribe, and can succeed only if we are already conversant with the idioms. (Quine 1936: 271-272)

If the point can be framed in this way, then it cannot be a point about epistemic justification. Or, at the very least, it can't be a point that essentially concerns justified inference. So is there an alternative way to think about the nature of the regress?

### 2.3.2 Should the regress be understood in non-epistemic terms?

On an alternative interpretation, Quine's regress argument can be understood in a way where it is not focused on issues concerning epistemic justification. Recall that Quine develops his point in the context of talking about how the conventionalist program could be illustrated.

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that it wouldn't have epistemic consequences. For example, suppose the regress argument was understood in terms of some notion of explanation (and not epistemic justification). If the argument was successful, it might have the consequence of undermining an epistemological theory that was based on the logical conventionalist view.

<sup>14</sup>Although perhaps it would make more sense if some kind of language internal notion of justification were operative.

It will be instructive to derive (2) from (I)-(III), as an illustration of the general manner in which truths are generated by those conventions. (Quine 1936: 264)

Independently of any issues regarding epistemic justification, if the logical conventionalist program could only be illustrated by a person performing infinitely many inferences, then its plausibility would be compromised. And Quine does place an emphasis on worries concerning the idea that infinitely many tasks would need to be performed. For example, Quine's argument against logical conventionalism initiates with the observation that we cannot make infinitely many stipulations.<sup>15</sup>

The points about stipulation fall by the wayside as Quine goes into his regress argument, but his discussion of the regress still involves talk of tasks being completed. For example, recall the previously cited passage where Quine mentions the regress.

But the task of getting (7) from (10) and (II') is exactly analogous to our original task of getting (6) from (8) and (II'); the regress is thus under way. (Quine 1936: 271)

One way of understanding the main idea here is that each task will imply a further task. The tasks at issue are not acts of stipulation, but rather acts of inference. So there does seem to be evidence for thinking that a regress of inferential tasks is part of the issue. And there would also be a regress of premises. Each one of the inferences is authorized by the convention (II'). And given the way that Quine characterizes inferences that happen on the authority of (II'), each one these inferences will have a different instance of (II') as a premise.<sup>16</sup>

The key point is that this interpretation does not invoke notions of epistemic justification. If conventionalism about logic required an infinite number of inferences to be performed (or premises to be accepted), then that would make for a powerful argument against the view. At the very least, the view could not be illustrated for all practical intents and purposes. It is possible that Quine had in mind both epistemic and non-epistemic considerations. But even if that's true I don't see anything in the text that supports anything beyond the non-epistemic reading.

However the regress is understood, should we think Quine's argument is successful? I'm

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<sup>15</sup>At the very least, the meaning of logical terms cannot be based on an infinite number of stipulations because we have not in fact made an infinite number of stipulations. See Warren (2017) for this point.

<sup>16</sup>For example, the inference from (3) and (5) to (6) will have (9) as a premise.

somewhat sympathetic to the argument, but I will end this section with some reasons for thinking we should be skeptical. As mentioned previously, Quine's reasoning occurs in the context of a discussion of how the conventionalist theory might be illustrated via a derivation. But if the regress only occurs in Quine's derivation from conventions to a logical truth, then wouldn't Quine need to say more about why the conventionalist is forced into making all the assumptions that are built in to his characterization of the derivation?<sup>17</sup> For example, why couldn't the explicit conventionalist have an alternative theory of which claims need to be treated as premises?<sup>18</sup> Perhaps the explicit conventionalist could find room to resist certain assumptions that Quine makes about what is needed in a derivation. But instead of going into more detail about how these assumptions might be challenged, I will move forward to discuss the idea that conventionalism about logic might be reformulated on the basis of implicit conventions.

## 2.4 Quine's view regarding the idea that logic might be explained on the basis of implicit conventions

Even if Quine's argument is successful against the view that logical truths are true on the basis of explicit convention, there is an alternative form of conventionalism about logic that tries to explain logical truth on the basis of implicit convention. Quine describes an implicit convention view where the idea is supposed to be that conventions can be adopted through behavior without being formulated verbally. He says

It may be held that we can adopt conventions through behavior, without first announcing them in words; and that we can return and formulate our conventions verbally afterward, if we choose, when a full language is at our disposal. (Quine 1936: 272)

But Quine thinks his regress argument is not applicable to this version of conventionalism about logic. He says

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<sup>17</sup>One question worth asking is whether (and on what grounds) an explicit conventionalist could deny that (II') (or (9)) is needed as a premise in a derivation from (3) and (5) to (6). Also, if a claim like (II') or (9) is needed, it's worth asking whether it is needed in the same way that (3) and (5) are needed. As noted previously, Rhees attempts to dissolve the Carroll regress by pressing on distinctions like this.

<sup>18</sup>Or if the regress is read with an epistemic interpretation, why couldn't the explicit conventionalist have an alternative theory about what makes an inference justified?

So conceived, the conventions no longer involve us in vicious regress. Inference from general conventions is no longer demanded initially, but remains to the subsequent sophisticated stage where we frame general statements of the conventions and show how various specific conventional truths, used all along, fit into the general conventions as thus formulated. (Quine 1936: 272)

Many would argue that an implicit convention picture is much more plausible in the first place, but Quine provided some independent reasons for rejecting it. His main point in "Truth by Convention" is that implicit conventions are explanatorily useless. He says

In dropping the attributes of deliberateness and explicitness from the notion of linguistic convention we risk depriving the latter of any explanatory force and reducing it to an idle label. We may wonder what one adds to the bare statement that the truths of logic and mathematics are a priori, or to the still barer behavioristic statement that they are firmly accepted, when he characterizes them as true by convention in such a sense. (Quine 1936: 273)

In the passage, a challenge is also raised for how one might distinguish the view that logical truths are true as a matter of implicit convention from the view that logical truths are a priori or firmly accepted.

It's an interesting question whether Quine was right to think that the implicit conventionalist thesis could be dispensed with so easily. There is a nice critique of Quine's contention that implicit conventions are explanatorily vacuous in Warren (2017). Warren also develops a version of the view that logic is explained on the basis of implicit conventions, so I'll look at his criticism of Quine and raise some questions about whether Warren's implicit conventionalism about logic should be accepted.

## 2.5 Warren's Implicit Conventionalism about Logic

Warren's logical conventionalism is explicitly stated in terms of explanation. He says the conventions that explain the truth of logical truths are inferential rules that govern the use of logical connectives.

[L]ogical conventionalism is the idea that the rules governing the use of logical connectives fully explains the truth of logically true sentences in our language. (Warren 2017: 9)

As examples of inferential rules, Warren lists the following schemas:<sup>19</sup>

$$(CP) \frac{\begin{array}{c} \phi \\ \vdots \\ \psi \end{array}}{\phi \rightarrow \psi} \qquad (MP) \frac{\phi \quad (\phi \rightarrow \psi)}{\psi}$$

A key part of the view is that these rules can be followed implicitly. The rules are posited as explanatory of an agent's inferential behavior on analogy to the way that linguists posit grammatical rules to explain linguistic behavior. There are different ways to unpack the idea that a rule (grammatical or otherwise) is followed implicitly, and Warren understands this in terms of behavioral dispositions. Regarding linguistic rules, he says

[a] standard assumption among naturalists is that following linguistic rules like (*CP*) and (*MP*) is a matter of having certain complex patterns of linguistic dispositions. (Warren 2017: 10)

Warren has a specific view about which dispositions are relevant. He cites Field (2001) and gives a rough description of the view he has in mind.

Roughly and without attempting any analysis, when I say that an agent *S* follows rule *R* I mean not just that *S* is disposed to act in accordance with *R* in the vast majority of some class of independently specified standard situations, but also that *S* is disposed to accept corrections toward *R* quite readily; that we can expect *S*'s behavior to continue in this manner in a wide variety of hitherto unobserved situations; that there is some positive assessment by *S* of behavior in conformity with *R* and some negative assessment of behavior not in conformity with *R*, etc. (Warren 2017: 10)

On top of this, he says that there is indeterminacy in the rules that we actually follow.

In actual cases there will likely be more than a little indeterminacy concerning exactly which linguistic rules we are following, but this is to be expected and shouldn't seriously trouble us. (Warren 2017: 10)

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<sup>19</sup>These schemas need to be understood in a way where the relevant notion of convention does not have explicitness built in to it. The initial  $\phi$  in *CP* must also be understood as a supposition that is discharged. This is indicated in Warren's text via superscripts.

Given the details of the view, Warren explains how logical truth might be explained on the basis of implicit inferential conventions. He develops his explanation in terms of an inferentialist theory of meaning for the logical connectives. An inferentialist theory of meaning for logical connectives can be combined with the view that we implicitly follow inferential rules. He says the truth of an instance of the principle of identity can be explained by the “provability” of the instance using the inferential rules (which, for the inferentialist, also determine the meaning of logical connectives).

Against this inferentialist backdrop, one conventionalist option is to see the truth of logical truths like those of the form  $\lceil \phi \rightarrow \phi \rceil$  as *fully explained by* the provability of all sentences of this form using the above rules. So the truth of all instances of the schema  $\lceil \phi \rightarrow \phi \rceil$  is, for the conventionalist, fully explained by the provability of any instance of this schema using the inference rules of our language (Warren 2017: 9)

## 2.6 How an implicit conventionalist might respond to Quine's objections

With this view of implicit conventions in hand, Warren addresses Quine's explanatory emptiness objection in the following way. He argues that we have the same theoretical motivations for positing implicit inferential rules as we do for positing implicit grammatical rules in linguistics. One example of this in linguistics is based on the observation that small children will misconjugate certain verbs. It is well known that children say things like “I hurted my finger”, and they don't say this as a result of having heard the word “hurted”. This can be explained on the assumption that English speaking children are following a rule for forming a past tense morphology for verbs.

Warren uses an example from a study by Jean Berko Gleason ((1958) as recounted by Steve Pinker (1999)) where children were shown the following text along with illustrations of creatures that looked like a bird.

This is a wug.

Now there is another one.

There are two of them.

There are two ———

A large majority of small children responded by filling in the blank with the word “wugs”. How are children able to do this without having previously heard the word “wugs”? A natural explanation for this behavior can be given if we assume that children are following a rule for forming the plural of a noun. Warren also mentions linguistic phenomena that are not supposed to be explained on the basis of a speaker’s semantic competence. He highlights Chomsky’s (1957) famous colorless green ideas example in the following passage.

Empirical data suggests that syntactic judgments that a sequence of words is or isn’t grammatical aren’t piggybacking on some understanding of the meaning of the words. This is illustrated by Chomsky’s famous and shopworn nonsense sentence “colorless green ideas sleep furiously” — the sentence is meaningless but easily recognized as grammatical without any prompting. (Warren 2017: 17)

The point of all of this is that Warren thinks we have analogous reasons to posit the existence of inferential rules.

But the very reasons that linguists posit rules of morphology and grammar apply as well to rules of inference. Although this hasn’t — as far as I am aware — been noted in the literature, upon reflection this should be relatively unsurprising. It is, after all, very widely believed that English speakers follow natural language versions of the rule *modus ponens* (*MP*) when reasoning with “if”. (Warren 2017: 18-19)

Warren gives his own example to provide evidence that the case is analogous.

[B]ut it seems highly plausible that directly analogous reasoning would lead us to posit syntactic rules of inference. Consider the following argument:

1. Wugs eat computers
2. If wugs eat computers then lugs eat computers
3. *So*: lugs eat computers (1, 2)

Almost all English speakers, perhaps including fairly young children, would agree that this argument is valid, correct, or otherwise acceptable. Moreover, if we presented English speakers with (1) and (2) and asked them what, if anything, they could conclude from these two claims, it’s likely that a large majority of them would come up with something very much like or even identical to (3). (Warren 2017: 19)

So, in effect, Warren has a response to Quine's contention that implicit logical conventions cannot play an explanatory role.

## 2.7 Response to Warren

Even if Warren is right that Quine's explanatory emptiness objection can be avoided by analogizing implicit inferential rules to the case of implicit rules in syntax, this still won't vindicate the view that logic is true by convention. It is one thing to posit inferential rules to explain an agent's behavior, but quite another thing for the rules to explain the truth of logical truths.<sup>20</sup> In the following, I'll note a few reasons for thinking that implicit inferential conventions do not provide a better theoretical framework for making sense of the view that logical truths are true by convention. Two points I will only briefly mention (because I will come back to them in more detail in chapter 5).

One worry with Warren's view is that the analogy he draws between linguistic rules and inferential rules isn't clearly right. If the inferential rules are not supposed to operate on the basis of semantic competence, then a person's inferential behavior regarding the example with computer eating wugs should not be explained on the basis of semantic competence. But there's no reason for thinking that this example isn't explained partly on the basis of someone's grasp of the meaning of "if...then...". Warren says that such inferential behavior in the computer eating wug example could not be explained on the basis of semantic competence.

And since sentences (1) and (3) in the above argument aren't actually sentences of English, there is no way that the argument above could have been arrived at by performing semantic operations on its various premises and their components. (Warren 2017: 20)

This is not the right conclusion to draw however. What the example shows is that the person

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<sup>20</sup>Warren has something to say about the sense in which implicit rules are supposed to explain logical truth. As noted previously, part of the idea is that the truth of logical truths is supposed to be explained by the fact that they are provable given the rules. I suppose there are many questions about why provability via a rule could guarantee truth in general. Usually there is some restriction to acceptable rules (so as to exclude rules like Prior's "Tonk"), but some of the details might depend on Warren's understanding of inferentialism. He says "[t]he central idea behind inferentialism is that rules like these are *automatically valid* (where a rule is valid just in case it is necessarily truth preserving)." (2017: 9) I'm not sure how or why rules could count as automatically valid, but perhaps this has to be understood relative to the conventionalist backdrop where it is supposed to be wholly a matter of fiat what rules we accept ("Tonk" included). In any case, in the following, I don't aim to press on any issues that are raised by Tonk-like rules.

wouldn't be drawing the inference on the basis of imitating someone who transitioned from those specific premises to that specific conclusion. Warren reasons that the inference can't be based on semantic competence because (1) and (3) are not sentences of English. But that isn't a sufficient reason since a person's transition from premises to conclusion might be done partly on the basis of their grasp of the meaning of some of the component English words of the premises (namely "if" and "then"). So it doesn't seem analogous to the colorless green ideas case in Chomsky. In the colorless green ideas case, it's not obvious how grasp of semantic rules could explain the grammaticality judgments that people make.

There is another worry for the implicit convention version of logical conventionalism. The worry is based on the idea that there will always be a kind of indeterminacy regarding which implicit logical conventions we follow. For example, Warren describes implicit conventions as things that can be grounded in facts about an agent's inferential dispositions. But there are difficulties with the idea that an agent's finite dispositions could determine facts about which particular logical conventions they follow. This type of worry is raised in Kripke's *Wittgenstein* (1982) where he discusses dispositional theories of rule following. This is not to say that the ideas in Kripke's *Wittgenstein* are right, but as noted before, Warren (2017: 10), grants that there will be some indeterminacy regarding which linguistic rules we follow. If we grant that an agent's inferential dispositions underdetermine the facts about which logical conventions they follow, then this creates a further explanatory problem for the implicit conventionalist view of logical truth. Since an agent's finite inferential dispositions do not determine any particular logical conventions as the ones that the agent follows, then no particular distribution of truth values will be determined for what we might think of as the logical truths.<sup>21</sup> There will be a gap between the finite inferential dispositions of an agent and the infinitely many logical truths which we hope to establish a truth value for.

It is worth noting that Warren's view about the indeterminacy of rule-following may have evolved. In more recent work (forthcoming), Warren provides a disposition theorist response to the worries raised in Kripke's *Wittgenstein*. In chapter 5, I will look at Warren's more recent view in

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<sup>21</sup>For example, let's suppose that the ideas in Kripke's *Wittgenstein* are right, so that an agent's finite dispositions do not determine whether they are following an addition as opposed to a "quaddition" rule. Then, in the same way, an agent's inferential dispositions will not determine whether they are following a modus ponens rule as opposed to some quus-like counterpart of modus ponens. Then we would not be able to say that there is a classical distribution of truth values for logical truths, and the same will hold for non-classical distributions of truth values as well.

detail, and I'll argue that it leaves all of the original worries unresolved. To the extent that I am right about this, it leaves the implicit conventionalist version of the truth by convention thesis with an underdetermination problem. In the same way that our finitude precludes us from explicitly stipulating a truth value for the logical truths one at a time, the finitude of our dispositions will preclude them from being an adequate grounds for the truth value of the infinitely many logical truths in our language.

There is one more worry that I will briefly mention. In spite of Quine's claim that implicit conventions avoid a vicious regress worry, I don't think Quine is right on this point. Even if we granted that implicit inferential dispositions could determine facts about which inferential rules we follow, a vicious regress argument can still be developed for implicit convention versions of the view that logic is true by convention. I'll develop the argument for this claim in chapter 5.

## Concluding Segment

So we have seen that Quine's vicious regress argument can be analyzed in terms of its underlying assumptions and its intended conclusion. While there are open questions about how to understand the thesis he aims to criticize, he does clarify that he aims to criticize a view about the nature of logical truth. In particular he aims to criticize the view that logical truth is somehow a matter of linguistic convention. A major point worth noting is the issue about whether a vicious regress criticism can be applied to implicit conventionalism about logic. While Quine criticized both explicit and implicit conceptions of the truth by convention thesis in the case of logic, he took his regress argument to apply only in the case of explicit conventions. He had a separate argument against the implicit truth by convention thesis.

As we saw in the case with Carroll, there are also questions about whether Quine's regress argument should be understood in epistemic terms. I presented a few reasons for thinking that Quine's argument should not be understood in this way, and this point will be important later when discussing the relationship between Quine's argument and Kripke's argument. Quine's argument (if successful) may have had epistemic consequences. But even if this is true, it doesn't mean his criticism was primarily directed towards an epistemic thesis (or based on assumptions about

epistemic justification). His argument is directed towards the view that logical truth is somehow determined by linguistic conventions, and this view is not a thesis about epistemic justification. This same kind of issue will be important in part two when vicious regress arguments against logical pluralism are discussed. Some of the objections to vicious regress arguments against logical pluralism will only be applicable if the regress is based on epistemic considerations.

Lastly I looked at an attempt to respond to Quine's criticism of implicit logical conventionalism from Warren. I mentioned two main reasons for being skeptical that an approach like this could succeed. One point was that despite Quine's views to the contrary, implicit conventionalist views about logical truth still face vicious regress worries. I'll take this point up and defend it in section 5.2.<sup>22</sup> The other point was based on considerations of underdetermination. If dispositions underdetermine facts about which rules an agent follows, then we cannot appeal to dispositions to explain how truth values for logical truths or validity attributions are determined. I will return to this point in 5.3.

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<sup>22</sup>Chapter 5 focuses on Field's view, but some words of caution are necessary here. Field describes a view where the truth values of validity attributions may be (in some way) relative to policies. Field describes a view of policies that is similar to Warren's, but Field does not aim to defend a Carnapian thesis like Warren. In particular, Field does not subscribe to an inferentialist theory about the logical connectives (see Field (2009b)). The argument in 5.2 is directed towards Field's view, and it may not apply to a view that is framed under an inferentialist backdrop. That being said, I think the underdetermination puzzle described for Field's view in 5.3 can be applied to Warren's implicit conventionalism about logic (independently of any complications about inferentialist theories of meaning).

## Chapter 3

# Kripke's Regress Argument and Quine's Empiricism about Logic

Quine is known for holding a form of empiricism about logic that is most famously developed in his “Two Dogmas of Empiricism” (1951). Saul Kripke takes up this view from Quine and argues that it faces a Lewis Carroll style puzzle that is reminiscent of the one that Quine himself raised for Carnap’s conventionalist thesis about logic. In section 3.1, I’ll comment on some preliminary exegetical matters, and in section 3.2, I’ll give an exegesis of Quine’s empiricism about logic. I’ll discuss Kripke’s interpretation of Quine’s empiricism about logic in section 3.3, and I’ll explain the details of Kripke’s criticism of Quine in section 3.4. In section 3.5, I will discuss similarities and differences between Kripke’s argument, Quine’s argument, and the regress in Carroll (largely to argue that they exhibit significant similarities). This section will focus on many passages from Romina Padro (2015). Padro discusses the relationship between Kripke and Quine extensively. She comments on the aim and underlying assumptions of both arguments, and much of the discussion will be moved along by reference to Padro’s commentary. I will also have some critical things to say about Padro’s conception of how the arguments are related. The discussion of Padro is also relevant to a criticism of Kripke’s argument that I take up in section 3.6. In that section, I’ll argue that Kripke’s argument depends on a specific interpretation of Quine that isn’t clearly warranted. I’ll refer to arguments in support of this specific interpretation from Padro and I’ll argue that they

do not provide sufficient reason for thinking that Quine's view faces the difficulties attributed to it by Kripke.

### 3.1 Preliminary Exegetical Matters

As mentioned, Quine holds a form of empiricism about logic that Kripke criticizes. But the exegesis of Kripke is mired by the issue of his arguments only presently existing as unpublished manuscripts. There are many passages from these manuscripts that are cited in Padro's dissertation (2015). At the suggestion of the Saul Kripke Center, I will rely on Padro's dissertation as a source.

The passages from Kripke should be understood relative to some words of warning that Padro makes. It's worth quoting her at length on the matter. She says

Unfortunately, Kripke's material on the nature of logic is unpublished. I have had access to the tapes and a transcription of the first two lectures of his 1974 Princeton seminar (the first, as far as I know, comprehensive presentation of the material) and to a transcription of the lecture "The Question of Logic," given at Pittsburgh in 1974. I also attended a seminar at the Graduate Center in the fall of 2006 where some of the material was presented.

Writing about unpublished material is a tricky business for both the reader and the writer. The reader may wonder whether the presentation of the original views is accurate and fair. The writer must be careful not to step in too much without saying so, since the reader will have no way of comparing her account with the original views. I have tried to explicitly say which points are solely my own and to reconstruct Kripke's main arguments as accurately as I could. (Padro 2015: 21 fn. 32)

The same precautions must be taken here, and any attribution to Kripke has to be taken as highly qualified. The attributions to Kripke are relative to my own understanding of Kripke based on citations from these unpublished manuscripts.

### 3.2 Quine's Empiricism about Logic

It will be useful to focus on some passages from Quine in order to better understand his view (as well as Kripke's criticism of it). It will also be helpful to situate Quine's view historically in

terms of his rejection of Carnap's logical conventionalism. Breaking from Carnap, Quine does not reserve any special place for statements that have a status of being "true by convention". So, all statements will be on an equal footing for Quine, in that all of them are amenable, in some sense, to the vicissitudes of experience. There will be no statements that are true wholly as a matter of convention, and this includes statements of logical laws. This aspect of Quine's view is expressed in the following often quoted passage from "Two Dogmas of Empiricism".

It becomes folly to seek a boundary between synthetic statements which hold contingently on experience, and analytic statements, which hold come what may. Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle? (Quine 1951: 40)

In this passage, Quine claims that there is an option to revise hypotheses about logical laws in the face of recalcitrant experience. Since this idea will be central in understanding Kripke's criticism below, it will be worth elaborating on what's built into Quine's claim that there is an option to revise hypotheses about logic. In the passage, he says that any statement can be maintained as long as other requisite revisions are made. He also says that any statement can be revised, including statements of logical laws. Quine says there is no difference, in principle, between revising statements of logical laws and revising hypotheses in physics (as indicated by his comparison to cases of scientific revolution). While there is no philosophically significant difference between logical hypotheses and other statements on Quine's view, he does grant that there is a difference between logical hypotheses, physical hypotheses, and observation statements that can be spelled out in terms of a notion of proximity to experience. Statements of immediate observation make a kind of immediate contact with experience on Quine's view. More theoretical statements (e.g. Newton's laws) will make contact with experience in an indirect way due to a kind of logical relationship they bear to observation statements. This metaphor of proximity shows up in the following passage.

The totality of our so-called knowledge or beliefs, from the most casual matters of geog-

raphy and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth-values have to be redistributed over some of our statements. Re-evaluation of some statements entails re-evaluation of others, because of their logical interconnections - the logical laws being in turn simply certain further statements of the system, certain further elements of the field. (Quine 1951: 42-43.)

It is not stated directly in this passage, but logical statements are understood as being the furthest from the periphery on Quine's view.<sup>1</sup> An important element of the passage is the reference to "logical interconnections" towards the end. In the passage, Quine says that re-evaluation of some statements will entail (in some sense) re-evaluation of others. He says this holds because of "logical interconnections" between statements. Presumably Quine is talking about psychological connections that exist in the mind of an agent in passages like this.<sup>2</sup> And he goes on to describe logical laws as being "simply further statements of the system", and this is in keeping with his attitude about logical hypotheses not having any special status.

It will be worth emphasizing the fact that Quine talks about a relationship between recalcitrance and the acceptance (or rejection) of statements of logical laws. This can be seen in a subsection of one of the previously cited passages.

Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. (Quine 1951: 40)

Here Quine says that we can hold on to statements in the face of recalcitrant experience by revising statements of logical laws. Presumably the converse holds as well. By accepting

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<sup>1</sup>For a more detailed analysis of this aspect of Quine's view, see the Stanford Encyclopedia entry on Willard Van Orman Quine, especially section 3.2 and 4.2. There's some vagueness regarding what is supposed to count as a "logical statement". Presumably the statements of primary interest are highly general ones (like statements of laws of logic), but this can be confusing seeing as how Quine uses the phrase "logical truths" to describe statements that provide connections between the things we say about the world (see passage below from *Word and Object*). In passages like that, I'm not sure Quine's usage of expressions like "logical truth" are meant to describe claims like "I'm hungry or not". The passage occurs in the context of a discussion between "occasion" sentences (like "There was copper in it") and "eternal" sentences (like "Copper oxide is green"). Presumably, the logical statements of primary interest are more like the latter category and would therefore need to be more general than "I'm hungry or not".

<sup>2</sup>It's worth noting another possible interpretation where by "logical interconnections", Quine is talking about some kind of implication relations that hold independently of what goes on in an agent's mind. It may be that Quine thinks that there are such relations existing independently of what goes on in an agent's mind, but presumably he is talking about psychological connections in the passages cited here because he is explicitly discussing things that can be revised or changed or that are elements of an agent's psychological profile or evidential system.

certain statements of logic, the recalcitrance can re-emerge. There is a natural way of thinking about why Quine would assume the existence of a relationship between recalcitrance and statements of logic. Presumably this is because Quine thinks that statements of logic can provide connections between other statements. He describes a connection providing role for statements of logic in the following passage.

In an obvious way this structure of interconnected sentences is a single connected fabric including all sciences, and indeed everything we ever say about the world; for the logical truths, at least, and no doubt many more commonplace sentences too, are germane to all topics and thus provide connections. (Quine 1960b: 12-13.)<sup>3</sup>

This would explain Quine's view about the relationship between statements of logic and recalcitrance. If we think of statements of logic as providing connections between statements, then this helps to explain why amending statements of logic would have something to do with recalcitrance. The existence of recalcitrance will depend on the logical relationships between statements.<sup>4</sup> So if revising statements of logic has an impact on the logical relationships between statements, this can therefore have an impact on the presence of recalcitrance. But precisely how Quine understands this relationship between statements of logic and "logical connections" between statements will be a question of primary importance in Kripke's criticism. I will turn to that now.

### 3.3 Kripke's Interpretation of Quine's Empiricism about Logic

Kripke focuses on Quine's assumption that there is an option to revise hypotheses about logic in the face of recalcitrant experience.<sup>5</sup> This aspect of Quine's view was noted in the previous section in regards to the initial passage cited from "Two Dogmas of Empiricism", and Kripke has the following to say regarding Quine's view.

There is no sharp cleavage between logic and other domains of scientific inquiry or

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<sup>3</sup>While Quine uses the expression "logical truths" in this passage, I think his comment is applicable to highly general statements of logical laws. See the previous footnote on Quine's distinction between "occasion" and "eternal" sentences.

<sup>4</sup>As noted previously, I think Quine's talk of "logical interconnections" should be understood as referring to something psychological. The experience of recalcitrance will depend on whether there is a perceived inconsistency (even if there isn't an inconsistency in point of fact).

<sup>5</sup>He also focuses on Putnam (1968). Putnam argues that considerations from Quantum Mechanics might provide a case for the empirical revision of logical hypotheses.

what have you. They are all of a piece. Logic is just one of a variety of tools we use in organizing our experience. So far, perhaps, classical logic, which is the logic we have originally adopted, has well stood the test of experience. No doubt it should not be abandoned for trivial reasons. Maybe it has been well confirmed, since by using classical logic we have been able to make a large number of predictions which are well justified. We could, however, in principle and in response to recalcitrant experience, choose to revise our logic rather than to revise something else, like physics or geometry, and perhaps some day experience will lead us to do so. (Kripke 1974a: in Padro fn. 125)

In this passage, Kripke takes up Quine's idea that we might revise our logic in response to a recalcitrant experience. He describes Quine's view as one where classical logic may count as confirmed on the grounds that it has been used to make predictions that are successful (or in Kripke's words "justified"). Kripke also imagines how logic might play this prediction making role in a particular case.

He envisions a scenario where someone accepts the universal claim that all crows are black, and then has a recalcitrant experience where it appears as though there is a white crow in front of them. He imagines what it would take for the person to be able to eliminate recalcitrance by revising a logical hypothesis, in this case, the law of universal instantiation. Suppose we formulate the law of universal instantiation as "all universal claims imply each of their instances". On Kripke's view, if the revision of such a law were to eliminate recalcitrance, then the revision of that law would have to break a logical connection between the universal claim "all crows are black" and the relevant instance in this case. If this were right, then acceptance of such a law would be part of what generates the logical connection in the first place. Kripke puts this in terms of the language of "commitment" (and what a person is "able to conclude"). In those words, by accepting that "all crows are black", a person is not thereby committed to, or able to infer, that a particular crow is black. It is only when they accept universal instantiation that the person has the commitment and is able to draw the conclusion. Kripke's way of describing the scenario can be seen in the following passage.

Just because we believe all crows are black, that doesn't in itself commit us to believing that this crow is black. It's only if we believe that all crows are black plus universal instantiation, that we are committed to believing that this particular crow is black. All we have to do to reject this conclusion is to deny, or doubt, or at least hold in suspended judgment, the law of universal instantiation; and then it will be doubtful whether this

conclusion really follows, and we will certainly not be committed to it.

[...] if we just believe all crows are black, we are not ipso facto committed to concluding that this crow is black. We have a choice: to either go and deduce this or revise our logic so that this conclusion doesn't follow. This means that, in the absence of this particular statement in the system of interconnecting statements, one would not be able to conclude this crow is black. (Kripke: in Padro p. 108)<sup>6</sup>

This last passage indicates that on Kripke's interpretation of Quine, Quine is committed to the view that for someone who accepts "all crows are black", the acceptance of a logical hypothesis like UI, will in some way generate a commitment to accept that a particular crow is black. For someone who accepts that all crows are black, but doesn't accept UI, there will be no recalcitrance in the face of an apparently white crow. In the same way, acceptance of "all crows are black" won't be useful for generating a prediction (or drawing the conclusion) that the next observed crow will be black (unless UI is accepted). That Kripke also describes this connection in terms of prediction will be evident from passages cited in the following section.

### 3.4 Kripke's Puzzle for Quine's Empiricism about Logic

Given Kripke's interpretation, a puzzle is supposed to result for Quine's view.<sup>7</sup> If Kripke's interpretation is right, then Quine holds the view that the mere adoption of a belief in UI is sufficient to provide a kind of logical connection between statements. In the previously cited passages, Kripke characterizes the type of connection that is at issue in terms of what one is committed to and whether one has the ability to infer. He also characterizes the connections in terms of what one is able to predict. But Kripke argues that adopting UI would be useless for the purpose of establishing such a connection. He says

I want on the contrary to hold that, so regarded, the law of universal instantiation is completely useless. It has never led to a single prediction, or been of any use to us whatsoever, and so it cannot be said to be confirmed. Moreover, we cannot be thought

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<sup>6</sup>Notice in this passage that Kripke interprets Quine as holding the view that "[i]t's only if we believe that all crows are black plus universal instantiation, that we are committed to believing that this particular crow is black." Presumably this interpretation of Quine would need to be highly qualified because Quine could say that there are other routes through which a commitment could be generated. For example, the claim that a particular crow is black might be entailed by other claims as well (aside from the claim that "all crows are black").

<sup>7</sup>Kripke is not the only philosopher to develop a Carroll-style regress for Quine's empirical revision model. For example, see Priest (1978).

of as having adopted it. If we did adopt it, it would have done us absolutely no good.  
(Princeton Seminar)

[...]

Now what does this show? Does this show some sweeping conclusion, like “Logic is not revisable?” I certainly don’t want to lead to some big conclusion like that. But it does show that if you start talking about logic on the analogy with just anything else in science, that’s not so easy a thing to do. It has a very different character because, without being able to reason, we can’t say what are the consequences of a hypothesis, or what will happen if we adopt this rule or that. You have to reason in order to say what would be the case then. And there are certain rules which you just couldn’t adopt: you couldn’t tell them to yourself, because if you told them to yourself without already using them, they would be useless; so they either don’t help you or they were superfluous anyway. Universal instantiation has this character. If anyone tells it to you, it is superfluous – that is, superfluous for getting you to perform any particular inference. (Kripke: in Padro p. 112)

In this passage, Kripke clarifies that he does not aim to establish a highly general claim according to which logic cannot be empirically revised. He is merely arguing that this cannot happen along the lines described in his characterization of Quine.

When discussing the issue of empirical revision, it is helpful to put the point in terms of prediction and confirmation. But Kripke’s point can be put in terms of the language of commitment as well. This can be seen in the previous passage with the example regarding crows. Suppose accepting the statement “all crows are black” didn’t commit you to accepting that a particular crow was black. Why would the state of play be changed by your acceptance of a logical law like “all universal claims imply each of their instances”? If your acceptance of the statement “all crows are black” didn’t commit you to accepting that a particular crow was black, then why would your acceptance of the statement “all universal claims imply each of their instances” commit you to thinking that a particular universal claim implied each of its instances? The statement of the logical law is a universal claim, just like the statement “all crows are black”. So if the latter statement didn’t generate any commitment, it’s not clear why the former one would generate a commitment either.

In other words, if we follow Quine in thinking that there is no philosophically significant distinction between logical hypotheses and empirical hypotheses, then there won’t be a philosophically significant difference between the logical hypothesis “all universal claims imply each of their

instances”, and the empirical hypothesis “all crows are black”. To put the point in terms of reasoning, if you didn't already reason from the universal claim “all crows are black” to one of its particular instances, then adding “all universal claims imply each of their instances” to your belief set isn't going to help you draw the inference. If you didn't infer anything from the claim that “all crows are black”, then there's no reason to think you will infer anything from the claim that “all universal claims imply each of their instances” because they are both just universal claims.

There is another point in Kripke's discussion that is worth highlighting. It concerns the idea that a rule (somehow to be distinguished from a hypothesis) might change the main issue. For example, if accepting a hypothesis like UI doesn't help a person to infer in accordance with UI, then why couldn't we re-conceive of UI in a different way where it is understood as being more like an imperative? Kripke considers the following imperatival construal of UI.

From every universal statement, deduce each instance. (Kripke 1974a: in Padro p. 57)

Would accepting an imperatival construal of UI help a person to infer in accordance with UI? Kripke thinks the answer is no, and he gives an argument for this that is analogous to his previous reasoning. He says

... you may think, ‘Ah, the point of this is that we shouldn't adopt a statement ... we really mean to adopt a rule. But what does ‘a rule’ mean? If it is an imperative that I tell this guy, that will not help him either.

Suppose he agrees to obey. I, of course, will not regard him as having really obeyed if then he doesn't infer universal statements and their instances. But if we are adopting a so-called neutral standpoint from which it is supposed to be up for grabs what follows until we are told what rule to adopt, then you can quite well say, ‘All right, I will always infer each instance of a universal statement from the universal statement itself. Now, I don't see that that means, though, that I must infer this particular instance of this universal statement.’

Without begging any questions, I don't see that that follows either. After all, if the inference wasn't obvious to him in standard declarative logic it will not be obvious in imperative logic either. So the adoption of something that he will assent to – of this statement as a rule – will not help him either. (Kripke 1974: in Padro p. 60 fn. 72)<sup>8</sup>

Given what has been said up to this point, it should be clear that Kripke's point can be put in a way that doesn't overtly concern matters of infinite regress. Nonetheless, I think there is a natural

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<sup>8</sup>There is an interesting question about whether alternative non-imperatival conceptions of a rule might change the matter. I'll come back to this point in chapter 5.

way of seeing how the style of reasoning in Kripke is based on considerations that can be cited to construct an infinite regress argument. I'll reserve discussion of this point to a footnote.<sup>9</sup>

### 3.5 The relationship between Kripke's argument, the Carroll dialogue, and Quine's criticism of logical conventionalism

The central question regarding the relationship between Kripke's argument, the Carroll dialogue, and Quine's criticism of Carnap is whether or not they are all based on the same kind of underlying consideration. I think the most natural answer to this question is yes; they all seem to exhibit some kind of structural similarity. That being said, I think that a certain amount of caution is due when answering questions like this because they involve tricky details about what it would mean for two different regresses (or arguments) to count as being of the same kind. For the most part, I want to try to ignore some of these more fundamental theoretical issues, and instead proceed by looking at

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<sup>9</sup>It's a bit awkward to try to develop a regress in terms of Kripke's example with UI. One difference between Kripke's example with UI and Carroll's example with claim  $C$  concerns the degree of generality between UI and  $C$ . UI is a highly general claim that concerns every universal statement. But claim  $C$  is highly specific. It only concerns the specific premises  $A$  and  $B$  and the specific conclusion  $Z$ . Given the set up in Carroll, there is a straightforward way for the tortoise to construct a series of hypothetical claims with antecedents that continually increase their level of complexity ( $D$ ,  $E$ , etc.). In the case of UI, there isn't a further claim that would be analogous to  $D$  in Carroll's dialogue. But this may be incidental to the set up. Kripke is talking about general logical hypotheses like UI because he is discussing Quine (and the idea that general logical hypotheses might be empirically confirmed on the Quinean model). But Kripke's point can be applied to specific logical hypotheses as well.

For example, consider the specific conditional claim ( $\gamma$ ): "If (UI), then "All crows are black" implies that this crow in front of me is black". We can imagine someone who accepts "All crows are black" and then encounters an apparently non-black crow. In Kripke's discussion, recalcitrance is described as something that can be eliminated (on the Quinean model) by rejecting UI. But we can also discuss the possibility of recalcitrance being eliminated by the rejection of  $\gamma$ . If the person didn't accept  $\gamma$ , they would be very much like the tortoise prior to the tortoise's acceptance of  $C$ . So in the same way that Achilles thinks that logical force will emerge when the tortoise accepts  $C$ , we can consider whether recalcitrance will emerge when the person accepts  $\gamma$ . Would it?  $\gamma$  sets the agent up for an inference by *modus ponens*.

(UI) Every universal claim implies each one of its instances

( $\gamma$ ) If (UI), then "All crows are black" implies that this crow in front of me is black

( $\zeta$ ) "All crows are black" implies that this crow in front of me is black

But then, just as the tortoise can reject  $D$ , the imagined person can reject  $\delta$ .

( $\delta$ ) If (UI) and ( $\gamma$ ), then ( $\zeta$ )

Would recalcitrance emerge when the person accepts  $\delta$ ? It's hard to see why it would if recalcitrance wasn't already present given the person's acceptance of  $\gamma$ .  $\delta$  sets the person up for an MP inference, but the person was already set up with an MP inference with their acceptance of  $\gamma$ . In other words, if we needed a claim like  $\delta$  at the first stage, then we have all the same reasons for thinking we are going to need a further claim like ( $\epsilon$ ).

( $\epsilon$ ) If (UI) and ( $\gamma$ ) and ( $\delta$ ), then ( $\zeta$ ).

Since this reasoning can be repeated infinitely, an infinite regress is generated. We might consider whether a general statement of MP changes the matter, but that would only give us something like convention (II) from Quine's discussion. Even after accepting (II), we can imagine someone rejecting the claim "If (II), then (UI) and ( $\gamma$ ) imply ( $\zeta$ )". So it looks like a regress can be generated from many different angles in a way that is based on Kripke's style of reasoning. Moreover, these regresses can be paralleled to the Carroll regress in a straightforward way.

some different senses in which the regresses can be compared and contrasted (largely to argue that they are indeed of the same kind).

In section 3.5.1, I'll look at how Kripke conceives of the relationship between his argument, Quine's argument, and the considerations in Carroll. In some way, he thinks that his and Quine's argument are based on the same kind of point (and that this point can be found in Carroll's dialogue). If Kripke's understanding is right, it provides for a nice way of bringing these three trains of thought together; they are all based the same underlying consideration. But even if this is right, it still leaves open other questions about how they are (or aren't) of a piece.

Padro argues that there are important senses in which Kripke's and Quine's argument are not the same, and much of the following material will be guided by what she has to say. After discussing Kripke's conception of the relationship in more detail, I'll look at three specific senses in which the considerations in Carroll, Quine, and Kripke can be compared. In section 3.5.2, I'll look at the question of whether the considerations are supposed to apply to any logical principle (or only a restricted set). In section 3.5.3, I'll focus on the question of whether the considerations involve a certain feature that is clearly present in the case of Kripke's argument. In Kripke's development of the problem, part of the idea is that a belief in UI won't be useful for a person who doesn't already infer in accordance with UI. More generally, the puzzle involves the idea that belief in a logical principle won't be useful for someone if they don't already infer in accordance with that very same principle. So it can be asked whether this sort of feature is present in the case of Carroll and Quine as well. Lastly, in section 3.5.4, I'll look at differences that concern the aim of the consideration (or the point that the authors were trying to make).

### 3.5.1 Kripke's conception of the relationship

It was already noted in the previous chapter that Quine describes his criticism of Carnap in terms of the regress in Carroll. Kripke sees his own argument as similar to the point in Carroll as well, although he expresses some uncertainty about how to understand the moral of Carroll's dialogue.

it's hard to say in fact, in the Lewis Carroll case, what kind of moral he thought he was

drawing because he just sets up a puzzle. (Kripke 1974a: in Padro p. 66)

Kripke characterizes his argument as being (in some sense) the same as the one that Quine raised for Carnap. This is evident from passages where he characterizes Quine as not seeing that the points in "Truth by Convention" can be applied to his own empiricism about logic.

...one couldn't think of the laws of logic as based on a set of assumptions, whatever else one thought of them as being. [...] I don't know how far this kind of consideration goes in the history of philosophy [...] but it is in the article by Lewis Carroll, who was taken over in one article by Quine but seems to be ignored by him in others. (Kripke 1974a: in Padro p. 66)

This is noted more explicitly in the following.

...the Carnapian tradition about logic maintained that one can adopt any kind of laws for the logical connectives that one pleases. This is a principle of tolerance, only some kind of scientific utility should make you prefer one to the other, but one is completely free to choose. Of course, a choice of a different logic is a choice of a different language form.

Now, here we already have the notion of adopting a logic, which is what I directed my remarks against last time. As I said, I don't think you can adopt a logic. Quine also criticizes this point of view and for the very same reason I did. He said, as against Carnap and this kind of view, that one can't adopt a logic because if one tries and sets up the conventions for how one is going to operate, one needs already to use logic to deduce any consequences from the conventions, even to understand what these alleged conventions mean.

This is all very familiar as a criticism of Carnap. Somehow people haven't realized how deep this kind of issue cuts. It seems to me, as I said last time, obviously to go just as strongly against Quine's own statements that logical laws are just hypotheses within the system which we accept just like any other laws, because then, too, how is one going to deduce anything from them? I cannot for the life of me, see how he criticizes this earlier view and then presents an alternative which seems to me to be subject to exactly the same difficulty. (Kripke 1974a: in Padro p. 113)

So is Kripke right to think that his and Quine's argument are based on the same underlying consideration? We can start out by asking whether their arguments are only applicable to a restricted set of principles (e.g. to UI but not disjunctive syllogism).

### 3.5.2 Do the regresses only apply to a restricted set of logical principles?

Padro argues that Quine's argument can be distinguished from Kripke's argument on the grounds that Quine's argument is supposed to apply to any rule of logic. She refers to Kripke's puzzle as the "adoption problem".

Several differences can be found between the adoption problem and Quine's argument. The latter, for example, makes a general point; namely, that every convention attempting to set up a logical principle, whatever the principle may be, will be confronted with the same problem: the need to use logic to extract consequences from the convention. Whether the principle is a basic one or not is irrelevant, since the point will apply to every logical principle. (Padro 2015: 113-114)

She says this does not hold in the case of Kripke.

the adoption problem is only supposed to affect some basic logical principles or rules, like UI or MPP. (Padro 2015: 115)

Padro does not say precisely which principles Kripke's argument can be applied to, but MPP, UI, adjunction, and non-contradiction are supposed to be on the list.<sup>10</sup> But is it right to say that Quine's argument can be distinguished on account of it having a fully general applicability? Quine does indicate that there is a problem for most all of the conventions that he considers. Regarding conventions (I)-(III) (that were mentioned in section 2.2), he says

In the adoption of the very conventions (I)-(III) etc. whereby logic itself is set up, however, a difficulty remains to be faced. Each of these conventions is general, announcing the truth of every one of an infinity of statements conforming to a certain description; derivation of the truth of any specific statement from the general convention thus

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<sup>10</sup>She says

Kripke mentions two other principles as being affected by the adoption problem, adjunction and non-contradiction (the list, however, wasn't intended to be exhaustive). According to adjunction if 'A' is true and 'B' is true, then 'A and B' is true. The problem is that if we accept that 'A' is true and accept that 'B' is true, then adjunction is needed to conclude that one has accepted that "A is true and B is true." In the case of non-contradiction, if we try to get someone who doesn't reason in accordance with it to adopt it, the problem is that the subject could hold the law of non-contradiction itself to be both true and false (Padro 2015: 36 fn. 52)

And she goes on to say

it would be interesting to see which other principles are directly affected by this argument. That task, however, falls outside the scope of what I set out to do here. In any case, I think that the argument would be interesting even if it only worked with MPP and UI. (Padro 2015: 37 fn. 52)

requires a logical inference, and this involves us in an infinite regress. (Quine 1936: 270)

He also notes that a problem similar to the one he explicitly outlines can be formulated for the convention (IV) (the UI convention) and other steps that he doesn't explicitly analyze.

Incidentally the derivation of (9) from (II') by (IV), granted just now for the sake of argument, would encounter a similar obstacle; so also the various unanalyzed steps in the derivation of (8). (Quine (1936: 271)

If we assume that the same holds for every convention that Quine discusses, would this show a difference between his argument and Kripke's? To the extent that there is any difference here, I think it may be superficial. The passages in Quine indicate a certain kind of generality, but this can be understood in terms of a generality regarding the range of principles that can be cited in the beginning of a regress. Even if we grant this, it may still be that in every case, the regress is generated on the basis of properties concerning a restricted set of principles (like UI and MP).

For example, principle (I) isn't a statement of UI or MP. But if a regress is initiated through a derivation where (I) is taken as a premise, the regress may still in some way be based on a feature of UI or MP. Why think this? One reason why this might be the case is that principle (I) is a universal claim. So any derivation where (I) is cited as a premise would have to happen (in Quine's terminology) "on the authority" of convention (IV). In that case, we'll need to reason by UI in order to apply principle (I). This should be enough to see that it's not obvious that Quine's argument can be distinguished in terms of considerations of generality, but I'll reserve more detailed discussion of this point to a footnote.<sup>11</sup>

What this shows is that Quine's argument might have a kind of generality, and still nonetheless

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<sup>11</sup>Quine's main example concerns convention (II) (which is a modus ponens convention). But an argument in the same style of the one that Quine explicitly gives for convention (II) can be given for convention (I) as well. In order to apply (I) you would need to assume that some statement  $\alpha$  is the result of putting a statement for ' $p$ ', a statement for ' $q$ ', and a statement for ' $r$ ' in the schema labeled (1) (see section 2.2 in the previous chapter). Then from that, and convention (I), you would need to infer that  $\alpha$  is true. But since convention (I) is a universal statement, that very transition would have to happen on the authority of (IV) (which for sake of convenience I will re-formulate as "all universal claims imply each of their instances"). Following the style of reasoning in Quine, if (IV) is to be relevant (as a premise), you would need to recognize that (I) is a universal claim, and that the ascription of truth to  $\alpha$  is an instance of (I). That would set us up for a new inference from (IV), the claim that (I) is a universal claim, and the claim that the ascription of truth to  $\alpha$  is an instance of (I) to the conclusion that  $\alpha$  is true. But this would place us in a situation that is not significantly different from the one that we were originally in when we wanted to derive something from (IV), (I), and the claim that  $\alpha$  is the result of putting a statement for ' $p$ ', a statement for ' $q$ ', and a statement for ' $r$ ' in the schema labeled (1). So in developing the regress for convention (I) the details can be seen as still nonetheless turning on features concerning universal instantiation.

be based on the same kind of consideration in Kripke's reasoning. In particular, a regress in the style of Quine might begin with a focus on a principle like (I) which isn't a MP or UI principle. But the regress may still be generated on the basis of features that concern UI. For this reason, I will assume that even if there is a kind of generality in the scope of Quine's argument, we may still see his and Kripke's arguments as based on the same kind of underlying consideration. Even if a Quinean regress can be developed for any of the conventions (I)-(VII), it may still be that the key points always involve universal instantiation and modus ponens (or some larger but still restricted set of principles).

### 3.5.3 Does each consideration turn on the idea that acceptance of a logical principle is only useful when inference in accordance with it is already presupposed?

It's actually somewhat difficult to talk about the feature of interest here, but a helpful way of understanding it is by looking at Kripke's example. Much of Kripke's discussion focuses on a case where someone isn't able to infer by UI. And a main point is that you can't enable such a person to draw a UI inference merely by getting them to form the belief that UI is valid (or that every universal claim implies each of its instances). So the example involves a feature where belief in a certain principle (in this case UI) doesn't help a person who doesn't already infer in accordance with that *same* principle (in this case UI).

Padro argues that there is also a distinction between Kripke's and Quine's argument on this account. She describes Quine's puzzle as one that doesn't turn on any point about the usefulness of a convention presupposing an inference that accords with that very same convention.

[...] Quine doesn't say that the logical inference needed is precisely the *same* one that the convention is trying to set up. He just says that "logic" or "a logical inference" will be needed. Of course, given the generality of the conventions, it is hard to see how UI and MPP inferences could be avoided. But the point remains that Quine was making a general claim: all logical conventions presuppose logical inferences for their application to individual cases, and the logical principle established by the convention and the logical inference required need not coincide (in fact, in most cases they will *not* coincide). (Padro 2015: 114)

She describes Kripke's puzzle as being different. She says

It arises when inferences that accord with a certain rule of inference or logical principle are needed to apply those very same principles or rules to individual cases. And it is for this reason that they cannot be 'adopted.' (Padro 2015: 115)

As I see the matter, this is not so obviously a point of distinction between Kripke and Quine's argument. For one thing, Quine's actual reasoning proceeds in a fashion where it is the same rule that is needed in order for the convention to be useful. For example, when Quine discusses convention (II'), the regress turns on the idea that a modus ponens inference would be needed in order for a modus ponens convention (i.e., (II')) to be helpful for the conventionalist in their attempt to illustrate how logical truths follow from the general conventions.

But the task of getting (7) from (10) and (II') is exactly analogous to our original task of getting (6) from (8) and (II'); the regress is thus under way. (Quine 1936: 271)

This is also the same passage where Quine cites Carroll, and it should probably be treated as evidence that Quine's argument does involve this presuppositional structure (which occurs when acceptance of a principle is only useful on the grounds that inference in accordance with that *same* principle is presupposed).<sup>12</sup>

### 3.5.4 How the points may differ in terms of their moral

While there are (in my opinion) some obvious structural similarities between Quine and Kripke's critiques, there are some clear differences in terms of what Quine and Kripke took to be the wider implications of their arguments. Padro argues that Quine and Kripke's arguments can be distinguished on the basis of points regarding their aim. In fact, she says the main distinction is that Quine's argument is aimed at Carnap's views about necessity and *a prioricity*, while Kripke's

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<sup>12</sup>This point would probably be reinforced by Quine's claim that a similar regress can be developed for convention (IV) (1936: 271). (IV) is a statement of UI and presumably the idea is that you would need to infer in accordance with UI for convention (IV) to be useful. The same point can be made in relation to convention (I). Given the way that Quine reasons, since (I) is a universal claim, convention (IV) will be needed when (I) is cited in a derivation. But then a regress can be developed where (IV) is shown to be useful only on the assumption that reasoning in accordance with (IV) is presupposed. This kind of structure seems to be present in the case of Carroll's regress as well. The claim *C* would only be useful if something like an MP inference can be made. Even though *C* is not a statement of MP, it can still be interpreted as a claim that is related to an *instance* of modus ponens that is associated with the transition from *A* and *B* to *Z* (even though, as noted, the transition from *A* and *B* to *Z* involves some quantificational structure).

argument is aimed at a view (supposedly in Quine) regarding the nature of (and relationship between) hypotheses about logical truths and inferential transitions.<sup>13</sup>

When Padro discusses Kripke, she distinguishes the conclusion of his argument from something that is supposed to be a corollary of it. The conclusion is supposed to be that a certain view about the relationship between hypotheses about logical truths and inferential transitions is wrong. The corollary of this is supposed to be that Quine's particular way of understanding the empirical revision of hypotheses about logic is wrong as well (see the passage cited in the footnote above).

Padro is right that Carnap's views on necessity and *a prioricity* were (in some sense) under scrutiny by Quine. And these views are not to be identified with the view that Kripke presumed Quine to hold (regarding the relationship between belief and inference). But while we can distinguish between a conclusion of Kripke's argument and something that is a corollary of it, we can also do this in the case of Quine. As noted in the previous chapter, a problem for Carnap's views of necessity and epistemology can be seen as a corollary of Quine's criticism of the conventionalist view about logic. If logical truth cannot be explained in terms of convention, then that might therefore result in a problem for theories of modality and epistemology that are based on the truth by convention thesis. Moreover, Quine's argument against the view that logic is matter of convention can be seen as a corollary of a problem concerning the relationship between statements of logical laws and inferential transitions. In other words, it is because you would need to presuppose inference in order for the statements of logical conventions to be useful that it is not possible to explain truth by convention. But this means that Kripke and Quine's arguments are only obviously distinguished in terms of the corollaries or the wider implications they saw as resulting from their reasoning. It does not show that there is any difference in the underlying considerations that their

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<sup>13</sup>She says

But the main difference between the adoption problem and Quine's argument is that the latter is intended to show that Carnap's attempt to offer an account of the necessity and *a prioricity* of all the logical principles by a conventional stipulation of their meaning fails, because inferential transitions not explained by means of conventions will be required to apply the conventions to particular cases. In contrast, when we look at Quine's views on logic in the light of the adoption problem we observe, *not* that the logical principles cannot be empirically confirmed or revised in the way Quine proposes (though this is a corollary), but rather that the status that he assigns to the logical truths in the web and his conception of the nature of the inferential transitions is untenable. (Padro 2015: 115)

arguments are based on.

Padro also argues that Quine's argument can be distinguished on the grounds that it is more like what she calls the "standard objection". The standard objection, according to Padro, is a point made in various ways by different philosophers, but which can be stated as a problem concerning the empirical justification of logic.<sup>14</sup> If this is right, it raises a question about why Kripke would have mistakenly identified his own argument with Quine's. Padro suggests that Kripke may have identified his argument with Quine's because of a failure to distinguish between what she calls the justification problem and the inference problem. The justification problem is supposed to concern epistemic matters (e.g. concerning our knowledge of logical laws), and the inference problem is supposed to concern the nature of deductive inference.<sup>15</sup> She says

Kripke doesn't distinguish between what we have called 'the justification problem' and the 'inference problem.' Perhaps for this reason he identifies Quine's argument with the adoption problem. (Padro 2015: 116 fn. 129)

If Quine's argument was properly understood in terms of epistemic justification and Kripke's argument was not articulated in epistemic terms, then this would make for a certain kind of difference. But even if Kripke's argument is not articulated in epistemic terms, that's still not a sufficient reason for thinking that it isn't similar (on that account) to Quine's argument. As noted in the previous chapter (in section 2.3), there is a non-epistemic interpretation of Quine's argument. In other words, it may not have been primarily directed towards an epistemological thesis (even if

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<sup>14</sup>She cites various authors as representatives of this critique, e.g., Katz (2000), Wright (1986), Boghossian (2000), Bonjour (2001), Sober (2000), and Shapiro (2000). Here is her description of it.

A common criticism of Quine's attempt to give an empirical justification of the logical truths is that inferential transitions will have to be presupposed in setting up the web and making it responsive to observations. Justification requires the confirmation of the system as a whole [...] Yet, if in setting the system up and making it responsive to observations, we have to rely on inferences that correspond to those very logical truths, the empirical justification of at least some of those truths will appear to be circular. (Padro 2015: 98)

She claims that Quine's argument against Carnap is more like the standard objection in the following passage.

Quine's Carroll-argument seems to be closer to what we have called the 'standard objection' than to the adoption problem. In both cases the problem appears to be that the justification of the truths of logic, whether by empirical or conventional means, will presuppose inferential transitions that correspond to at least some of those truths. (Padro 2015: 115)

<sup>15</sup>In making this distinction she is drawing on Wright (2004, 2014), but see Padro (2015: 8-17) for the specific details about how she understands the matter.

the successfulness of Quine's criticism would have had corollary epistemological consequences). As noted, there are certain considerations that might support a non-epistemic interpretation.<sup>16</sup> And given that a non-epistemic interpretation of Quine is a live option, we can still see the arguments in Quine and Kripke as being based on the same underlying considerations.

### 3.6 Critical Points

Kripke's puzzle is interesting, but it's based on an interpretation of Quine that isn't obviously correct. Kripke thinks Quine is assuming that a belief in UI is needed in order for a person to be committed to the instances of universal claims that they accept. For example, he says "[i]t's only if we believe that all crows are black plus universal instantiation, that we are committed to believing that this particular crow is black."<sup>17</sup> He also characterizes Quine's view in terms of an assumption about inference or prediction. He describes Quine as assuming that a belief in UI could help a person acquire an ability to infer or make a prediction in accordance with UI. For example, he says "I want on the contrary to hold that, so regarded, the law of universal instantiation is completely useless. It has never led to a single predication". Here, Quine is described as holding the opposite view where accepting universal instantiation is useful for generating a prediction. But what are the reasons for attributing this view to Quine? Part of the idea is based on passages in Quine where he talks about removing recalcitrance by amending statements of logical laws. Padro reasons in this

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<sup>16</sup>For example, Quine may have been hesitant to characterize his argument in terms of justification given some of Carnap's skepticism regarding the notion. Moreover, Quine thought his point could be framed in terms of communication, and so understood, the point isn't directly connected to matters concerning epistemic justification. It would of course need to be argued that Quine is right to think he is merely recasting the same argument in different terms. But it does seem to fit naturally with the way he frames the problem as being one that shows up for the conventionalist's attempt to illustrate their view. You couldn't convey the meaning of "all" to someone that didn't already understand it by saying "all universal claims imply each of their instances". They would have to already understand the logical particle "all", so in that way, stating the conventions couldn't illustrate the meaning of the logical particles. This is quite similar to Quine's regress point. In the same way that a statement of UI would be useless for someone that didn't already draw UI inferences, it would also be useless for explaining the meaning of "all" to someone that didn't already understand the meaning of "all". That being said, see Padro (2015: 114 fn. 128) for discussion of the matter regarding whether Quine's argument can be framed as a point about communication (where she also cites Boghossian (1996: 217, fn. 36) on the same issue).

<sup>17</sup>It's worth keeping in mind a point about this claim that was noted previously. As an interpretation of Quine, it would probably need to be qualified in some way, because you might be committed to believing that the particular crow is black because of some other claims that you accept. Related to this point is a more general question about whether Quine would want to say that it is a single statement of a law (like UI) that generates a commitment, or whether it is multiple statements that are needed. Quine's affinity for so-called holistic theories of meaning and confirmation may lend support for the latter view. In that case, accepting UI alone may not be enough (or the only way) to generate the requisite commitment.

way when describing Kripke's interpretation of Quine. She says

abandoning the statement of UI would have to block the inference from "All ravens are black" to "This raven is black." And Quine seems to think that this is easily done by re-evaluating "the statements of the logical connections themselves" – in this case, the statement of UI. But then it is only if we are committed to the UI principle that "This raven is black" follows from "All ravens are black;" otherwise, how would my dropping the principle affect the logical connections themselves?" (Padro 2015: 107)

Below, I'll take a close look at the reasoning that Padro gives to support this interpretation of Quine. I'll argue that the reasoning isn't sufficient to establish the needed interpretation. I'll also give some positive reasons for resisting this characterization of Quine. My reasons for resisting this interpretation of Quine are based on a principle of charity in interpretation, but also on the fact that there are alternative ways of reading key passages in Quine.

### 3.6.1 Reasons cited by Padro in favor of Kripke's interpretation of Quine

Padro reads Quine as holding the view that just by revising a belief about a law of logic, you thereby change the inferential connections between other beliefs. Regarding Quine's view, she says

On this view, revising "the statements of the logical connections" has an immediate effect on the inferential connections themselves. Hence, when facing a recalcitrant experience, it would seem that we could just go to the core of the system, modify a logical truth, and by doing so change all the inferential connections. (Padro 2015: 103)

Padro says there are two considerations that support this interpretation of Quine. She says it's supported on the grounds that Quine failed to stress a distinction between laws of logic and inferential connections. She also says it's supported on the grounds that Quine claimed that logical laws provide inferential connections.

But the idea that we can block an inferential connection between the statements in the system by dropping or changing a logical law indicates that it is because we accept the principles that the corresponding inferential transitions are taken to hold. Quine's failure to stress the distinction between the inferential connections and the laws of logic, and his claim that the latter "provide" the connections also reinforce this interpretation. (Padro 2015: 110)

To support the claim that Quine failed to stress the relevant distinction, and to support the

claim that Quine thought logical laws provide inferential connections, Padro cites the following passages.

Re-evaluation of some statements entails re-evaluation of others, because of their logical interconnections – the logical laws being in turn simply certain further statements of the system, certain further elements of the field. Having re-evaluated one statement we must re-evaluate some others, which may be statements logically connected with the first or may be the statements of logical connections themselves. (Quine 1951: 42)

Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. (Quine 1951: 43)

In an obvious way this structure of interconnected sentences is a single connected fabric including all sciences, and indeed everything we ever say about the world; for the logical truths, at least, and no doubt many more commonplace sentences too, are germane to all topics and thus provide connections. (Quine 1960b: 12-13.)

Regarding these passages, Padro says

These passages not only make clear that Quine neglects the distinction between the statements of the logical truths and the inferential transitions, but also that he thinks the key role is played by the statements and only grants a derivative status to the inferences themselves. (Padro 2015: 103)

So Padro claims that the cited passages support an interpretation of Quine where he “neglects the distinction between logical truths and the inferential transitions” and where he thinks statements of logic have a kind of primacy in their relationship with inferences. And this is supposed to support the interpretation of Quine where he thinks that just by revising a statement of a logical law, you thereby change the inferential connections between beliefs.

### **3.6.2 The issue of whether Quine fails to stress the distinction between laws of logic and inferential connections**

Is it true that Quine failed to stress the distinction between laws of logic and inferential connections? Before considering whether the previous passages support this claim, it's worth asking what the relevance of this would be if it were true. Even if Quine did fail to stress this distinction, that doesn't imply a lack of awareness or conflation of the distinction. Most importantly, if Quine failed

to stress the distinction, that wouldn't support an interpretation of Quine according to which he held the view that revising statements of logical connections would have "an immediate effect on the inferential connections themselves" (Padro 2015: 103).

But there are also reasons for thinking that Quine didn't fail to stress the distinction. Padro italicizes the phrase "provide connections" in the passage from Quine (1960b) to support the point that on Quine's view, there is some kind of relationship between statements of logic and the connections between statements that are relevant in cases of recalcitrant experience. If this is right, it doesn't support the claim that Quine fails to stress the distinction. It actually supports the opposite (because he is referring to a distinction between logical truths and connections).

It's also the case that Quine explicitly mentions a distinction between statements of logical laws and inferential connections in "Truth by Convention". As noted in the previous chapter, he distinguished between the possibility of a convention being explicit vs. pre-verbally formulated. While he didn't buy into either notion of convention as being explanatory of logical truth, he did raise an issue about how pre-verbally formulated conventions would be distinguished from a pre-positivist conception of the a priori or a psychological claim stating that a statement is merely firmly accepted. This latter psychological notion of firm acceptance could be cited to make sense of the "connections" that are referred to in previously cited passages. For example, when Quine talks about re-evaluation of some statements entailing re-evaluation of others because of their logical interconnections, the connections could be understood as psychological in nature (perhaps as dispositions to accept some statements given that certain others are accepted). At the very least, the fact that Quine is known to consider psychological notions like firm acceptance in his discussions of logic should give us pause before assuming that he failed to stress the distinction between logical truths and inference. Moreover, the passages that Padro cites don't support the claim that any such distinction is being overlooked or not stressed enough.

### 3.6.3 The issue of whether Quine takes statements of logical truth as having a kind of primacy in their relationship to inferences

The previously cited passages are also supposed to support Padro's view that Quine treats statements of logical laws as having a kind of primacy over inferential transitions. It certainly is true that some of the passages can be read as indicating a view where acceptance of a logical law will establish some kind of connection between claims. For example, in the passage from p. 43 in "Two Dogmas of Empiricism", Quine says that amending statements of logical laws can eliminate recalcitrance. But when Quine says that revision of logical hypotheses can eliminate recalcitrance, it doesn't follow that he meant that the mere revision of a logical hypothesis eliminates recalcitrance. It is open for us to think that Quine meant that revision of logical hypotheses, perhaps in conjunction with some other modification of associated psychological properties (e.g. a disposition to infer in accordance with the hypotheses) could eliminate recalcitrance. In the same way, in the passage from Quine (1960b), when Quine uses the phrase "logical truth", he may be using it as a vague proxy for not just statements like "every instance of  $\phi \vee \neg \phi$  is true" but also associated patterns of inference.<sup>18</sup>

There are also some reasons for thinking that Quine couldn't have thought that a belief in UI was necessary in order for an individual to have an ability to infer by UI. It is not clear why Quine would have thought this especially if it is implausible to think that children have logically sophisticated beliefs about implication (e.g. that every universal claim implies each of its instances).<sup>19</sup> In other words, it's not clear that Kripke's interpretation of Quine is charitable because it may presume that Quine was grossly over-intellectualizing the process of reasoning.

### 3.6.4 What if Kripke's interpretation of Quine is right?

Even if Kripke's interpretation is granted as a plausible characterization of what Quine meant, a form of empiricism about logic may still be defensible from the standpoint of Quine's wider views

<sup>18</sup>In that case, it might lend some plausibility to Padro's view that Quine fails to stress the distinction, but it doesn't support the claim that Quine thought that you can block an inference merely by revising a belief in a logical law. The passages are compatible with the view that Quine thought you could block an inference by revising a general inferential rule (as Michael Devitt suggests in unpublished work).

<sup>19</sup>As noted previously, Boghossian mentions a point like this in connection with internalist assumptions about warranted reasoning (see his "How are Objective Epistemic Reasons Possible" (2001)).

about naturalized epistemology.<sup>20</sup> In other words, If Kripke is right, it may merely mean that Quine had a false belief about an empirical question, namely regarding the way that belief revision works. For example, if it's true that individuals (e.g. children) engage in logical inference without needing any beliefs about implication or necessary truth preservation, then it will follow from a naturalized epistemology that acceptance of a claim like "all universal claims imply each of their instances" won't be necessary for (and therefore doesn't have any logically primary status with respect to) inferences that accord with UI.<sup>21</sup>

## Concluding Segment

Kripke's puzzle provides another example of how vicious regress arguments can be analyzed in terms of their intended aim and underlying assumptions. I've argued that even though Quine and Kripke's arguments can be distinguished in terms of the theories they aim to critique, they are both comparable in the sense that they turn on the same kind of underlying considerations. Notably, neither of them needs to be understood in terms of considerations that concern epistemic justification. This same kind of issue will be relevant in the following chapter on Beall and Restall. I will develop a vicious regress argument for their version of logical pluralism and I will argue that the regress isn't based on considerations that concern epistemic justification.

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<sup>20</sup>I don't want to focus too much on details concerning how exactly to understand Quine's naturalized epistemology, but see Quine (1969) for a key text. It should suffice to say that he held a view where empirical methods can be brought to bear on (and inform us about) our epistemic faculties. So it's part of his view that empirical psychology might ultimately show us that inference is something that needn't be underpinned by a belief about anything like logical laws. It is worth comparing a distinction that Quine (1970) makes concerning grammatical systems. He says a grammar may "fit" a speaker's behavior even if the speaker does not know the grammar and the speaker's behavior is not "guided" by the grammar. The same distinction can be made in the case of logical inference. And our best theory of inference may ultimately show us that inferences needn't be guided by any beliefs about logical laws. A logical law may fit an inference even if the inference isn't guided by it.

<sup>21</sup>Devitt (unpublished) elaborates a view that is compatible with a naturalized epistemology where we can explain the empirical revision of logical principles. To the extent that an agent has beliefs about about logical laws, the revision of those beliefs is something that can be explained (on Devitt's view) in terms of facts about what the agent believes and facts about the inferential rules that govern the agent's belief revision process.

## Part II

# Vicious Regress Arguments and Logical Pluralism

## Chapter 4

# Beall and Restall's Logical Pluralism

In chapters 1-3, I discussed the Carrollian regress and various arguments that are supposedly based on it. In each case, Carrollian regress points were cited to argue against some thesis in the epistemology or metaphysics of logic. In the following chapters, I'll argue that Carrollian regress points can be cited to formulate a criticism of logical pluralist theses in the philosophy of logic, starting with a form of logical pluralism that is defended by Jc Beall and Greg Restall (2006).

Beall and Restall defend a version of logical pluralism that is based on a case-theoretic analysis of logical validity.<sup>1</sup> I'll describe their view in section 4.1, and in section 4.2, I'll argue that their view results in a vicious infinite regress. I'll develop the vicious regress argument in 4.2.1, and I'll give an account of vicious regress arguments in section 4.2.2 where I'll refer to a view of vicious regresses from John Passmore (1961). The regress developed in 4.2.1 is based on explanatory considerations, and it is generated on the basis of a challenge for logical pluralists (in the style of Beall and Restall) to explain why an instance of disjunctive syllogism is classically valid. I'll also argue that this point about vicious regress constitutes a consideration in favor of logical monism. In other words, logical monism has a theoretical advantage over logical pluralism because it avoids the vicious regress.

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<sup>1</sup>In the following, I'll sometimes refer to Beall and Restall as "BR".

In section 4.3, I'll look at a few objections to the vicious regress argument that I present. Some of those objections are premised on the idea that there is no such regress in the first place. To respond to objections of this form, I'll consider the following questions. Why should a Beall and Restall-style pluralist need to explain why an instance of disjunctive syllogism is classically valid in the first place? (4.3.1) Why couldn't they trivially give an explanation? (4.3.2) Why couldn't they give an explanation in terms of modus ponens that avoids a regress? (4.3.3) I'll also look at some objections that accept the existence of a regress, but deny that the regress is vicious. In particular, I'll look at an objection that says that the regress is no more problematic than a kind of circularity that emerges in an ordinary soundness proof. (4.3.4) I'll also look at an objection which grants that there is a vicious regress, but denies that it is a problem specific to logical pluralism. It will be important to respond to an objection like this because it will motivate the claim that the vicious regress point should be treated as a consideration in favor of logical monism.<sup>2</sup> So I will look at an objection which purports to describe a vicious infinite regress for logical monism.<sup>3</sup> (4.3.5) I'll also address a worry that is based on the fact that the logical pluralism literature is quite large. This makes it difficult to know when a point hasn't already been made. Moreover, other philosophers have raised regress (or circularity) type worries for logical pluralism, so for that reason, I'll also provide some reasons for thinking that the point being offered here is new.<sup>4</sup> (4.3.6) A main observation is that other regress objections tend to be about epistemic justification or rational reasoning (rather than explanation).

In section 4.4, I'll consider another objection which I want to keep separate from the others. It concerns the idea that Beall and Restall's view might be elaborated in terms of an indexical contextualist semantic theory regarding expressions like "logically valid". I want to keep this section separate because I do not think that Beall and Restall understand their view in terms of any particular semantic thesis. That being said, an indexical contextualist version of logical

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<sup>2</sup>A complete discussion of this point would also require a comment about logical nihilist positions (e.g., see Cotnoir (Forthcoming) and Russell (2018)), but I will not consider this for reasons of space and desire for focus.

<sup>3</sup>The discussion of this objection is organized into subsections. The first presents an example of a purportedly analogous regress for logical monism, and the second argues that the example isn't analogous. The third section presents some reasons for thinking that it shouldn't be easy to tweak the example in a way where an analogous regress results for logical monism.

<sup>4</sup>This section will also be organized into subsections. Each subsection will look at an objection that Beall and Restall have responded to, and in each case I'll argue that the responses don't apply to the argument presented in section 4.2.

pluralism is defended by Colin Caret (2017). He argues that certain regress worries can be avoided if Beall and Restall's view is articulated in terms of an indexical contextualist theory. I'll give an exegesis of Caret's view, and I'll argue that a vicious explanatory regress will still emerge for a theory like Caret's where Beall and Restall's view is elaborated in terms of an indexical contextualist hypothesis about expressions like "logically valid".

## 4.1 Exegesis of Beall and Restall's Logical Pluralism

Beall and Restall defend a version of logical pluralism that is based on the idea that we have a pre-theoretic notion of logical consequence. They say that the notion is somehow imprecise.

[T]he pre-theoretic notion of logical consequence is not formally defined, and it does not have sharp edges. (BR 2006: 28)

On their view, there are multiple ways to make the notion precise, and there are boundaries on what counts as a suitable precisification. They say

the notion is not so free that any relation will do. (BR 2006: 28)

One constraint is that deductive validity concerns truth preservation across all cases.

[D]eductive validity is a matter of the preservation of truth in all cases (BR 2006: 23)

They give a formal characterization of this idea with a schema they call the GTT:

Generalized Tarski Thesis (GTT): An argument is *valid<sub>x</sub>* if and only if, in every *case<sub>x</sub>* in which the premises are true, so is the conclusion (BR 2006: 29)

But this still leaves open details that can be settled in different ways. They say the GTT

is only a recipe for specific accounts of consequence; particular precisifications are gained only when *case<sub>x</sub>* is specified. (BR 2006: 29)

Cases can be specified in terms of Tarski models, situations from a relevant logic, constructions, or metaphysically possible worlds. The following *instances* of the GTT result from different specifications of the notion of case (provided that "world" and "model" are understood in terms of

metaphysically possible worlds and Tarski models, respectively).

An argument is valid if and only if, in any world in which the premises are true, so is the conclusion. (BR 2006: 37)

An argument is valid if and only if, in every model in which the premises are true, so is the conclusion. (BR 2006: 39)

But Beall and Restall say only some instances of the GTT are “admissible”. For an instance of the GTT to be admissible, it must in some way agree with language.

Whether candidates are *admissible* turns on whether they agree with the settled parts of language, on whether they exhibit the features required by the (settled) notion of logical consequence. (BR 2006: 29)

In regards to the features that are required by the settled notion of logical consequence, Beall and Restall seem to have in mind something concerning the usage of expressions like “follows from” and “logical consequence”. In particular they are concerned with usage that somehow concerns necessity, normativity, and formality. They say their thesis

points to a number of different ways to make the pre-theoretic notion of (deductive) logical consequence precise. Each such precisification purports to incorporate the core features involved in the use of ‘follows from’ or ‘logical consequence’ (e.g. necessity, formality) (BR 2006: 28)

Beall and Restall do not mention normativity in the above passage, but it falls alongside necessity and formality on their view as a key constraint on logical consequence.

The truth of the premises of a valid argument *necessitates* the truth of the conclusion (BR 2006: 14)

Logical consequence is *normative* (BR 2006: 16)

*Formality* is one feature of deductive logic that is almost invariably taken as distinctive (BR 2006: 18)

I'll say more about their understanding of these notions below, but a key point is that these features can be cited to rule out certain instances of the GTT as inadmissible.<sup>5</sup>

<sup>5</sup>They cite McFarlane (2000) in their discussion of formality, but they don't settle on a particular analysis (see Beall and Restall 2006 p. 22).

Given the account of admissibility, their logical pluralism can be characterized as the view that there is more than one admissible instance of the GTT.

*Logical pluralism* is the claim that at least two different instances of GTT provide admissible precisifications of logical consequence (BR 2006: 29)

Beall and Restall are logical pluralists by this definition since they say there are multiple admissible instances of the GTT. They say Tarski models, constructions, and situations from a relevant logic can all be employed to generate admissible GTT instances.

But this raises a question that will be helpful to consider for the purpose of clarifying their view. They think both classical logic and some form of relevant logic will be admissible. But if situations from a relevant logic form an admissible class of cases, there will be admissible notions of case where the law of explosion fails. So how can classical logic (which includes explosion) count as meeting the necessity constraint?

To understand this, it will be important to clarify Beall and Restall's conception of necessity. Their conception of necessity involves universal quantification over cases.

Consequence is *necessary* in the following sense—it applies in “all cases”. (BR 2006: 24)

This by itself does not really help to resolve the issue about why classical consequence counts as meeting the necessity constraint. If explosion fails in some situations, and situations are admissible cases, then explosion doesn't apply in all admissible cases. And if it doesn't apply in all admissible cases, then doesn't classical consequence fail to be necessary according to this conception? This might seem to be the case if Beall and Restall said nothing more about necessity. But they go on to say more.

They say that there is a pre-theoretic notion of necessity which is imprecise, and they characterize this imprecision with a schema they call the (GLT).

It is a great insight that the notion of necessity may be made more precise and amenable to analysis:

GENERALISED LEIBNIZ THESIS (GLT): A claim is *necessarily<sub>x</sub>* true if and only if it is true in every *case<sub>x</sub>*. (BR 2006: 26)

GLT serves as (at least part of) the ‘settled core’ of what we might call the pre-theoretic notion of necessity (BR 2006: 27)

So like the GTT, the GLT can be made precise with different specifications of the notion of case.

When discussing the term “necessity”, Beall and Restall say:

Sometimes we use the term to pick out what must be the case *no matter what* unconstrained by circumstance, and at other times we may talk of what is necessary, *given* some constraint or other. (BR 2006: 26)

So does this mean classical logic is only necessary in the sense that it applies over a restricted set of admissible cases (i.e. Tarski models)? This doesn't help to answer the question because the very thing we are asking is whether Tarski models count as meeting the necessity constraint. Luckily Beall and Restall provide further details. They say that classical logic will meet the necessity constraint if the following condition is satisfied: If it's possible for an argument to have true premises, and a false conclusion, classical logic will provide a counter-model. They talk in terms of “TM” (Tarski model) validity.

What of TM validity? This also satisfies the necessity constraint if, for any argument, if it is *possible* that the premises be true and the conclusion be false, then there is some model in which the premises are true and the conclusion is false. (BR 2006: 40)

The operative notion of possibility is left unspecified here. But this passage does help to resolve the question of how classical logic can meet the necessity constraint (regardless of the fact that there are admissible cases where explosion fails). Classical logic meets the necessity constraint as long as it provides counter-models for all possible failures of truth preservation.

But can a dialetheist accept this brand of logical pluralism and endorse classical logic? Typically, a dialetheist position is understood as one where classical logic is not endorsed. But on Beall and Restall's view, there are two kinds of endorsement. To explain this, they define a constraint that an instance of the GTT may or may not meet. They say an instance of the GTT meets the “actuality constraint” if and only if the “actual case” is in the domain of its quantifier.

An instance of GTT satisfies the actuality constraint if and only if the *actual case* is in the domain of its quantifier. (BR 2006: 82)

Beall and Restall say that an agent strongly endorses a consequence relation when the agent

takes the relation to be an instance of the GTT and thinks the instance meets the actuality constraint.

One *strongly endorses* a consequence relation if one takes it to be an instance of GTT and accepts that it satisfies the actuality constraint. (BR 2006: 82)

They say that a dialetheist cannot strongly endorse classical logic.

Can a dialetheist strongly endorse classical consequence, in addition to a paraconsistent consequence relation? The answer is 'no', because the actual case, according to the dialetheist, is not consistent. (BR 2006: 83)

All that is required to "weakly" endorse a consequence relation is that you think the relation is an admissible instance of the GTT.

One *weakly endorses* a consequence relation if one takes it to be an admissible instance of GTT. (BR 2006: 83)

So they say a dialetheist can weakly endorse an explosive consequence relation.

A dialetheist may *weakly* endorse an explosive consequence relation, as long as she (with us) does not impose the 'actuality constraint' as a criterion of the *admissibility* of a consequence relation. (BR 2006: 83)

I am actually somewhat puzzled about how a dialetheist could endorse classical logic even in the weak sense.<sup>6</sup> But if this is right, a dialetheist can weakly endorse both an explosive and non-explosive consequence relation. They just won't be able to strongly endorse the explosive relation. Nonetheless, they would count as a logical pluralist in Beall and Restall's sense. This is because Beall and Restall characterize logical pluralism as the view that "at least two different instances of GTT provide admissible precisifications of logical consequence". (See the passage quoted from p. 29.) According to them, a dialetheist can treat two different instances of the GTT as providing admissible precisifications of the GTT even if the dialetheist does not strongly endorse both of the relations.

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<sup>6</sup>If a dialetheist weakly endorses classical consequence, they should think classical consequence is admissible. But that would mean classical consequence meets the necessity constraint. And that would mean classical logic provides counter-models for all possible failures of truth preservation (as per the passage from p. 40). But a dialetheist thinks that explosion is a possible (in fact actual) failure of truth preservation. And classical logic can never provide a counter model for explosion. So a dialetheist shouldn't think classical consequence can meet the necessity constraint. This means a dialetheist shouldn't think that classical consequence is admissible. So a dialetheist shouldn't think they can weakly endorse classical consequence.

Setting aside issues about necessity and endorsement, it's worth mentioning that Beall and Restall give various characterizations of their view. Sometimes they characterize it in a linguistic way where they quantify over multiple senses for expressions like "logically follows".

the pluralist endorses at least *two* instances, giving rise to two different accounts of deductive consequence (for the same language), two different senses of 'follows from'. (BR 2006: 29)

They also characterize their view in a way where they quantify over multiple relations of logical consequence.

Crudely put, a pluralist maintains that there is more than one relation of logical consequence. (BR 2006: 25)

And they talk as though logical consequence relations are in natural language.

provided that each of the noted senses of 'validity' corresponds to an admissible instance of GTT, there are at least two relations of logical consequence (in English), and so logical pluralism follows. (BR 2006: 31)

I am not sure whether these different formulations are intended to be saying the same thing, but it seems that they are in some way interchangeable on Beall and Restall's view.

## 4.2 A Lewis Carroll-style puzzle for Beall and Restall's version of Logical Pluralism

The argument that Beall and Restall's view faces a vicious regress is composed of two stages. It first of all has to be argued that there is an infinite regress. So, in section 4.2.1, I will argue that Beall and Restall's logical pluralism does result in an infinite regress. Second, it has to be argued that the regress is vicious. So, in section 4.2.2, I'll say a little bit about how I understand what it means for a regress to be vicious, and then, in section 4.2.3, I'll argue that the regress for Beall and Restall's style of logical pluralism is indeed vicious.

I think it is important to say something about how I am understanding what it means for a regress to be vicious because there are various ways of spelling out the details of this idea. Since my

discussion of this is informed by a view of vicious regress arguments from John Passmore (1961), I will look at some things he says regarding the matter in section 4.2.2.<sup>7</sup> Before moving forward, I'll simply note that the regress is generated on the basis of an explanatory challenge. The challenge is to provide an explanation of why the following argument (A) is classically valid:

(A1) Two and two is five or two and two is four.

(A2) Two and two is not five.

(AC) So, two and two is four.

There's a question about why anyone should need to explain the fact that (A) is classically valid in the first place. I'll look at objection below (in 4.3.1) which says that a BR-style pluralist shouldn't need to explain why (A) is classically valid, but for now I'll assume that this is something that can and should be explained. The key point for developing the regress is that, for logical pluralists, there won't a fact of the matter about whether (A) is really valid. This is because (A) is an instance of disjunctive syllogism, and Restall says explosion and disjunctive syllogism are

valid classically, and invalid relevantly. As a pluralist about logical consequence, I take it that there is no further fact of the matter as to whether explosion, or disjunctive syllogism are *really* valid. (Restall 2002: 426)<sup>8</sup>

So there is, for the logical pluralist, no fact of the matter about whether (A) is really valid.<sup>9</sup> But the passage also shows, that on their view, it is supposed to be a fact that (A) is *classically* valid. That (A) is classically valid is something that should be granted by logical pluralists and monists alike. But in the following section I'll show how an infinite regress results from an explanation of the classical validity of (A) (when a logical pluralist framework is assumed).

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<sup>7</sup>I'm referring to Passmore at the suggestion of Priest. I think my view of vicious regresses is also largely compatible with the characterization of vicious regresses in Priest (2014).

<sup>8</sup>Restall speaks in the first person here. But he also speaks on behalf of Beall and Restall in passages where he says "[w]e take it that there are arguments that are valid according to one logic, and invalid according to another, and that there is no further fact of the matter as to whether the argument is *really* valid." (Restall 2002: 426)

<sup>9</sup>Someone might raise a question about whether the idea of an argument being "really" valid even makes sense. But some notion of an argument being really valid seems to be operative among philosophers of logic who take seriously the idea that there are open questions about what the laws of logic are. For example, theorists who reject principles of classical logic will sometimes says things like "I grant that LEM is classically valid, but it isn't really valid". There is a useful discussion of a notion of validity that plays this role in Field (2015).

### 4.2.1 Why explaining the classical validity of (A) will result in an infinite regress for Beall and Restall's logical pluralism

If we are asking for an explanation of why (A) is classically valid, what would an explanation need to look like on Beall and Restall's view? Given the role that the GTT plays in their case-theoretic form of logical pluralism, Tarski models should figure into their explanation.<sup>10</sup> For example, it should be part of their view that:

(B1) (A) doesn't preserve truth in all Tarski models or (A) is classically valid.

Citing (B1) as a premise, we can regiment an explanation of why (A) is classically valid via the following argument (B):

(B1) (A) doesn't preserve truth in all Tarski models or (A) is classically valid.

(B2) It's not the case that (A) doesn't preserve truth in all Tarski models.

(BC) So, (A) is classically valid.

While there are other (perhaps more natural) ways of regimenting an explanation in terms of Tarski models, this appears to be a perfectly acceptable regimentation.<sup>11</sup> The premises describe

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<sup>10</sup>Note I'm not making an exegetical claim here. I'm not assuming that Beall and Restall say anything about what an explanation of the classical validity of (A) would need to look like. I'm claiming that they are committed to thinking it would be appropriate to cite facts about Tarski models in an explanation of why an argument is or isn't classically valid. I think this is plausible given that they have a case-theoretic analysis of validity (encapsulated by the GTT). But to reinforce this point, it may be worth noting the following passage where they explicitly refer to Tarski models.

Now we turn to what we take to be the most powerful and productive idea, constraining the concept of logical consequence. The idea is available in nascent form throughout the tradition, and it has surfaced explicitly in Leibniz's possible worlds, Bolzano's variable terms, Husserl's manifolds, and most clearly, Tarski models. (BR 2006: 23)

<sup>11</sup>A more natural explanation (without any double negation) can be regimented as follows, where M is an arbitrary Tarski model:

- 1 "Two and two is not five" is true in M (by assumption)
- 2 "Two and two is five or two and two is four" is true in M (by assumption)
- 3 Either "Two and two is five" is true in M or "two and two is four" is true in M (2, classical semantics)
- 4 "Two and two is five" is not true in M (1, classical semantics)
- 5 "Two and two is four" is true in M (by DS 3, 4)

While this regimentation may be more natural, a step by DS will still occur at the step from 3 and 4 to 5. So the more concise regimentation in the main text will save space and be more illuminating for this reason. There's an issue about whether a step by DS would have to occur in a suitable explanation. But even if DS doesn't need to occur, some other basic rule will have to occur, and that rule can be taken up as a starting point for generating a regress. For that reason (and others), I'll consider an objection (in section 4.3.3) which raises an issue about whether the classical validity of (A) might be explained in terms of an argument in the form of MP. I'll respond by showing

relevant facts about truth preservation in Tarski models, and the conclusion states that (A) is classically valid.<sup>12</sup> But a regress can be developed here that will ultimately reveal a difficulty for the logical pluralist's ability to treat (B) as a suitable explanation of why (A) is classically valid.

On the logical pluralist view (as per the previously cited passage from Restall), there will be no fact of the matter about whether (A) is really valid. But if there is no fact of the matter about whether (A) is really valid, then the same should hold for (B) (since they are both just instances of DS). And if there's no fact of the matter about whether (B) is really valid, it's not clear what relevance (B) could have for the purpose of explaining why (A) is classically valid.

It might be suggested that (B) can still nonetheless explain why (A) is classically valid. Even if there is no fact of the matter about whether (B) is really valid, couldn't there still be a fact of the matter about whether (B) necessarily preserves truth? If (B) is a necessarily truth preserving argument (with true premises), what more is needed?

Unfortunately, this will not salvage any explanatory relevance. Let's say we add some precision to the notion of necessity, and articulate it in terms of some notion of "metaphysical" necessity. To say that (B) is metaphysically necessarily truth preserving is to say nothing more than that in every metaphysically possible world, either one of the premises of (B) is false, or the conclusion of (B) is true.<sup>13</sup> Given this, we can grant, that in the actual world, either one of the premises of (B) is false or the conclusion of (B) is true. In other words, the following claim (C1) is true.

(C1) At least one of the premises of (B) is false or the conclusion of (B) is true.

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that a regress of the same sort can be generated for an explanation in the form of MP.

<sup>12</sup>I want to make a clarification about why I think (B) seems acceptable as an explanation of the classical validity of (A). It isn't merely the fact that (B) is logically valid. The premises of a logically valid argument needn't explain their conclusion. For an example, consider the following argument: (p1) The weather reporter says it will rain; (p2) If the weather reporter says it will rain, it will rain; (c) It will rain. Even granting that the premises are true (and that they logically imply (c)), they still won't explain why it will rain. So why should the premises of (B) be seen as an explanation of the conclusion of (B)? The conclusion of (B) is that (A) is classically valid. The premises of (B) state facts about whether (A) preserves truth in Tarski models. That means that the premises and conclusion are connected via Beall and Restall's Tarski model analysis of classical validity.

<sup>13</sup>Someone might argue that this is not the right way to understand necessary truth preservation. For example, some give a conditional analysis of necessary truth preservation. In fact, this seems to be the view of Beall and Restall. They say "What is necessary, in an argument from A to B, is not the conclusion B but the connection between A and B. The *conditional if A then B* is true of necessity." (BR 2006: 15) Even if a disjunctive and conditional analysis are not treated as equivalent, I don't think the main point changes because the same regress argument can be run for an explanation in the form of MP (and I'll argue for this in section 4.3.3).

But it's not clear why this would be explanatorily relevant on the logical pluralist view. We might grant that the left disjunct is false, but that won't be of any relevance if there is no fact of the matter about whether DS is really valid. This point can be illustrated if we draw our attention to the following argument (C), where (C1) is treated as a premise.

(C1) At least one of the premises of (B) is false or the conclusion of (B) is true.

(C2) It's not the case that at least one of the premises of (B) is false.

(CC) So, the conclusion of (B) is true.

Given Beall and Restall's view, there won't be a fact of the matter about whether (A) and (B) are really valid. But if there isn't a fact of the matter about whether (A) and (B) are really valid, then there won't be a fact of the matter about whether (C) is really valid either (since they are all just instances of DS). And if there is no fact of the matter about whether (C) is really valid, it's not clear how the explanatory relevance of (B) could be improved by pointing out that (B) is metaphysically necessarily truth preserving. The important explanatory upshot of the fact that (B) is metaphysically necessarily truth preserving is that in the actual world either one of the premises of (B) is false or the conclusion of (B) is true; this is just what is stated in the first premise of the argument (C). We can grant that none of the premises of (B) are false; i.e., we can grant the second premise in the argument (C). But according to the pluralist, there is no fact of the matter about whether (C) is really valid. Only the monist can say that (C) is really valid, so only the monist can make sense of the idea that metaphysically necessary truth preservation is an explanatorily relevant feature.

But even if the pluralist cannot say that (C) is really valid, can't they say that (C) metaphysically necessarily preserves truth? They can say this, but only the monist can explain why that would make (C) explanatorily relevant. If (C) metaphysically necessarily preserves truth, then in every metaphysically possible world, either one of the premises of (C) will be false or the conclusion of (C) will be true. The important explanatory upshot of this is that in the actual world, either one of the premises of (C) will be false or the conclusion of (C) will be true. We can grant that none of the premises of (C) are false. But if there is no fact of the matter about whether DS is really valid, then the explanatory relevance of this is nil. This can be illustrated by considering the

following argument (D).

(D1) At least one of the premises of (C) is false or the conclusion of (C) is true.

(D2) It's not the case that at least one of the premises of (C) is false.

(DC) So, the conclusion of (C) is true.

(D1) encapsulates the important explanatory upshot of the fact that (C) metaphysically necessarily preserves truth. But how can this important explanatory upshot be important if there is no fact of the matter about whether (D) is really valid? Only the monist can grant that this argument is really valid, so only the monist can make sense of the idea that (C)'s explanatory relevance is supported by the fact that (C) metaphysically necessarily preserves truth.

It should be evident at this point that the state of affairs will not be significantly changed by drawing attention to the fact that (D) metaphysically necessarily preserves truth. The explanatorily significant upshot of that fact would be that either one of the premises of (D) is false or the conclusion of (D) is true. We can grant that none of the premises of (D) are false, but even if we grant that, it's not clear how any of this would be explanatorily relevant unless DS were really valid. We could illustrate this by regimenting a further argument (E), but this would only move us to a further stage in the progression that is for all intents and purposes exactly like the stage before it. And no matter how far up the ladder we climb, the same thing will continue to happen. We will encounter some instance of DS of the form (X):

(X1) At least one of the premises of (X-1) is false or the conclusion of (X-1) is true.

(X2) It's not the case that at least one of the premises of (X-1) is false.

(XC) So, the conclusion of (X-1) is true.

But at none of these stages will the pluralist ever grant that any one of these instances of DS is really valid. So there will never be a stage where they reach a suitable explanation. For any given (X), the pluralist can assume that (X) metaphysically necessarily preserves truth. But the explanatorily significant upshot of this amounts to nothing more than what is endorsed in the first premise in (X+1). But that will be of no help since they won't grant that (X+1) is really valid. Only the monist can grant that. And since the pluralist cannot grant this, each stage will provide

exactly the same incentive to move on to a further stage; the regress is infinite.

### 4.2.2 What it is for a Regress to be Vicious

Granting that Beall and Restall's style of logical pluralism results in a regress, there is still a further question about whether the regress is vicious. I'll answer this question in the affirmative, but some clarifications are in order because we need to say what it means for a regress to be vicious in the first place. When someone says that a view results in a vicious regress, this is obviously meant to be a problem for the view in question. But different philosophers will not always have the same thing in mind when they discuss vicious regress arguments. For example, there may be variance regarding what they take to be the aim of a vicious regress argument and what it is about a vicious regress that makes it problematic.

For these reasons, I'll say what I mean by "vicious regress", and in the following I'll show that the previous regress meets the requirements of viciousness (as I understand it). Ultimately, I think the previous regress shows that a certain kind of explanation cannot be given in terms of Beall and Restall's pluralist framework. So the main aim in making the clarifications below will be to show how the regress reveals an underlying explanatory difficulty.

When philosophers talk about vicious regresses, they may describe them as things that are problematic in and of themselves or else as things that merely reveal an underlying problem.<sup>14</sup> I'll be following the latter way of thinking about vicious regresses, but no matter how they are understood, it is important to distinguish between a harmless and a vicious regress. This is crucial because a vicious regress argument cannot be based on the assumption that there is a problem with an infinite series.<sup>15</sup>

If we think of vicious regresses as things that reveal an underlying problem, this will be compatible with a way of reading the regress in Carroll. The Carroll regress can be seen as merely revealing a sense in which some underlying assumption of the tortoise is problematic. In particular, the Carroll regress can be seen as something which merely reveals a kind of arbitrariness

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<sup>14</sup>See the Stanford Encyclopedia entry from Cameron (2018) for this point and many other helpful distinctions regarding the logic of infinite regress arguments.

<sup>15</sup>Obviously this is barring certain views about the nature of totalities and finitist theories in philosophy. See Cameron (2018) for more on this point about finitism.

in the tortoise's demands. If the tortoise didn't think that the initial premises ( $A$  and  $B$ ) were sufficient, then it would be arbitrary for the tortoise to think that some further premise  $C$  would be helpful.

In the same way, the regress developed in the previous section can be understood as revealing an underlying problem with Beall and Restall's logical pluralism. The idea is that if there is no fact of the matter about whether an argument like (B) is really valid, then there will be no explanation of why an argument like (A) is valid in a qualified senses (i.e, you will not be able to explain why (A) is "classically valid"). So does the regress actually reveal a problem? Is it vicious? Since I am thinking of vicious regress arguments in a way that is in line with Passmore, I'll note a few things that he says about regresses. This will be helpful for showing why the regress is vicious, but it will also be helpful for understanding what kind of assumptions are built into a vicious regress argument.

First of all Passmore clarifies that viciousness requires more than a mere infinite regress. There can be both vicious and non-vicious infinite regresses. He says

Consider such propositions as: every event has a predecessor, every event has a cause, it is always logically possible to question a proposition. Now, it is sometimes objected to assertions such as these that 'they involve an infinite regress', a regress which can only be stopped by claiming privilege for a particular event which has no predecessor (e.g. the creation of the universe) or some proposition which it is logically impossible to question (e.g. that a necessary being must exist). But in fact the regress argument simply does not apply in these cases. If to 'it is logically possible to doubt every proposition' the objection is raised: 'then it must be logically possible to doubt *this* proposition, and logically possible to doubt whether this proposition can be logically doubted, and so on'—the reply can simply be given 'and so it is'. (Passmore 1961: 28)

To understand what makes a regress vicious, it is helpful to think about what a vicious regress argument aims to establish. In Passmore's characterization, they are described as arguments that aim to establish that a certain explanation or analysis cannot be given.

Philosophical regresses, on the contrary, demonstrate only that a supposed way of explaining something or 'making it intelligible' in fact fails to explain, not because the explanation is self-contradictory, but only because it is, in the crucial respect, of the same form as what it explains. (Passmore 1961: 33)

So a vicious regress argument (on Passmore's conception) doesn't aim to establish that a

hypothesis is false or contradictory. It merely concludes that an explanation (of a certain type) cannot be given. In the above passage, Passmore also explains the reason why the explanation is said to fail. The problem turns on the idea that the explanation would be “of the same form as what it explains”. In other words, the explanation or analysis is said to fail on account of the fact that it re-introduces the very same kind of problem that it was brought in to solve.

There is clearly some vagueness here about what it means for an explanation to be problematically “of the same form” as the thing it tries to explain, and it’s not unnatural to think that an explanation will have to have *some* kind of similarity with the thing it explains. But Passmore gives many examples that are helpful for understanding the key idea. He cites Plato’s Parmenides, and one of the main examples in his discussion concerns a vicious regress argument that is applied to a “Platonic Form” analysis of predication. His aim is not to exegete Plato, but in his discussion of the example, the aim of the argument is to show that predication cannot be explained in terms of relationships to something like Platonic Forms. Passmore says

[t]he infinite regress argument can then be used as an emphatic way of pointing out that if ‘sharing the same property’ is unintelligible, then so also is ‘participating in the same form’. For suppose we consider any property P which  $x, y, z$ , share; and then we say that when  $x, y, z$  appear to share the property P what really happens is that they are related in a certain way to the form P. Then we have simply replaced the original property of ‘being P’ by a new property ‘being-related-to-the-form-P’. If there is any unintelligibility attaching to the fact that  $x, y, z$ , can all share the property P, there will be exactly the same difficulty in understanding how they can all share the property of ‘being-related-to-the-form-P’. (Passmore 1961: 21)

The Platonic Form case also provides a good example of Passmore’s characterization of the conclusion of a vicious regress argument (where the conclusion states that a certain kind of explanation fails). He says

[t]his means that if anybody wants to have it explained to him how a number of different things can share the same property, he has to be told that this *cannot be explained*, in the sense, at least, that we cannot substitute for ‘having the same property’ another predicate the use of which does not already presume that things can have a predicate in common. (Passmore 1961: 23)

He also describes the vicious regress argument as establishing that predication is simply part of how things are.

the importance of the infinite regress argument remains: it shows that in an important sense of 'explain' it is impossible to explain predication. If somebody says: 'I find it unintelligible that things should have the same property', there is a sense in which we have to say to him: 'that's just the way things are'. (Passmore 1961: 24)

So the Platonic Form case provides us with an example where the notion being analyzed (having a property in common) is presupposed in the analysis (being related to the same form). If the idea of being related to the same form just is the idea of having a certain property in common (the property of being related to the same form), then the analysis fails.<sup>16</sup>

Passmore gives a general description of this feature of vicious regress arguments (where the aim is to show that an explanation re-introduces the problem it was originally brought in to solve).

As I have already suggested, it is the first step in the regress that counts, for we at once, in taking it, draw attention to the fact that the alleged explanation or justification has failed to advance matters; that if there was any difficulty in the original situation, it breaks out in exactly the same form in the alleged explanation. If this is so, the regress at once develops; whether it is so, is the point of controversy. (Passmore 1961: 31)

It's relatively straightforward to see how a feature like this will generate a regress. If the first stage required the second stage, and the second stage is not significantly different (i.e., it involves the very same problem), then a further stage will be called for as well.

This feature is significant because it is one place where a regress argument might be resisted. Someone might deny that there is a regress at all. If someone can support the claim that the explanation or analysis is privileged (i.e., that it is not problematically "of the same form as what it explains"), then the regress will not be generated. Passmore notes this in the following passage where he comments on a vicious regress argument that is applied to a certain theological explanation.<sup>17</sup>

an infinite regress argument, like any other argument, has force only under relatively

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<sup>16</sup>It's worth asking whether there needs to be a univocal sense in which an explanation is problematically "of the same form as what it explains", but I don't see that anything in the main text depends on how this question is answered.

<sup>17</sup>He also relates this point to his example regarding Platonic form theories of predication. He says "[s]o far, I have been simplifying, because I have been proceeding as if the infinite regress argument were entirely 'knock-down'. But of course, there are ways of evading it." (1961: 24) He says a person may respond on behalf of the Platonic form theory of predication by saying that "[i]t is not necessary for me to suppose the existence of a further form to explain the possession of a common relational predicate, for this does not need explanation." (1961: 24)

complex circumstances. We can, wrongly, be convinced that by it alone we can demolish a philosophical explanation when in fact we are working with an auxiliary hypothesis—the assumption that no privilege is to be claimed for any of the class of propositions to be explained—and this hypothesis our opponents will simply not admit. Thus to revert to an earlier example, they will not admit that to explain why ‘anything’ exists by reference to God’s existence involves a regress, because, they will say, the existence of God does not require explanation in the sense in which the existence of other things requires explanation. (God is not, that is, part of ‘anything’.) (Passmore 1961: 28)

This leaves us with an analysis of vicious regress arguments where two things are necessary for viciousness. On the one hand, there has to be an infinite regress. On the other hand, the regress has to be generated in a way where each stage re-introduces the same problem that was originally supposed to be solved.

### 4.2.3 Why the Regress for Beall and Restall’s Pluralism is Vicious

Given this understanding of viciousness, there are only two things that have to be argued in order to establish that there is a vicious infinite regress. First, an infinite regress must be generated. Second, the regress must be generated on the grounds that each stage places us in a position that is not significantly different from the stage we were previously at. The purpose of section 4.2.1 was to establish that we do have an infinite regress. So is it generated in a way that suffices for viciousness?

As mentioned in section 4.2.1, the regress is initiated with an instance of DS, i.e. (B), which is supposed to be an explanation of why another instance of DS, i.e. (A), is classically valid. The problem for the pluralist is that (B) cannot play the requisite explanatory role, because on their view, there is no fact of the matter about whether (B) is really valid. The pluralist may cite the fact that (B) is metaphysically necessarily truth-preserving to account for why (B) is explanatorily relevant. But it’s not clear how this fact would be explanatorily relevant if logical pluralism is true. If (B) is metaphysically necessarily truth-preserving, then at every metaphysically possible world, either one of the premises of (B) is false or the conclusion of (B) is true. That means at the actual world, either one of the premises of (B) is false or the conclusion of (B) is true. But that is to say nothing more than what is stated in the first premise of (C), and there is (according to the logical pluralist) no fact of the matter about whether (C) is really valid. In other words, moving to the

next stage leaves us in a position that is not significantly different from the one we were originally in. So the regress meets the requirements for being vicious.

### 4.3 Objections

Some objections deny that there is a regress in the first place. I'll look at objections of that form in 4.3.1, 4.3.2, and 4.3.3.. Other objections grant the existence of a regress, but deny that it is vicious. I'll look at objections of that form in section 4.3.4. In section 4.3.5, I'll consider an objection which takes a different form. Instead of denying that there is a vicious regress, it instead argues that monism would face an analogous regress. In section 4.3.6, I'll make some comments regarding the possibility that Beall and Restall have already responded to a consideration that is like the vicious regress argument being presented here.

#### 4.3.1 Why should a Beall and Restall-style pluralist need to explain the classical validity of (A) in the first place?

Someone might say that the classical validity of (A) isn't something that needs to be explained. If that's right, then questions of viciousness would be neither here nor there, and the regress wouldn't even get off the ground. But it seems like we can (and should) give an explanation of why an argument counts as classically valid. Classical validity is a technical notion with well understood properties, and explanations of why arguments count as classically valid are frequently given when logic is taught. A model-theoretic explanation is natural, and one way of putting the point of the regress argument is that logical pluralists cannot grant that the standard Tarski model explanations are really valid.

It also seems like Beall and Restall should be open to the idea that (B) is a sufficient explanation of the classical validity of (A) since they opt for a model-theoretic analysis of validity with their GTT schema. If a precisification of the GTT over Tarski models isn't relevant to an explanation of classical validity, then it's not really clear what the explanatory role of the GTT schema is supposed to be. The GTT is supposed to capture our pre-theoretic notion of logical

consequence, but then a precisification of the GTT over Tarski models should capture (at least one of) our theoretical notion(s) of classical validity. Given that a precisification of the GTT over Tarski models captures this theoretical notion, it must have some relevance in an explanation of classical validity. The difficulty is that only the monist can account for this relevance.

### 4.3.2 Why can't a Beall and Restall-style pluralist explain why (A) is classically valid in a way that is entirely trivial?

There is another way of resisting the idea that a regress will be generated. Instead of saying that no explanation is needed, someone could say that an explanation can be given in a way that doesn't re-introduce the problem it was brought in to solve. One way of elaborating this idea would be to say that we can explain why (A) is classically valid in a way that is entirely trivial. For example, why can't we explain the classical validity of (A) by simply noting that (A) is an instance of the classically valid schema DS?

This will not work. Instead of setting up the regress by looking at a particular instance of DS, a regress can just as easily be generated by considering DS itself. It's a bit awkward to talk in this way because DS is a schema and schemas can't be truth preserving (except in the derivative sense that their instances are truth preserving). That being said, the main point is not changed because we can set up an analogue of (B), (B!):

(B1!) (DS) doesn't preserve truth in all Tarski models or (DS) is classically valid

(B2!) It's not the case that (DS) doesn't preserve truth in all Tarski models

(BC!) So, (DS) is classically valid

As before, the pluralist view is that there is no fact of the matter about whether (B!) is really valid.

But that compromises the explanatory relevance of (B!).<sup>18</sup>

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<sup>18</sup>Someone might say that (B!) is explanatorily relevant on the grounds that it is classically valid. But appealing to the classical validity of (B!) seems too much like the thing to be explained (when the thing to be explained is the classical validity of (DS)).

### 4.3.3 Why can't a Beall and Restall-style pluralist employ modus ponens to explain why (A) is classically valid?

Here I will look at one more objection which says an explanation can be given in a way that doesn't re-introduce the same problem it was originally brought in to resolve. Instead of saying that an explanation can be given in a way that is somehow trivial, the idea is that an explanation can be given in terms of modus ponens.

The regress argument starts out with a challenge for Beall and Restall to give a case-theoretic explanation of why (A) is classically valid. It then considers a case-theoretic explanation that is given in terms of (B) (which is an instance of DS). Then the argument goes on to say that this would lead to a regress. But couldn't a case-theoretic explanation be given in terms of modus ponens instead? And couldn't the points about metaphysically necessary truth preservation be put in terms of a conditional also? Moreover, since the GTT is stated in terms of a bi-conditional, wouldn't an initial explanation in terms of modus ponens be more natural?

I do think a modus ponens explanation is more natural given the bi-conditional structure of the GTT. But I don't think this will change the overall issue because a wholly analogous regress can be constructed for MP. To see this, consider an MP analogue of (A), (A\*).

(A1\*) Two and two is not five.

(A2\*) If two and two is not five, then two and two is four.

(AC\*) So, two and two is four.

Here the challenge will be to explain why (A\*) is classically valid, but instead of explaining why (A\*) is classically valid in terms of DS, we can cite the following argument (B\*) as an explanation of why (A\*) is classically valid.

(B1\*) (A\*) preserves truth in all Tarski models.

(B2\*) If (A\*) preserves truth in all Tarski models, then (A\*) is classically valid.

(BC\*) So, (A\*) is classically valid.

Now we have a regimentation of an explanation via MP, and this does more closely align

with the GTT as stated. But this doesn't change the matter because on Beall and Restall's view, there shouldn't be a fact of the matter about whether (B\*) is really valid.<sup>19</sup> If there is no fact of the matter about whether (B\*) is really valid, it's not clear what significance it could have for explaining why (A\*) is classically valid.

As before, logical pluralists could suggest that (B\*) will count as explanatorily relevant on the grounds that it metaphysically necessarily preserves truth. But for the same reasons as before, logical pluralists cannot treat this fact as explanatorily relevant. And it won't matter if metaphysically necessary truth preservation is analyzed in terms of a conditional. Let's suppose that if (B\*) is metaphysically necessarily truth-preserving, then in every metaphysically possible world, if the premises of (B\*) are true, the conclusion of (B\*) will be true also. Given this, in the actual world, it will be the case that, if the premises of (B\*) are true, then the conclusion of (B\*) is true as well. We can grant that the premises of (B\*) are true, but none of this is explanatorily helpful if logical pluralism is true, and we can illustrate this with the following argument (C\*).

(C1\*) If the premises of (B\*) are true, the conclusion of (B\*) is true

(C2\*) The premises of (B\*) are true.

(CC\*) So, the conclusion of (B\*) is true.

If there is no fact of the matter about whether (A\*) and (B\*) are really valid, then the same will hold for (C\*). But if there is no fact of the matter about whether (C\*) is really valid, then the fact that (B\*) is metaphysically necessarily truth preserving will not improve the explanatory

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<sup>19</sup>Why? On many accounts, DS is equivalent to MP. So any non-factuality in the validity of DS should be shared with the validity of MP. A pluralist might resist this argument though. MP is equivalent to DS classically, but not in some relevant logics. So if one of those relevant logics was admissible, Beall and Restall could say there is no fact of the matter about whether DS is equivalent to MP. Nonetheless, there are logics without modus ponens (e.g. FDE) that meet Beall and Restall's admissibility constraints. This shouldn't be too surprising since Beall and Restall have already shown how logics with gaps and gluts can meet the admissibility constraints. But we can also see how FDE meets the constraints by looking at each constraint in turn. The necessity constraint is explained in terms of whether a logic can provide counter models for all possible failures of truth preservation. (BR 2006: 40). So since Tarski models meet this constraint, FDE models will also (since Tarski models are a proper sub class of FDE models). For the normativity constraint, they say rejecting the arguments that are valid in an admissible logic "involves cutting the ground from under your own feet." (BR 2006: 24). So if classical consequence meets this constraint, FDE will also (because FDE validities are a proper sub class of classical validities). Lastly, FDE draws the formal vs. non-formal distinction in the same way as other logics that Beall and Restall have already granted meet the formality constraint. Maybe if further requirements were added for admissibility, logics without MP could be ruled out, but I'm not sure what the details of this would look like. It could be suggested that modus ponens is part of our pre-theoretic notion of logical consequence, but this would make Beall and Restall's view much less principled. It would no longer be the case that admissibility is determined by general properties (like necessity and formality), so worries would be generated about the view being *ad hoc*.

relevance of  $(B^*)$ . The important explanatory upshot of the fact that  $(B^*)$  is metaphysically necessarily truth preserving is that in the actual world, if the premises of  $(B^*)$  are true, the conclusion of  $(B^*)$  is true as well. But that is just the conditional premise  $(C1^*)$ . We can grant the antecedent of this conditional, i.e., we can grant the second premise,  $(C2^*)$ . But even if we grant that these premises are true, the logical pluralist cannot grant that  $(C^*)$  is really valid. So, given the logical pluralist's view, it's not clear how the observation that  $(B^*)$  is metaphysically necessarily truth preserving could improve  $(B^*)$ 's explanatory significance.

It won't help to say that  $(C^*)$  is metaphysically necessarily truth-preserving either. The important explanatory upshot of  $(C^*)$  being metaphysically necessarily-truth preserving is that in the actual world, if the premises of  $(C^*)$  are true, then the conclusion of  $(C^*)$  is true. But in the same way that the argument  $(C^*)$  was constructed for  $(B^*)$ , we can construct an argument  $(D^*)$  for  $(C^*)$ , and the same goes for any further stage in the progression. At every stage, there will be an instance of MP. But for the logical pluralist, there will never be a stage where a suitable explanation is attained because there will never be a stage where they can say that any of the instances of MP are really valid. At any stage  $(X)$ , they can say that  $(X)$  metaphysically necessarily preserves truth, but it's not clear how this is explanatorily helpful. It amounts to nothing more than the truth of the first premise of  $(X+1)$ . But even if the first premise of  $(X+1)$  is true, it's not clear how that could be explanatorily helpful for the pluralist, because on their view, there will be no fact of the matter about whether  $(X+1)$  is really valid. In other words, all the same points from before can be made to show that there is a vicious infinite regress.

#### **4.3.4 Why not see the regress as non-vicious? For example, why shouldn't we think it's based on a kind of circularity that is present in any ordinary soundness proof?**

Instead of denying that there is a regress, another avenue for response is to simply deny that the regress is vicious. I'm sure there is more than one way to formulate an objection like this, but I'll look a version of this objection that is based on the following observation. The observation is that the regress argument raises a challenge for Beall and Restall to do something that is similar to what gets accomplished in an ordinary soundness proof. If the regress was based on properties that

are present in an ordinary soundness proof, then it couldn't be any more vicious than a soundness proof is problematic. So how is there any similarity between the explanatory challenge and what gets accomplished in an ordinary soundness proof?

The regress argument raises a challenge for a Beall and Restall-style pluralist to explain why an argument is classically valid. A soundness proof for classical logic requires someone to show that when something is classically provable, it's also classically valid. Suppose a logician is at a stage in a soundness proof where they are showing that if MP is classically provable, then MP is classically valid. In the process of showing this, the logician will use MP. In other words, the logician uses MP in order to infer (under the scope of a supposition that MP is classically provable) that MP is classically valid. Whether this counts as a problematic form of circularity (or whether it should be counted as a form of circularity at all), I'll refer to this feature as the kind of circularity that occurs in a soundness proof.

Presumably, this is not a problematic form of circularity. So if the regress is based on this kind of circularity, then it couldn't be any more problematic than an ordinary soundness proof. Not only would this undermine the impression that the regress is vicious, it would also show that the regress is a highly general phenomenon that couldn't count as an issue that was specific to logical pluralism. To respond to this, we need to be clear about exactly what kind of circularity there might be in a soundness proof. After clarifying this, I'll consider whether the regress for logical pluralism is based on the kind of circularity that appears in a soundness proof (ultimately to answer in the negative).

To say what kind of circularity there is in a soundness proof, we have to say what the conclusion of a soundness proof is. We also have to say what the premises are, and what rules of reasoning are used in the proof. Throughout this section, I'll simplify things by only talking about soundness proofs for classical logic and I'll only focus on the section of a soundness proof where the soundness of MP is at issue. I'll also talk in a way that aims to abstract from the details of any particular proof system that might be used.

A soundness proof for classical logic will presuppose that we have already defined a syntactic and a semantic relation for the language ( $\vdash_{CL}$  and  $\Vdash_{CL}$ ). The goal of a soundness proof is to

show that when the syntactic relation holds, the semantic relation holds also. In the case of modus ponens:

(*SOUND<sub>MP</sub>*) Given  $\{ \Phi, \Phi \rightarrow \Psi \} \vdash_{CL} \Psi$ ,  $\{ \Phi, \Phi \rightarrow \Psi \} \Vdash_{CL} \Psi$ .

Wherein lies the circularity? A key observation is that modus ponens is used as a rule in the process of proving this result. The proof is not “premise” circular; there is no place where we assume a claim that we are trying to prove. But the proof does exhibit something that is similar to what is sometimes referred to as “rule” circularity. Rule circular arguments are typically described in the following way. They conclude that a certain rule is valid, and they are also an instance of the very same rule that is referred to in the conclusion. For example, you might have an argument that concludes that modus ponens is valid, but is also itself an instance of modus ponens. An argument like this need not be “premise” circular because it doesn't need to have a premise that states (or somehow presupposes) that modus ponens is valid. While there is some similarity between rule circularity and the kind of circularity that occurs in a soundness proof, the cases are not exactly the same. In the soundness proof, a logician will use MP in order to infer that MP is classically valid, but this all happens under the scope of a supposition that MP is classically provable.

So is the regress argument based on the same kind of circularity that occurs in a soundness proof? It would be a problem if it was, but this question can be answered in the negative. This may be difficult to see because the argument (B) does exhibit a similar kind of circularity. (B) concludes that an instance of DS is classically valid, and (B) is an instance of that very same rule.<sup>20</sup> So how is the regress argument different?

The regress argument presents a challenge for Beall and Restall-style pluralists to give an explanation of why (A) is classically valid in terms of Tarski models. But the problem is not that Beall and Restall would have to give a rule circular explanation. The problem is that if their view is right, there would be no fact of the matter about whether the explanation (B) was really valid. If you present an argument as an explanation, but there is no fact of the matter about whether the argument is really valid, then it's not clear how the argument can constitute a sufficient explanation.

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<sup>20</sup>It is therefore rule circular in the sense described in the main text.

This explanatorily problematic feature does not occur in an ordinary soundness proof. To see this, it is important to stress that the assumptions of a soundness proof will not commit someone to this problematic feature. If someone is doing a soundness proof, there is no point where they have to say that there is no fact of the matter about whether the rules they are using are really valid.<sup>21</sup>

Given that this general point has been made, I want to clarify that the point should hold independently of certain philosophical attitudes that someone might hold regarding the significance of soundness proofs. In the following, I'll look at a view from Michael Dummett where soundness proofs are understood as having a deep philosophical significance. I'll argue that the general point in this section holds even if we have a view like Dummett's where soundness proofs are understood as having a more than merely technical significance. I also think that the general point holds if we think of soundness proofs as having a merely technical significance but I will reserve discussion of this point to a footnote.<sup>22</sup>

Dummett attributes more than mere technical significance to soundness proofs. For example, he says soundness proofs can show that inference rules are in fact valid.

By means of a soundness proof, we demonstrate that the primitive rules of inference are in fact valid (Dummett 1973: 291)

When he says this, he does not mean that a soundness proof by itself will show this. He is saying this relative to a view where the semantics of formal logic is thought of as an attempt to capture the meaning of logical connectives in our language. He talks about this in regard to disputes about the appropriateness of two-valued semantics. He says the dispute over the legitimacy of two-valued

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<sup>21</sup>In particular, when a monist is doing a soundness proof, they may rely on MP, but they never need to say there's no fact of the matter about whether it's really valid. There's a separate question about whether it should count as a problem to use MP, a classically truth preserving rule, to infer (under the scope of a reductio) that MP is classically truth preserving in a soundness proof. But if the classical soundness proof for MP is understood as a merely technical result, then it won't establish that MP is really valid. MP may be used, but if MP is really valid, and a soundness proof doesn't establish that MP is really valid, then it doesn't seem problematically circular.

<sup>22</sup>The soundness proof for classical logic tells us that when you prove something by modus ponens in CL, any assignment of truth values making the premises of the proof true, will also be an assignment making the conclusion of the proof true. For those who treat the soundness proof as a merely technical result, this fact about assignment functions will not tell us anything about whether the argument in fact preserves truth. But understood in this way, it is clear that the regress argument does not turn on any assumptions that are built into an ordinary soundness proof. *Qua* merely technical result, a soundness proof never forces someone to assume that there is no fact of the matter about whether the steps in their proof are really valid. But the regress argument does turn on the fact that, for the pluralist, there is no fact of the matter about whether (B) is really valid.

semantics concerns the question

whether we do or do not possess, for the sentences of our language, notions of truth and falsity such that to each particular utterance of any complete sentence one or other truth-value determinately attaches. (Dummett 1973: 294)

He says if this question is answered in the affirmative, then

the truth-tables provide at least one legitimate way of explaining the meanings of certain possible sentential operators. (Dummett 1973: 294)

So to the extent that our language admits bivalence, Dummett would count the two-valued semantics as a legitimate *explanation* of the meaning of disjunction. If this is right, then it presupposes a philosophical view about the relationship between meaning and truth conditions. If the meaning of a disjunction is to be understood in terms of its truth conditional contribution, then the two-valued semantics will explain meaning by representing that truth conditional contribution.

Dummett also talks about a soundness proof having explanatory relevance for the inference rules. Given the background view about the role of formal semantics, he thinks a soundness proof can explain how the classical proof rules are “justified”.

Thus the standard two-valued semantics for classical logic involves a conception under which to grasp the meaning of a sentence is to apprehend the conditions under which it is, or is not, true. If this is a correct general model for the meaning of any sentence of our language, then the sentential operators and the quantifiers can also be explained in accordance with this model, and the rules of inference governing them which are embodied in classical logic can be justified by reference to that representation of the meanings of the logical constants. The significance of a soundness or completeness proof, in terms of the two-valued semantics, for some systematisation of logic depends therefore, upon a thesis which does not belong to logic, and cannot be tested by it, but belongs, instead, to the theory of meaning: the thesis that the correct representation of meaning for expressions of our language is one given in terms of the truth-conditions of sentences. (Dummett 1973: 305)

Given these background assumptions, a soundness proof for classical logic can have philosophical relevance in the following sense. It tells you that the classical rules are classically truth preserving. The idea is that if we have an independent reason for thinking that the classical truth conditions capture the meaning of logical connectives in our language, then the soundness proof

will explain how the classical proof rules are in accordance with what the connectives mean.

It's worth noting that when Dummett talks about the rules being "justified", he isn't saying that a soundness proof would persuade someone who doubts the legitimacy of the rules in the first place. He is saying that, at least for those who accept the legitimacy of the rules, a soundness proof can provide an explanation of how the classical rules accord with the meaning of the connectives in our language. He says, regarding the soundness proof, that

its importance lies in its providing for deductive inference what a theory of meaning must provide for every component of our practice in the use of our language, an understanding of the way it works: we seek, not merely a description of our practice, but a grasp of how it functions. A semantics in terms of which a given fragment of logical theory can be proved to be sound, and, if that is possible at all, complete, supplies an answer to the question: How must our language be conceived to work — what model must we have for the meanings of our sentences — if the practice of deductive inference in which we engage is to be justified? It is not, in general, that we are in doubt as to whether that practice is justified: but, so long as we are unable to explain what the justification is, we lack an understanding of how our language works, of what it is that we are doing when we reason. (Dummett 1973: 310)

This distinction between explanation and persuasiveness is important for understanding Dummett's view about circularity in soundness proofs. Soundness proofs will give an explanation of why the rules are acceptable because the soundness proof will show that the rules are truth preserving (given the background philosophical assumptions connecting meaning and truth). Anyone who doubts the legitimacy of the rules should be unpersuaded by the explanation (since the explanation uses those rules). But that shouldn't compromise the legitimacy of the explanation. As Dummett notes, the aim of an explanatory argument isn't (necessarily) to persuade someone of the conclusion. Sometimes the conclusion of an argument will be accepted beforehand, and the premise will be accepted because it explains the conclusion. Regarding the circularity that occurs in a soundness proof, Dummett says it might compromise an attempt to persuade a skeptic, but it won't compromise its explanatory significance.

The circularity that is alleged against any attempt to justify deduction, viz. to justify a whole system of deductive inference, is not of the usual kind. The validity of a particular form of inference is not a premiss for the semantic proof of its soundness; at worst, that form of inference is employed in the course of the proof. Now, clearly, a circularity of this form would be fatal if our task were to convince someone, who hesitates to accept inferences of this form, that it is in order to do so. But to conceive the problem of

justification in this way is to misrepresent the position that we are in. Our problem is not to persuade anyone, not even ourselves, to employ deductive arguments: it is to find a satisfactory explanation of the rôle of such arguments in our use of language. (Dummett 1973: 295)

How should we think of the circularity that occurs in a soundness proof given these background assumptions? The circularity would need to be understood in a specific sense because on this understanding of the soundness proof, the soundness proof is understood as something that would enable us to “demonstrate that the primitive rules of inference are in fact valid”. The soundness proof couldn't be characterized as having such a strong conclusion if it were understood as a merely technical result. But given this background view where the soundness proof is understood as having a philosophical significance, we would have a form of circularity where the soundness proof demonstrates modus ponens to be *in fact* truth preserving while simultaneously relying on the use of modus ponens as a rule.

Let's grant that this kind of circularity can leave us with an argument that isn't convincing to a skeptic, but is nonetheless explanatorily legitimate. Should we think the regress for logical pluralism is based on this kind of circularity? If it is, it's not obvious why logical pluralists would have any problem explaining classical validity.

We should not think that the regress for logical pluralism is based on this form of circularity.<sup>23</sup> The form of circularity at issue is based on Dummett's philosophical views about the significance of soundness proofs. None of those views amount to an assumption that there is no fact of that matter about whether DS is really valid. But this is exactly the assumption that drives the regress in the case of Beall and Restall's logical pluralism. So even if we think about soundness proofs in

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<sup>23</sup>Perhaps there are forms of circularity in a soundness proof that someone could cite in order to try to develop a vicious regress. There are certainly ways to do this if you add enough auxiliary hypotheses. For example, suppose the soundness proof was cited in a person's story about how they acquired justification for believing that MP is a truth preserving rule. If that were the case, then they would presumably need to be justified in their reliance on the rules that are operative in the proof (and that includes their reliance on the rule MP). If we supposed, that in order to be justified in using MP as a rule, we needed to have an antecedent justified belief that MP is truth-preserving, then we would have a problematic form of circularity that could be cited as the basis for a regress. Roughly the idea would be that if each justified belief is based on a justified inference and each justified inference is based on an antecedent justified belief, then we can describe a chain that has no end point. But this is not the kind of regress that is supposed to be a problem for Beall and Restall's style of pluralism. This epistemic regress can easily be blocked by denying one of the auxiliary hypotheses. For example, someone might reject the idea that justified inference by MP requires an antecedent justified belief that MP is truth preserving. The regress argument for BR cannot be blocked in this way, and one thing that is distinctive about it is that it is specifically based on assumptions that are part of the pluralist view point (in particular, the assumption that there is no fact of the matter about whether DS is really valid).

the way that Dummett does, the circularity in a soundness proof cannot be what drives the regress in 4.2.1. There is another way of seeing the same point. Suppose we take on the background assumptions in Dummett so that a classical soundness proof is seen as aiming to demonstrate that the classical rules are “in fact” valid.<sup>24</sup> The regress for Beall and Restall should be seen as based on something different because it does not result from a challenge for Beall and Restall to explain how a classical rule is *in fact* valid. The regress for Beall and Restall results from a challenge for Beall and Restall to explain how an argument is *classically* valid, i.e. how it is valid in a merely technical or model-theoretic sense.

So the regress argument is not based on the same kind of circularity that Dummett describes. It is based on the fact that, on Beall and Restall's view, there's no fact of the matter about whether (B) is really valid. That's not an assumption that anyone is committed to in a soundness proof (even if they attribute explanatory significance to it in the manner of Dummett).

#### 4.3.5 Why shouldn't we think that Monism faces an analogous regress?

Even if we grant that there is a vicious regress for Beall and Restall's style of logical pluralism, this wouldn't count as a consideration in favor of logical monism unless the monist can explain why (A) is classically valid without an analogous regress. So does monism face an analogous regress?

Someone who thinks that monism does face an analogous regress would typically have some example in mind. In that case, the right way to respond would be to look at the purported example, and say why it isn't relevantly analogous. For this reason, I'll look at an example of how someone might try to develop an analogous regress for logical monism. While I think this is the best strategy for responding to this type of objection, it has the unfortunate consequence that the reader may feel as though they could come up with a better example to make the point. So I'll also provide some reasons for thinking that it shouldn't be easy to tweak the example so as to turn it into a problem for the monist.

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<sup>24</sup>See the passage cited from p. 291 for an example of where Dummett uses this language.

**Example of a purportedly analogous regress for Monism**

Suppose the monist is challenged to explain why (A) is classically valid. The monist might defend a model theoretic account of classical validity, and they might also cite (B) as an explanation. Parroting the language that was used when the regress was developed for logical pluralism, the pluralist could say that (B) appears to be an explanation of the right sort. And while the monist can say that (B) is really valid, the pluralist could challenge whether (B) is relevant on the grounds that the monist has no explanation of why (B) is really valid. It won't be helpful for the monist to appeal to a further argument to explain why (B) is really valid because the pluralist can ask for an explanation of why that further argument is really valid, and so on. So how is the monist not in an analogous position?

**Why the example doesn't present an analogous regress for Monism**

The purportedly analogous regress is not analogous. A key difference is that the monist has the option to cite the fact that (B) is really valid. Even if the monist treats this fact as an unexplained primitive, the pluralist doesn't have the option to cite this fact at all because it is part of their view that there is no fact of the matter about whether (B) is really valid. This is an important asymmetry because if you are asking whether (B) counts as a sufficient explanation, the fact that it is really valid is explanatorily relevant. If (B) is really valid, that is an explanatorily good thing. But if there is no fact of the matter about whether (B) is really valid, it's not clear how there could be a fact of the matter about whether (B) really works as an explanation.<sup>25</sup> Moreover, the assumption that there is no fact of the matter about whether (B) is really valid is what generates the vicious regress, and monists do not have to make this assumption.

There is another reason why the purported analogy fails. In the example, the monist says (B) is really valid. The pluralist says this will not be explanatorily relevant unless the monist can explain why (B) is really valid. But there are two problems with this. On the one hand, real validity may

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<sup>25</sup>It could be suggested that there is some kind of pluralism in the metaphysics of explanation. Perhaps there are classical explanations, intuitionistic explanations, etc. But this cuts no ice with regard to the issue at hand. The initial challenge starts out with a question about how classical validity is explained. But it would be of no help to say that we can give a classical but not a relevant explanation, because we can raise the same kind of question about the purported classical explanation. What explains why it's a classical explanation? A regress will be generated in the same way.

have explanatory relevance *even if* real validity cannot itself be explained. Zeus's anger may explain the lighting even if we can't explain Zeus's anger. On the other hand, in the regress argument, the stated problem for the pluralist was not that they could not give an explanation of something that was supposed to have explanatory relevance (e.g. the fact that (B) metaphysically necessarily preserves truth).<sup>26</sup> The problem was, that regardless of whether the pluralist can explain why (B) metaphysically necessarily preserves truth, the pluralist cannot account for why the fact that (B) metaphysically necessarily preserves truth is explanatorily relevant. The reasoning for this was based on the assumption that the important explanatory upshot of the fact that (B) metaphysically necessarily preserves truth can be unpacked in terms of a disjunction like (C1). But the truth of (C1) cannot be treated as explanatorily significant for the pluralist, because on their view, there is no fact of the matter about whether the argument (C) is really valid.

### **Why it's not clear how to create an analogous regress for Monism by tweaking the example**

In the previous example, the pluralist aimed to create an analogous regress for the monist by challenging the monist to explain why (A) is classically valid. The monist explanation was distinguished because the monist can (but the pluralist can't) say their explanation is really valid. But could the pluralist create an analogous regress by changing the initial challenge? For example, instead of initially asking whether a monist can explain why (A) is classically valid, couldn't the pluralist ask whether the monist can explain why (B) is *really* valid?

As far as I can see, it may be true that the monist will be set off on an infinite regress if they attempt to explain why (B) is really valid. But even if this is true, it doesn't provide an example of how logical monism results in an analogous regress. Even if the monist cannot explain real validity on pain of vicious regress, they can (unlike the pluralist) treat real validity as

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<sup>26</sup>Although it's worth noting that it also seems like there would be difficulty for a pluralist to give a possible-world theoretic explanation of metaphysically necessary truth preservation. We might construct the following argument:

1 There's a metaphysically possible world where (B) fails to preserve truth or (B) metaphysically necessarily preserves truth

2 It's not the case that there's a metaphysically possible world where (B) fails to preserve truth

3 So, (B) metaphysically necessarily preserves truth

For the pluralist, there won't be a fact of the matter about whether this argument is really valid. Only the monist can say it is really valid, so it seems like the monist is in a privileged position in regards to giving possible world theoretic explanations of why arguments like (B) metaphysically necessarily preserve truth.

an unexplained primitive.<sup>27</sup> For example, the monist can explain the classical validity of (A) in terms of an argument like (B) which is really valid. They may not be able to explain why (B) is really valid, but someone shouldn't be criticized merely on the grounds that they cannot explain a primitive in their ontology.

Of course it is open for the pluralist to treat classical validity as a primitive unexplainable fact, but classical validity isn't the kind of thing that should be treated as an unexplainable primitive. We should be able to explain classical validity in terms of facts about model theory, but that is just what the pluralist is said not to be able to do. If the monist treats real validity as an unexplained primitive, that's not obviously the kind of thing that needs an explanation in the first place. If explanations stop anywhere, or if some things are properly treated as outside the scope of possible explanation, then real validity (unlike classical validity) is the sort of thing that may plausibly be perceived as a candidate for being a place where explanations stop.

### 4.3.6 Haven't Beall and Restall already responded to arguments like this?

Beall and Restall have considered objections to their view, so it's worth saying something about why the regress argument shouldn't be subsumed under a category of objections that they have already addressed. I'll look at a few objections that they have already responded to, but which might be confused with the regress argument. I'll clarify in each case why the responses aren't applicable to the regress argument.

### Which logic governs your reasoning about the nature of logic?

Beall and Restall (1999) consider an objection that is premised on the idea that a single logic would have to govern an agent's reasoning (at least for the case of reasoning about logic itself). They describe the objection in the following way.

According to pluralism there are many different logics each of which specifies a genuine

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<sup>27</sup>To say something cannot be explained is not to say it isn't true or that it fails to be explanatorily relevant. It's worth recalling Passmore's discussion of the Platonic Form theory of predication. Perhaps predication cannot be explained in terms of something supposedly more basic (e.g. Platonic Forms). That doesn't mean that predication is an illusion. It only means that a theory (e.g. of Platonic Forms) cannot explain or enhance our understanding of predication.

consequence relation. But now there's a problem. Which of these many logics govern your reasoning about how many logics there really are? In other words, which logic ought to govern your reasoning about the nature of logic itself? And indeed, which logic ought to govern your reasoning concerning *that* question — the question of which logic ought to govern your reasoning about the nature of logic itself? (BR 1999: 6)

BR respond by questioning the premise that a single logic needs to govern a person's reasoning.

They say

though Logic and reasoning are intimately connected, we reject the view that all reasoning – or, perhaps, all *rational* reasoning, or etc. – corresponds to some logic. [...] Provided that some reasoning can be done that is, in some sense, independent of any logic, the objection does not seem to get off the ground. [...] Of course, it *may* be that, as the going objection presupposes, reasoning about Logic does indeed correspond to or “require” some given logic; however, until a strong case is made for this, we set the objection aside. (BR 1999: 6)

This response is based on the possibility that logic needn't correspond to reasoning (at least for the case of reasoning about logic itself). But this response isn't applicable to the regress argument. The regress argument concerns explanation rather than reasoning; it claims that the pluralist cannot treat (B) as explanatorily relevant.<sup>28</sup> The stated reason for why the pluralist cannot treat (B) as explanatorily relevant is not because they would need to reason in accordance with a single logic.<sup>29</sup> The problem is based on the logical pluralist's assumption that there is no fact of the matter about whether (B) is really valid. To clarify the point, we might suppose for the sake of argument that some reasoning can be done, as Beall and Restall say, “independent of any logic”. But whether or not we suppose this, (B) should still count as a suitable explanation of the fact that (A) is classically valid. And it will still be the case that there is no fact of the matter about whether (B) is really valid on Beall and Restall's view. If that is right, then Beall and Restall's view faces an explanatory difficulty that is separate from the issue of whether we can reason independently of a logic.

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<sup>28</sup>Various authors have either defended or resisted the claim that logical pluralism faces a “collapse” problem based on the normativity of logic. For example, see Stei (2017), Caret (2017), and Ferrari/Moruzzi (forthcoming). The worries surrounding the collapse problem are based on a role that logic supposedly plays in rational reasoning. But whatever may be said about regresses of epistemic justification (or regresses that somehow concern rational reasoning), there is a further issue about regresses of an explanatory nature. So even if the collapse problem can be avoided, or logic turns out not to be normative at all (see Harman (1986) and Russell (forthcoming)), the argument in the main text will still provide a hurdle for logical pluralists.

<sup>29</sup>The problem is also not predicated on the assumption that explanations need to accord with a single logic. (B) is valid in more than one logic that BR count as admissible.

**Which logic are you using now?**

Beall and Restall (2006) consider a similar objection that is based on the idea that their “fundamental theory” would presuppose a single logic.

You take all of your arguments to be valid, in some sense or other. *Which* sense might that be? In other words, your *fundamental* theory will presumably be based on a logic. Which logic? An answer to such questions seems to lead you to monism. (BR 2006: 99)

They interpret the objection as presupposing that their reasoning would have to be evaluated by a single logic. They say

We will take the current objection as a question about the validity or otherwise of our own reasoning. As anyone who *applies* formal logic knows, the fit between deductive validity and analysis of actual reasoning is not always an easy one [...] The pluralist claim is that, given a body of informal reasoning (that is, reasoning not produced in a particular *system* of logic), you can use different consequence relations in order to analyse the reasoning. As to *which* relation we wish our own reasoning to be evaluated by, we are happy to say: any and all (admissible) ones! (BR 2006: 99)

As I understand their point, their idea is that it is permissible to reason in accordance with any logic as long as it is admissible. But this response cannot count as a reply to the regress argument. As before, the regress argument concerns explanation rather than permissible reasoning. The stated reason for why Beall and Restall can't treat (B) as a suitable explanation was not that it is impermissible for a pluralist to reason in accordance with (B). The reason why a pluralist can't treat (B) as explanatorily relevant is because it's part of their view that there is no fact of the matter about whether (B) is really valid. The fact that it is permissible to reason from a set of premises to a conclusion does not in general imply that the premises explain the conclusion, so even if we grant that Beall and Restall can permissibly reason in accordance with (B) that does not imply that they are able to treat (B) as a suitable explanation of the fact that (A) is classically valid.

### Warrant objection

Beall and Restall (2006) also consider an objection based on the idea that there would need to be a single notion of warrant that corresponds to a unique consequence relation.

there is a single relation of preservation of warrant or entitlement, and that singles out a unique consequence relation. Take the premises of an argument. If one has warrant to believe those premises, is the conclusion warranted too? Even if there's an ambiguity in the notion of 'follows from' which can be made precise in a number of ways, there is a single relation which tracks the preservation of entitlement, and you can't be a pluralist about *that*. (BR 2006: 94)<sup>30</sup>

Their response invokes an example from Priest (unpublished manuscript).<sup>31</sup>

We often reason about some situation or other; call it  $s$ ; suppose that  $s$  is in different classes of situations, say,  $K1$  and  $K2$ . [...] Should one use the notion of validity appropriate for  $K1$  or for  $K2$ ? [...] Take some inference that is valid in  $K1$  but not  $K2$ ,  $\alpha \vdash \beta$ , and suppose that we know (or assume)  $\alpha$  (Priest as cited in Beall and Restall 2006: 93)

Beall and Restall say:

For concreteness, let us suppose that  $\beta$  is  $\gamma \vee \sim \gamma$  for some  $\gamma$  irrelevant to  $\alpha$ . Then, indeed, there is a situation in which  $\alpha$  is true but in which  $\gamma \vee \sim \gamma$  fails.

The inference from  $\alpha$  to  $\gamma \vee \sim \gamma$  is *valid* in the usual classical sense: if  $\alpha$  is true, then of necessity  $\gamma \vee \sim \gamma$  is true. There is no possibility (that is, no possible world) in which  $\alpha$  is true and in which  $\gamma \vee \sim \gamma$  fails. So, we are *classically entitled* to infer  $\gamma \vee \sim \gamma$  from  $\alpha$ . (BR 2006: 94)

But they also say

The argument is invalid; so, we are *not relevantly entitled* to infer  $\gamma \vee \sim \gamma$  from  $\alpha$ .

<sup>30</sup>Beall and Restall credit Gary Kemp and Stephen Read for raising this objection. There is a valuable discussion of these objections and replies in Read (2006). He criticizes Beall and Restall's response to a central objection from Priest, and also provides a useful account of the order of events regarding these objections and replies. For example, he notes that Beall and Restall cite Priest's objection in the form of an unpublished manuscript and that this is later revised in Priest's *Doubt Truth to be a Liar* (2006).

<sup>31</sup>The more recent version of Priest's argument can be found in *Doubt Truth to be a Liar* ch. 12. The example from Priest is based on an objection which is related to the one discussed in the main text. Priest says "We often reason about some situation or other; call it  $s$ ; suppose that  $s$  is in different classes of situations, say,  $K1$  and  $K2$ . Should one use the notion of validity appropriate for  $K1$  or for  $K2$ ? We cannot give the answer 'both' here. Take some inference that is valid in  $K1$  but not  $K2$ ,  $\alpha \vdash \beta$ , and suppose that we know (or assume)  $\alpha$ ; are we, or are we not entitled to accept  $\beta$ ? Either we are or we are not." Priest elaborates on the possibility that there might be no fact of the matter about whether we are entitled or not, but the main point from Priest is that there would be a problem if the notion of entitlement singled out one logic as privileged.

We conclude, then, that in an important sense, *whether we are entitled to infer*  $\beta$  is ambiguous; it has more than one answer. This is not a pluralism or a relativism about truth, since it is the plurality in the notion of *entitlement* that is doing the work. [...] As far as we can see, there is nothing in the notion of *entitlement* that will [...] serve to dictate a unique relation of deductive consequence that is properly favored for capturing entitlement. (BR 2006: 94)

As before, this response rejects the assumption that a single logic needs to be associated with warranted reasoning. But even if we grant that there are multiple notions of warranted reasoning, this can't be treated as a response to the regress argument. The regress argument is focused on explanation rather than warranted reasoning. The issue is not about whether a pluralist is licensed to reason in accordance with (B), but rather with whether (B) counts as a suitable explanation.<sup>32</sup>

#### 4.4 Contextualist articulations of Logical Pluralism?

Someone might wonder whether Beall and Restall's pluralism could avoid a vicious regress if it was articulated in terms of a semantic theory. There may even be some reason to think that Beall and Restall's theory could helpfully be understood in terms of a semantic theory because they describe their view in terms of ambiguity:

logical consequence is not arbitrarily ambiguous, but rather, it has a simple structure: a conclusion is a logical consequence of some premises if and only if that conclusion is true in every circumstance in which the premises are true. Different disambiguations of this notion arise from taking different extensions of the term "circumstance." (Restall 2002: 427)

There is nothing strange about ambiguity, so why not interpret admissible instances of the GTT as corresponding to different disambiguations of "valid"? This may be natural since features relevant for admissibility (e.g. necessity) are supposed to be connected to linguistic use. They say

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<sup>32</sup>It's easy to imagine an analogous response where it is said that nothing in the notion of explanation will single out a particular logic. I think someone could very well say this, but no matter how many notions of explanation there are (classical, relevant etc.), we can still raise an explanatory challenge that is wholly analogous to the one in the main text. The only difference is that it would be initiated with a challenge for the pluralist to explain why (B) should count as a classical explanation. We could try to explain why this is the case by setting up an argument by (DS), but it will still be a feature of Beall and Restall's view that there is no fact of the matter about whether this argument is really valid, and that compromises its status as a sufficient explanation. So it doesn't look like a pluralism about explanation will change the main issue, i.e. avoid a vicious regress.

their view

purports to incorporate the core features involved in the use of 'follows from' or 'logical consequence' (e.g. necessity, formality) (BR 2006: 28)

To reply to this criticism, it would be helpful to know more about how this non-arbitrary ambiguity is understood.<sup>33</sup> But I don't think Beall and Restall say much more about it (aside from points already discussed concerning admissible precisifications of the GTT). To respond, I will look at a way of thinking about Beall and Restall's view where this non-arbitrary ambiguity is articulated in terms of a specific semantic thesis. Then I will argue that a vicious regress still emerges. In particular, I will look at a view from Caret where ideas from Beall and Restall's pluralism are seen through the lens of an indexical contextualist semantic theory for the expression "valid". I don't think Beall and Restall endorse a form of indexical contextualism about "valid", but the point here is not exegetical. The point is just to show that the regress puzzle cannot be avoided merely by introducing a specific semantic theory.

#### 4.4.1 An indexical contextualist version of logical pluralism

Contextualist semantic theories employ the notion of a context of use to explain how the truth conditions of sentences can vary with features of the situations in which they are used.<sup>34</sup> Contextualism is natural for expressions like "I" where the extension varies systematically depending on who uses the expression. Formally, contexts of use can be modeled as n-tuples that encode information about situations where linguistic expressions are used.

Some versions of contextualism will also invoke a notion of content. This can be spelled out in different ways, but we can think of it as something that plays a role in determining the extension of an expression in a context of use. Since Caret's indexical contextualism uses a Kaplanian framework, it invokes notions of context, content, and character. A character can be understood formally as a function from contexts to contents. A context for Caret is modeled as an n-tuple that includes a placeholder for a deductive standard, i.e., an admissible class of cases in Beall and

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<sup>33</sup>It's worth noting Keefe (2014) who points out various ways that Beall and Restall's reference to imprecision might be understood.

<sup>34</sup>Two of the main historical sources for contextualist theories of linguistic expressions are Lewis (1980) and Kaplan (1989).

Restall's sense.

Every context selects for a deductive standard, i.e. an admissible class of cases. (Caret 2017: 753)

Caret thinks of the character of "valid" as being given by the GTT.

The character of the predicate 'valid' is given in the GTT. (Caret 2017: 753)

So Caret sees the GTT as something that takes the deductive standard of a context as an input and returns a content corresponding to a precisification of the GTT as an output. When discussing deductive standards, he says they give content to the expression "valid" as it occurs in a context of use.

Each context selects for a deductive standard and this, in turn, gives content to validity attributions in that context. (Caret 2017: 752)

In any given context, the content of 'valid' is the relation produced by instantiating the GTT on the deductive standard of that context (Caret 2017: 753)

This makes Caret's view a form of indexical contextualism about "valid" (i.e. a view where the content of "valid" shifts across contexts of use).<sup>35</sup> The framework allows for the formulation of a general statement of context dependent truth conditions for validity ascriptions. For example, recalling argument (A\*) from section 4.3.3, we can give a general statement of context dependent truth condition for a sentence like "(A\*) is valid":

(Gx) For all contexts  $c$ , "(A\*) is valid" is true in  $c$  iff [In every member of the class of admissible cases in  $c$  where the premises of (A\*) are true, so is the conclusion of (A\*)].

There are a lot of questions concerning universally quantified bi-conditionals like (Gx).<sup>36</sup> But with (Gx), we can build a model of the truth conditions for "(A\*) is valid" in a particular context. Suppose we were in a classical context of use (call it  $c_1$ ). If the character of "valid" is applied to  $c_1$ , it is supposed to return a content for "valid" that determines the class of classically valid

<sup>35</sup>He actually mentions non-indexical contextualist views where content doesn't shift across contexts of use and says "[a]s far as I can see, nothing much rides on this choice for present purposes, so I will continue to focus on the indexical approach". (Caret 2017: 753)

<sup>36</sup>For example, what kind of conditional will be used? How does the theoretical notion of truth in a context relate to truth itself?

arguments as an extension. So the truth conditions of “(A\*) is valid” in c1 can be given as:

(Gy) “(A\*) is valid” is true in c1 iff [In all Tarski models where the premises of (A\*) are true, so is the conclusion of (A\*)].

#### 4.4.2 An indexical contextualist version of logical pluralism will not avoid a vicious regress

On an indexical contextualist theory, “(A\*) is valid” won't be true independently of a context of use.<sup>37</sup> However, on the view, it is supposed to be the case that “(A\*) is valid” is true in a classical context, e.g. in a context like c1.<sup>38</sup> But can a contextualist explain why “(A\*) is valid” will count as true in a classical context? The indexical contextualist should be able to appeal to a bi-conditional like (Gy) to formulate an explanation because (Gy) is supposed to provide a model of what the truth conditions of “(A\*) is valid” are in a classical context like c1. That means that the indexical contextualist could regiment an explanation in terms of the following argument (G) where the first premise is just the left-to-right direction of (Gy).

(G1) If [In all Tarski models where the premises of (A\*) are true, so is the conclusion of (A\*)], then “(A\*) is valid” is true in c1.

(G2) In all Tarski models where the premises of (A\*) are true, so is the conclusion of (A\*).

(GC) So, “(A\*) is valid” is true in c1.

(G) may seem like a sufficient explanation of why “(A\*) is valid” is true in c1. But if there is no fact of the matter about whether (A\*) is really valid, then there won't be a fact of the matter about whether (G) is really valid either (because they are both just instances of MP). And if there is no fact of the matter about whether (G) is really valid, it's not clear how (G) would count as a sufficient explanation of why “(A\*) is valid” is true in c1.

Perhaps the contextualist could say that (G) is explanatorily relevant on the grounds that it

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<sup>37</sup>This is because “valid” doesn't have a content independently of a context of use.

<sup>38</sup>There is a small technical point here. Even expressions like “classical” admit of some vagueness in theoretical contexts. They can sometimes (but aren't typically) used in a way that includes certain supervaluationist and non-transitive logics (e.g. ones that drop certain “meta-rules” that are accepted in standard classical logics).

metaphysically necessarily preserves truth. But as before, it's not clear how facts about metaphysically necessary truth preservation can count as explanatorily relevant on a logical pluralist view. If (G) metaphysically necessarily preserves truth, then the important explanatory upshot of this is that (G) preserves truth at the actual world. In other words, the following will be true:

(H1) If the premises of (G) are true, then the conclusion of (G) is true

That would allow us to formulate the following argument (H):

(H1) If the premises of (G) are true, then the conclusion of (G) is true.

(H2) The premises of (G) are true.

(HC) So, the conclusion of (G) is true.

But if there is no fact of the matter about whether (G) is really valid, there won't be a fact of the matter about whether (H) is really valid either. And if there is no fact of the matter about whether (H) is really valid, then it's not clear why it would be explanatorily relevant that (G) metaphysically necessarily preserves truth. The important explanatory upshot of the fact that (G) metaphysically necessarily preserves truth is represented by the first premise of (H). But how can that be explanatorily significant if there is no fact of the matter about whether (H) is really valid?

Someone could suggest that it doesn't matter whether there is no fact of the matter about whether (H) is really valid because (H) is still nonetheless metaphysically necessarily truth-preserving. But this observation only leads to a continuation of the regress. The explanatory relevance of (H) metaphysically necessarily preserving truth is that in the actual world, if the premises of (H) are true, then the conclusion of (H) is true. This will set us up with another instance of MP, but the pluralist will not grant that the further instance of MP is really valid. And it should be easy to see that this reasoning can be continued indefinitely. So, it will not matter if an indexical contextualist theory is set up for the semantics of "valid". There will still be a regress that meets all the same conditions for viciousness that were previously described.

## Concluding Segment

I argued that Beall and Restall's version of logical pluralism faces a vicious infinite regress. I explained the nature of vicious regresses by reference to a view from Passmore. The argument was based on two steps. I argued that Beall and Restall's view results in an infinite regress, and then I argued that the regress is indeed vicious. I also provided responses to various objections. The objection and response section showed that there are many auxiliary hypotheses that are assumed in a vicious regress argument. More than a few of these assumptions were challenged. I considered multiple objections which denied the existence of a regress. I also considered objections that granted the existence of a regress, but denied that the regress was vicious. Issues that were previously considered in part one were shown to be relevant as well (in particular the issue of whether a regress is based on epistemic considerations). Lastly, I considered an objection regarding whether or not the regress argument might be avoided by interpreting Beall and Restall's view in terms of an indexical contextualist semantic theory about expressions like "logically valid". I discussed a view like this from Caret, and I argued that a vicious regress cannot be avoided in this way. This kind of issue about the relationship between versions of logical pluralism and semantic theories of expressions like "logically valid" will come up again in chapter six when Shapiro's logical pluralism is discussed.

## Chapter 5

# Field's Logical Pluralism

Hartry Field develops a version of logical pluralism that is based on an underlying relativism about validity attribution.<sup>1</sup> He gives voice to this idea in various essays, but very roughly, the general idea can be thought of in the following way. Suppose that validity attributions are understood as being somehow normative in nature.<sup>2</sup> And suppose further that a kind of relativism is assumed about normative claims (where some of them only have truth values relative to “policies” in a sense to be specified).<sup>3</sup> What this allows for is a relativistic version of logical pluralism that is based on an underlying relativism about normativity.

I will look at a few different sources from Field to get an understanding of this kind of pluralism about logic, but I will primarily focus on a couple of texts (2009a, 2015). I'd prefer that my goal not be understood as an attempt to criticize any specific view that Field held at one time or another. Rather, my goal is to criticize versions of logical pluralism that can be developed in terms of relativistic themes found in various discussions from Field. That being said, section 5.1 will be exegetical. In 5.1.1, I'll look at a relativistic framework that Field (2009a) applies to evaluative claims (where evaluative claims are characterized as being somehow not fully factual). This section will be organized into three subsections with boldfaced headings (indicating the feature

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<sup>1</sup>I'll be looking at a selection of passages from Field (2001, 2009a, 2009b, 2015).

<sup>2</sup>One of the main questions in what follows will concern what it would mean for a validity attribution to be “normative”. I'll discuss multiple ways that this idea can be articulated.

<sup>3</sup>Whatever the relativism ultimately amounts to, there is a question about its scope. Is it all normative claims that lack a policy independent truth value? Or is it only some of them?

of Field's view that is under discussion). In 5.1.2, I'll look at how Field (2015) combines a normative conception of validity attribution with a relativistic conception of normativity to develop a version of logical pluralism.

In section 5.2, I'll argue that a Lewis Carroll-style puzzle can be developed for versions of logical pluralism that are based on these relativistic themes in Field. I don't think that there is a single way of bringing together ideas in Field to develop a version of logical pluralism. So I'll divide 5.2 into two subsections where different concrete proposals are discussed.

In section 5.2.1, I'll look at a relativistic version of logical pluralism that is fully general in the sense that it would apply to all validity attributions. The idea would be that no validity attribution has a policy independent truth value (but that validity attributions do have policy-relative truth values). I don't claim that Field holds a view like this. But it would emerge from a view where (i) all validity attributions are normative and (ii) normative claims are understood in terms of the relativistic model described in Field (2009a). I'll argue that a view like this faces a Carroll-style puzzle regardless of whether policies are understood in propositional terms, non-propositional terms, or even purely dispositional terms. This section will also be divided into subsections which each discuss a different conception of policies.

In section 5.2.2, I'll look at a relativistic version of logical pluralism that is not fully general. Here the idea is that while some validity attributions may have policy independent truth values, at least some validity attributions will not have policy independent truth values. Field (2015) describes (but does not explicitly endorse) a view like this. He gives an example of how this might be spelled out by referring to rival proposals for how to resolve a version of Curry's paradox. One theorist may want to restrict modus ponens to resolve the paradox, and another theorist may want to restrict conditional proof. Must it be the case that at least one proposal is objectively wrong? Field says it may be that "the difference between the views is irreducibly a matter of normative policy". I'll look at this Curry's paradox example in detail and argue that a Lewis Carroll puzzle will still emerge in this restricted case.<sup>4</sup> At the end of section 5.2.2, I'll discuss a further related

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<sup>4</sup>I don't argue that a Carroll puzzle will emerge for every way of restricting a fully general relativism about logic. I'll focus on Field's example with modus ponens, but this kind of argument may need to be modified for (or not even apply to) alternative ways of restricting a fully general relativism about logical validity. For example, someone might argue that it is irreducibly a matter of normative policy whether we restrict excluded middle to resolve a version of the liar paradox. The argument isn't designed to apply to a version of logical pluralism like that; it's only designed

worry for restricted forms of relativism about logic.

In section 5.3, I will raise a separate puzzle for versions of logical pluralism that are based on the kind of relativism found in Field. Suppose that certain validity attributions only have a policy-relative truth value. And suppose we give an account of what it is to follow a policy in terms of an agent having certain dispositions.<sup>5</sup> Then there will be an underdetermination issue. In order for a validity attribution to get a truth value relative to the logical policies that an agent actually follows, facts about an agent's dispositions will have to determine facts about which logical policies they follow. But can an agent's dispositions determine any facts about what policies they follow? The locus classicus for a negative answer to this question is Kripke's "Wittgenstein on Rules and Private Language".<sup>6</sup> There Kripke develops a criticism against the view that dispositional facts could determine facts about what rules an agent follows. The natural response for a disposition theorist would be to reject the points in Kripke's Wittgenstein. A standard complaint among dispositionalists is that Kripke is only considering overly simplistic versions of dispositionalism about rule following. The idea would be that if we add certain bells and whistles (e.g. if we appeal to things like normal situations or idealizations of an agent's dispositions), the problems in Kripke's Wittgenstein can be avoided. For that reason, I'll look at two theoretically sophisticated versions of dispositionalism about rule-following that aim to respond to the points in Kripke. In section 5.3.1, I'll consider a view from Tomoji Shogenji (1993), and in section 5.3.2, I'll consider a view from Jared Warren (2018). In each case, I'll include an exegesis of their view, and provide reasons for thinking that they don't provide a satisfying response to the points in Kripke's Wittgenstein.<sup>7</sup>

That leaves us with a problem for dispositional views of rule following. But it also leaves us with

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to apply to the case described by Field concerning modus ponens and conditional proof. There may however be interesting differences between the Curry's paradox case and a disagreement over the liar. If one party thinks that a liar sentence is true and false, and another party rejects the claim that the liar sentence is true or false, their disagreement seems to be reducible to a factual one; or at the very least, their disagreement can be described in a way that doesn't directly involve the notion of validity. In any case, to the extent that the Carroll-style argument presented here is similar to Kripke's argument (in terms of its scope), it may not apply to non-basic logical principles. For example, it may only apply to principles like universal instantiation and modus ponens. (See the discussion in 3.5.2 and fn. 9 regarding the issue of whether Kripke's argument would apply in the case of non-basic logical principles.)

<sup>5</sup>Field (2001) describes a dispositional view of what it is to follow a policy, but he also suggests looking at Shogenji (1993) for an ingredient of how he might ultimately develop a dispositional theory of rule following. In the following, I'll focus discussion on Shogenji's view as well as a view from Warren (2018).

<sup>6</sup>It may also be that Quine's (1960b) "gavagai" considerations are an important precursor to the points in Kripke.

<sup>7</sup>The discussion of Shogenji will be divided into three subsections. The first is exegetical, and the second two are critical. The discussion of Warren is divided into eight subsections. The first three are exegetical and the rest are critical.

a problem for the relativistic thesis that validity attributions can get truth values relative to the logical policies that an agent follows (when following a policy is construed in purely dispositional terms).

## 5.1 Exegesis of Field

As noted, there are various places where Field describes a version of logical pluralism that is based on an underlying relativism about normativity. He sometimes describes his relativistic conception of normativity as one that is somehow “antirealist” or “expressivist”, but what would motivate an antirealist conception of normativity in the first place? There are many reasons why someone might be tempted by this sort of picture, and Field lists some of the standard motivations (and how they might apply for both “moral” and “epistemic” normativity).<sup>8</sup>

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*. (Field 2009b: 354)

Once a general form of relativism or antirealism is accepted in the case of epistemic normativity, a relativistic or antirealist conception of deductive validity can be developed. The idea is to treat deductive validity (or attributions of deductive validity) as somehow *normative*. And this is the way that Field's logical pluralism is generated. He says

[b]ut 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. (Field 2009b: 355)

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<sup>8</sup>For some of the most influential defenses of broadly antirealist views in the case of morality, see Ayer (1936), Harman (1975), and Mackie (1977).

While this passage indicates a view where the notion of logical implication has a normative component, it does not clarify what exactly it would mean for the notion to have a normative component. To understand more precisely how we might think about this, I'll follow a detailed explanation that Field gives in later work (2015). I'll look at this detailed explanation more closely in section 5.1.2, but I'll make a few preliminary comments about the idea here. Field (2015) characterizes the notion of logical implication as being somehow *primitive*. Part of what this means is that the notion isn't explicated in normative terms. What Field proposes instead is that the notion be explained in term of its conceptual role. Field gives an attitudinal characterization of the conceptual role of the notion by describing what it is for someone to take an argument to be logically valid. And Field says that part of what it is for someone to take an argument to be logically valid is to accept a certain constraint on belief. So the notion of logical implication is normative in the sense that its conceptual role involves the acceptance of certain normative constraints.

All of these details provide an initial set up for how a form of logical pluralism can be based in a relativism about normativity. But all of this has been described in a very cursory way so far. To understand what this kind of logical pluralism would look like in more detail, we have to understand the underlying form of relativism. For that reason, I'll look at a relativistic framework that Field (2009a) applies to evaluative claims. On Field's view, evaluative claims only have policy relative truth values, and he gives a detailed explanation of what it means for a claim to have a truth value relative to a policy.

### 5.1.1 Field's relativistic conception of evaluative claims

Sentences like "John's belief is justified" can be used to formulate claims about epistemic justification. Such claims, on Field's view, are evaluative; they characterize things in normative terms. Field develops a framework for evaluative claims where they are treated as having a not straightforwardly factual status.<sup>9</sup>

[R]egarding a belief as justified (or reasonable, or rational, or whatever) is evaluating it, and evaluations aren't straightforwardly factual (Field 2009a: 250)

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<sup>9</sup>Care has to be taken because Field's view is not a claim about what ordinary speakers mean. The view should be understood as a model for how epistemic language could function, regardless of whether it captures what ordinary speakers mean.

This nonfactual status is explained with a form of relativism and expressivism. Field says it is a

view of normativity that combines elements of relativism and expressivism, and applies it to normative concepts in epistemology. The result is a kind of epistemological anti-realism, which denies that epistemic norms can be (in any straightforward sense) correct or incorrect (Field 2009a: 249)

He says that evaluative claims are, in some way, relative to norms of assessment, where norms of assessment are understood in terms of preferences and policies.

[I]n some sense, evaluative claims involve a free parameter, for a norm of assessment (Field 2009a: 251)

I will take the norms to simply be the preferences and policies (Field 2009a: 259)

On the view, there is no norm-independent goodness.

the relativist [...] holds that there is no such thing as norm-independent goodness. (Field 2009a: 256)

When Field rejects the existence of norm-independent goodness, he is referring to the views of “epistemological realists” who claim to believe in a property of “metaphysical justification”. But he hedges on whether such a property even makes sense. He says

[u]sually they attribute this belief to the ordinary speaker, as “part of what the ordinary speaker means” by “justification” [...] I think that to the extent that sense can be given to this notion of metaphysical justification, there's no such thing. (Field 2009a: 253)

Another way he puts it is that his view

adopts the metaphysics of the error theorist. (Field 2009a: 253)

This implies that in a dispute over an evaluative claim (between people who agree on all the relevant facts), neither side will be “metaphysically” privileged over the other. Field says

[t]he same normative proposition can be true at the actual world relative to some norms but not others, so that different speakers who agree on all the relevant facts can still evaluate it in different ways by employing different norms in making their evaluations.

And none of their conflicting evaluations would be metaphysically privileged over the others. (Field 2009a: 252)

Field goes into more detail about all of these points, and I'll order the the rest of the discussion in this section in the following way. I'll first look at how Field understands preferences and policies. Then I'll look at how Field understands what it means for an evaluative claim to be relative to a norm. After that, I'll explain Field's view of what it would mean for neither side of a evaluative dispute to be metaphysically privileged over the other.

### **Field's understanding of preferences and policies**

As mentioned, Field identifies norms with preferences and policies. He explains preferences and policies in terms of the roles that they play.

A person can act or believe in accordance with a norm (or largely in accordance with it), or her acting and believing may in some sense be guided by the norm. Or she can make her evaluations in accordance with a norm (or largely in accordance with it), or in a way that is guided by the norm. Or she can be committed to acting or believing in accord with a norm (or have a high degree of commitment to doing so), or to evaluating in accordance with it. (Field 2009a: 260)

So norms do not play just one role. One may guide an agent's action, while another guides the agent's judgment. The expressivism is partly explained by a psychological role that norms are supposed to play in the production of normative judgment.

[A]n evaluative sentence expresses a mental state that is a resultant of norms and factual beliefs. (Field 2009a: 252)

The expressivism is not counted as "non-cognitivist" in that it is not just norms, but also factual beliefs that explain the production of normative judgment.

the view has [...] elements associated with expressivism (though not of the non-cognitivist variety in which "no proposition is expressed" or in which one's factual beliefs don't enter into normative evaluation). (Field 2009a: 252)

The reference to propositions should be understood in a specific way. For Field, an agent's acceptance of a normative proposition is explained in terms of their having certain factual beliefs alongside preferences or policies.

Acceptance of a propositional norm about what's good is to be explained in terms of one's preferences, or of the preferences that one approves of; and that acceptance of a propositional norm about what one ought to do is to be explained in terms of one's policies, or of the policies that one approves of. (Field 2009a: 259)

Field discusses the psychological roles of norms at various levels of idealization. For example, in the case of non-evaluative beliefs, actual agents do not see all the implications of their beliefs. Moreover, their beliefs are riddled with inconsistencies. Nonetheless, there can be value in modeling agents as having logically consistent and complete non-evaluative belief sets. Similar points hold for norms on Field's view. Agents may be committed to (or guided by) multiple inconsistent norms. (Set aside for the moment what it would mean for two norms to be inconsistent.) But there can still be value in modeling agents as normatively consistent. Field discusses norms at a high level of idealization in the following passage.

Let's imagine an argument about the reasonableness of a particular action or belief, among agents each of whom is committed to a single relevant precise norm. (Field 2009a: 271)

In other passages, Field discusses norms at lower levels of idealization.

To get a model for how debate about any deeply entrenched belief or norm proceeds, we should abandon the assumption implicitly made in Section 9, that agents are logically omniscient. Actual agents are far short of this: they do not see all the consequences of their beliefs, policies and preferences, and this leads not only to many failures of logical closure but also to many unrecognized inconsistencies. (Field 2009a: 280-281)

Field notes different formal techniques for describing norms at higher and lower levels of idealization, but I will reserve discussion of this to a footnote.<sup>10</sup> When idealizing, Field says consistency is a natural norm attribution constraint.

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<sup>10</sup>Field's discussion focuses on how norms (in conjunction with factual beliefs) can produce mental states that are expressed by normative sentences. Factual (non-normative) beliefs can be modeled at lower or higher degrees of idealization. At a high degree of idealization (alongside a degree of belief conception) we can think of the degrees of factual belief in terms of a probability function  $f$  assigning real numbers from 0-1 to factual propositions. An evaluative norm can be seen as a probability function  $f^*$  that extends  $f$  to the normative propositions. This is supposed to capture a sense in which our normative attitudes are a result of both factual beliefs and norms. If I have a degree of belief  $x$  that I am in a situation satisfying property  $P$ , and I have a precise policy guiding my self-evaluation that mandates believing  $Q$  in situations satisfying property  $P$ , then we can extend  $f$  to an  $f^*$  so that a value is assigned to the proposition that I ought to believe  $Q$  (where the value ideally matches  $x$ ). Similar points apply when thinking at levels of less idealization. Factual beliefs can be modeled with an  $f'$  which is a set of probability functions over factual propositions, and an evaluative norm can be modeled with an  $f'^*$  which is a set of norms that extend the set of probability functions to normative propositions.

it is at least somewhat natural to suppose that the process of idealization imposes consistency, so that we can't reasonably suppose that the high-level rules governing a person's epistemic behavior are inconsistent. (Field 2009a: 283)

He also says that there is vagueness in the constraints on norm attribution that will lead to a kind of indeterminacy.

It seems reasonable to regard someone as "following" a rule when (i) the person's behavior by and large accords with the rule, and there is reason to expect that this would continue under a decent range of other circumstances; and (ii) the person tends to positively assess behavior that accords with the rule and to negatively assess behavior that violates the rule. [...] This is vague, which reinforces the point next to be made, about the considerable indeterminacy involved in ascribing epistemic or other rules to a person. (Field 2009a: 283)

The indeterminacy is supposed to imply that there is no uniquely reasonable characterization of an agent at the higher level of idealization.

This idealized description needn't be unique: one reason for this is that the doxastic core is inconsistent, and there may be more than one equally good way of getting an "approximate fit" by a consistent norm. (Field 2009a: 285)

Given the explanation of what norms are, two things still have to be explained: the precise sense in which epistemically evaluative claims are relative to norms, and what it would mean for neither side of dispute over an epistemically evaluative claim to be metaphysically privileged over the other.

### **What it is for evaluative claims to be relative to norms**

Field's understanding of relativism is developed in the context of giving a semantics and pragmatics for evaluative claims. Field says his view seems to be a notational variant of Alan Gibbard's view on evaluative claims.

In fact, the view seems to be a notational variant of Allan Gibbard's view (1990, Chapter 5) that evaluative claims express propositions in an extended sense: not just sets of possible worlds, but sets of norm-world pairs. If an evaluative claim A expresses an extended proposition consisting of a set of norm-world pairs [...], then A is something that can be true at a world  $w$  relative to a norm  $n$ . (Field 2009a: 252)

While worlds may determine truth values for factual propositions, a world plus a norm is needed to determine a truth value for evaluative claims.

“worlds” determine the truth values only of non-evaluative claims; because “normative facts” aren’t included in the worlds, then a world plus a norm is required in order to generate truth values for evaluative claims. (Field 2009a: 269)

These passages do not explain how norms determine truth values for evaluative claims. And they don’t unpack expressions like “A is true relative to a world norm pair” in further detail. Field wants to explicate these explicitly relativized expressions in non-evaluative terms, and to do this, he introduces a notion of compatibility.

At least for norms that are policies, the idea of an action, belief, etc. being reasonable-relative-to-a-policy admits an obvious explication in non-normative terms: it means that acting, believing etc. in the manner in question, given the circumstances in which the agent finds herself, is compatible with the policy. (Field 2009a: 261)

He says the non-evaluative notion of compatibility can be unpacked in model-theoretic or syntactic terms.

If you think that compatibility is itself normative, you can replace it by compatibility in a given system; this can be explained in syntactic or model-theoretic terms, which certainly aren’t normative. (Field 2009a: 261)

This is noteworthy because it would mean that there is a difference between evaluative claims and their explicitly relativized variants. The explicitly relativized variants will not themselves be evaluative.

### **What it would mean for neither side of a normative dispute to be metaphysically privileged over the other**

To explain Field’s view about what it would mean for neither side of an evaluative disagreement to be metaphysically privileged over the other, we have to relate the previous points about normative propositions having norm-relative truth values to Field’s view on the pragmatics of normative claims. Field’s pragmatics involves the description of a practice where, even when agents are alike in their factual beliefs, they may still evaluate a normative claim differently depending on whether

they have different norms influencing the way they evaluate normative claims.

The same normative proposition can be true at the actual world relative to some norms but not others, so that different speakers who agree on all the relevant facts can still evaluate it in different ways by employing different norms in making their evaluations. (Field 2009a: 252)

[T]he utterer or assessor uses her own norms to decide what to accept (Field 2009a: 274)

This might sound trivial, but it can be distinguished from a practice where agents would treat an evaluative assertion as true when it was true relative to a norm they thought the assertor was committed to.<sup>11</sup>

So when Field claims that neither side is metaphysically privileged in certain disputes over normative propositions (namely those disputes where all relevant facts are agreed on), should this claim be identified with the pragmatics point about how agents evaluate by their own standards when evaluating normative propositions? I think the answer is no. Even if Field's pragmatics characterized our actual practice, there might still be a norm-independent justification property that the relativist has to reject. The relativist is supposed to adopt the metaphysics of the error theorist, and the error theorist rejects norm independent justification properties. So when Field claims that neither side is metaphysically privileged in these kinds of disputes, this should be explained in terms of the error theorist's metaphysics. Because if the error theorist metaphysics were false, one person's evaluation could be privileged over the other's.

It is helpful to distinguish the "no privilege" claim from a mischaracterization of it (where it is presumed to imply that all norms are equally good). Field explicitly rejects this, and says that his:

view is not "committed to the idea that all norms are equally good." (Field 2009a: 255)

Field would only accept this claim if "good" is being understood in what he calls a perverse norm-independent sense.

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<sup>11</sup>Compare this alternative pragmatics to a pragmatics for evaluating weather reports: agents evaluate a claim that it's raining as true based on whether they think it's raining at a location the reporter has in mind.

Perhaps the person who says that relativism declares all norms equally good is defining “equally good” in a special way, to mean “have a norm-independent goodness to equal degree”? But in that perverse sense of ‘equally good’, the relativist will declare everything equally good, since he holds that there is no such thing as norm-independent goodness. (Field 2009a: 256)

The non-perverse sense of “good” is given by the relativistic semantics and pragmatics. So why are we not committed to the claim that all norms are equally good in the non-perverse sense? Part of the explanation concerns a point about norms that people would take seriously. No norm that anyone would take seriously will imply that all norms are equally good.

[N]o system of norms that anyone would take seriously will imply that all epistemic norms are equally good. (Field 2009a: 258)

So, it is in the following sense that Field's view is not committed to the claim that all norms are equally good. Assuming we have norms that aren't totally bizarre, and assuming we are understanding “good” in the non-perverse sense, we won't evaluate “all norms are equally good” as true. Does this mean it is left open on Field's relativism whether all norms are equally good? Could there be an agent with very bizarre evaluative policies, that held all norms to be equally good? I'm not sure whether there could be such an agent. But assuming there could be, if we had a dispute with them about whether all norms were equally good, Field's relativism wouldn't allow a norm-independent property of goodness to privilege either side of the dispute.

It would also be wrong to characterize Field's “no privilege” claim as the view that there is no best norm. Field does happen to think that there is no best norm.

[A]n evaluator needn't accept, and probably shouldn't accept, that any one norm is best (Field 2009a: 268)

But he distinguishes between there being no privileged norm and there being no best norm. He clarifies this by pointing out how someone might make a distinction like this in Special Relativity.

There might be some excuse for taking relativism to be committed to the claim that among norms of the same scope, there is none that is uniquely best. [...] I don't actually think that this is correct. (An advocate of Special Relativity might argue that a frame of reference in which the center of mass of the universe is at rest is in some sense best,

while at the same time insisting that it is not objectively correct in the way Newtonian or Lorentzian mechanics demands.) (Field 2009a: 256)

Also, whether there is a uniquely best norm is not straightforwardly factual.

the question of whether there is a uniquely best norm isn't straightforwardly factual, since it will be true or false only relative to a norm of goodness. (Field 2009a: 257)

This further supports the distinction because the question about whether there is no privileged norm is straightforwardly factual. It's not supposed to be a norm-relative matter whether an error theorist's metaphysics is correct.

### 5.1.2 Field's normative picture of validity attribution

Now that I have set out some of the details of the relativistic conception of evaluative claims in Field (2009a), I want to look at how this framework might be applied in a specific case. In particular, I want to look at how it might be applied to validity attribution. I should clarify that I'm not assuming that the framework in Field (2009a) was intended to be applicable to the case of validity attribution. The main examples in Field (2009a) are claims about epistemic justification. And while Field treats the notion of validity as being *in some sense* related to epistemic norms, he doesn't treat the notion of validity as something that can be explicated in normative terms. Nonetheless, he describes a form of logical pluralism that is based on a form of relativism about normativity (where logic is described as being normative in some form or fashion). This was indicated in the previously noted passage from Field (2009b: 355), and Field (2015) goes into more detail about a sense of normativity that is supposed to be directly applicable to the case of validity attribution. At this point, I'll explain the sense of normativity that is supposed to hold for validity attribution in Field (2015), but I will also treat the apparatus in (2009a) as a guiding idea for how we might think of the form of relativism that underlies the kind of logical pluralism that Field describes.

Field (2015) articulates a precise sense in which logic is normative. He says the notion of validity should be seen as a *primitive* notion. In other words, the idea is that the notion shouldn't be seen as something that is definable in terms of more basic notions. In particular, Field says it cannot be defined in terms of notions of necessary truth preservation or in terms of normative

notions. Instead of explicating the notion by defining it in more basic terms, the idea is that it can be explained in terms of its conceptual role (where the conceptual role is characterized in terms of constraints on degrees of belief).

The view I've been advocating has it that instead of trying to define validity in other terms, as the necessary truth-preservation account does, we should take it as a primitive, and explain its conceptual role in terms of how it constrains our (conditional) beliefs.

We should contrast this with another proposal: that we define validity, but in normative terms, in a way that reflects the connection between validity and belief. That alternative proposal is that we define  $A_1, \dots, A_n \Rightarrow B$  as "One shouldn't (in the non-subjective sense) fully believe  $A_1, \dots, A_n$  without fully believing B"; [...] I think it would sully the purity of logic to define validity in normative terms whose exact content is less than clear. (Field 2015: 54)

When Field describes the conceptual role of validity, he gives an attitudinal characterization where epistemic constraints are referred to in the description of what it is to take an argument (or inference) to be valid.

(VB)a To regard an inference or argument as valid is (in large part anyway) to accept a constraint on belief: one that prohibits fully believing its premises without fully believing its conclusion. (Field 2015: 42)

So this provides us with a sense in which logic is normative albeit not one where the notion of validity is seen as equivalent in meaning to a normative notion. Field gives voice to the idea that validity is normative in the following passage.

The underlying idea here is that a disagreement about validity (insofar as it isn't merely verbal) is a disagreement about what constraints to impose on one's belief system. (Field 2015: 42)

But passages like this have to be understood carefully. In the passage, Field says disagreements about validity are disagreements about what epistemic constraints to impose. But this shouldn't be understood as an endorsement of the view that a disagreement about validity is a disagreement over a normative proposition.

If we applied the framework outlined in Field (2009a) to validity attribution, this would mean validity attributions have a not-straightforwardly factual status. The form of logical pluralism described in Field (2015) also refers to a kind of non-factual status that validity attributions might

have. But Field also introduces a notion of objectivity and gives sufficient conditions for a validity attribution to be objectively wrong.<sup>12</sup>

To explain the sense of objectivity he has in mind, Field first discusses a notion of objectivity related to attributions of chance, and then he analogizes this to the case of validity attribution.

In the case of chance, it does not seem unreasonable to stipulate that any attribution of chance that departs drastically from actual stable frequencies is “objectively wrong” (Field 2015: 60)<sup>13</sup>

Just as relative frequency can play a role in constraining attributions of chance, Field thinks that truth preservation can play a role in constraining validity attribution.

The role that frequencies play for chance can be played by truth preservation for validity. Just as an attribution of chance that departs drastically from actual frequencies seems objectively wrong, an attribution of validity to an argument that definitely fails to preserve truth seems objectively wrong. (Field 2015: 60)

Field thinks that this suffices to make most disputes about validity objective.

Prima facie, this suffices for making most disputes about validity objective (Field 2015: 60)

But Field thinks there may still be room for a degree of non-objectivity in disputes about validity.

the impossibility of an actual reduction of validity to logically necessary truth preservation is a sign that there may be some degree of non-objectivity in the choice of logic (Field 2015: 60)

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<sup>12</sup>If some validity attributions are objectively wrong, then some features of the framework in Field (2009a) will not be applicable to validity attribution. That framework treats evaluative claims as systematically non-factual, so there wouldn't be any room for objective truth values. According to the (2009a) framework, even if a disagreement over moral obligation or epistemic justification is based on an underlying factual disagreement, it's still not going to be the case that one side of the dispute is objectively wrong (because on Field's view there isn't a norm independent property of moral or epistemic goodness). The view of validity attribution in Field (2015) is different. There, if a dispute about validity is based on an underlying factual disagreement (say about truth preservation), then one side of the dispute can be objectively wrong. One side can be objectively wrong even if there isn't a norm-independent property of epistemic goodness.

<sup>13</sup>Field says he uses quotes here because “there's a question as to whether this rather vague term is fully appropriate. And my view is that the ‘level of objectivity’ we get is greater in some cases than in others. If I differ with someone on the chance of rain tomorrow, it's very likely that this difference is largely due to differences in our information (information expressible without the term ‘chance’); to the extent that this is so, there is certainly no threat to objectivity.”(2015: 60)

If this were right, there would be a way of dividing validity attributions into two classes: the ones with objective truth values and the ones without objective truth values.

Field provides an example of a case where he thinks a dispute about validity may not be resolvable by any “objective” considerations. He thinks that this may occur in disputes over Curry's paradox regarding the validity of modus ponens (although he is careful to clarify that he is unsure whether this is actually the case).

It may be, for instance, that a view that locates the failure of the Curry argument in modus ponens and a view that locates it in conditional proof can't be distinguished in terms of how closely validity corresponds to truth preservation. I don't say that this is the actual situation, but suppose it is. In that case, the difference between the views is irreducibly a matter of normative policy. The proponent of unrestricted modus ponens will say that we ought to conform our degrees of belief to it, in the sense I've described, and the proponent of unrestricted conditional proof will say that we ought to conform our degrees of belief to a different standard. And each will take their 'oughts' to be non-subjective, in the sense that they aren't merely claims about what we ought to do given our logical theory. (Field 2015: 61)

If there really was no objective truth in disputes over the validity of MP, then I think the best way to understand Field's view would be to say that some validity attributions only get truth values relative to policies for constraining degrees of belief. But there would still be a question about how to understand what it means for a dispute to be irreducibly a matter of normative policy, and what it would mean for a validity attribution to be true relative to a policy for constraining degrees of belief. Even though the framework from Field (2009a) may not have been intended as a model for validity attribution, we can appeal to that framework as a guiding idea for how to think about disputes about validity that are irreducibly a matter of normative policy. Recall that the framework provides an analysis of certain kinds of explicitly relativized statements in non-normative terms. For example, Field says

[a]t least for norms that are policies, the idea of an action, belief, etc. being reasonable-relative-to-a-policy admits an obvious explication in non-normative terms: it means that acting, believing etc. in the manner in question, given the circumstances in which the agent finds herself, is compatible with the policy. (Field 2009a: 261)

To think about how this would work in the case at hand, consider an explicitly relativized statement of the following form.

(A) action  $t$  is reasonable relative to policy  $v$ .

In the analysis, a claim like (A) gets explicated in terms of a claim that contains the word “compatible” but no longer includes the word “relative” or “reasonable”, as in the following.

(B) action  $t$  is compatible with policy  $v$ .

But it's not obvious how to apply this analysis to the explicitly relativized claims at issue. The explicitly relativized claims at issue are of the following form.

(C)  $P$  logically implies  $Q$  relative to policy  $u$ .<sup>14</sup>

Claims about logical implication do not on the surface concern anything about an action or belief being reasonable, and Field's (2015) view is that the notion of validity can't be explicated in normative terms. Nonetheless, we can think of the truth conditions of a claim like (C) as being explicated in terms of the constraints that would be expressed by (or otherwise figure into the conceptual role of) validity attributions on Field's view. So we might see (C) as true just in case (D) holds.

(D) degrees of belief  $n$  for  $P$  and  $m$  for  $Q$  meeting condition  $X$  are compatible with policy  $u$  (where  $u$  is a policy for constraining degrees of belief).<sup>15</sup>

As before, in order to address the worry that the notion of compatibility is evaluative, it can be thought of in terms of some particular model theory.

## 5.2 A Lewis Carroll-style Puzzle for Field's version of Logical Pluralism

In the following section, I'll make an argument against versions of logical pluralism that are based on the relativistic themes in Field. The idea is that these forms of logical pluralism are susceptible

<sup>14</sup>I'm simplifying here by considering only single premise arguments.

<sup>15</sup>The condition  $X$  might be something like:  $m$  is greater than or equal to  $n$ , and certain modifications can be made to incorporate the generality associated with logical implication. For example, we might talk about pairs of degrees of belief  $z$  and  $w$  meeting condition  $X$  for all pairs of propositions  $\Phi$  and  $\Psi$  (where  $\Phi$  is of the form  $A$  and  $\Psi$  is of the form  $B$ ).

to a problem that is reminiscent of the regress in Lewis Carroll's dialogue.<sup>16</sup> But I'll organize the discussion in a specific way because the details of the argument depend on how general the form of relativism about logic is. As noted previously, Field (2015) discusses a form of relativism about logic that is not fully general, but I will first raise an issue for a fully general form of relativism about logic. Then I will go on to see whether similar points apply to a more restricted form of relativism.

### 5.2.1 A fully general relativism about logic

Consider the following passage where Field (2009a) says that no evaluative claim has a norm independent truth value.

“worlds” determine the truth values only of non-evaluative claims; because “normative facts” aren't included in the worlds, then a world plus a norm is required in order to generate truth values for evaluative claims. (Field 2009a: 269)

If validity attributions were counted as evaluative in the relevant sense, then it would follow that no validity attribution has a norm independent truth value. This would amount to a form of relativism about logic that is fully general in the sense that it would apply to all validity attributions. This would be different from the form of logical relativism described in Field (2015) where some validity attributions can be objectively wrong, but for now, I just want to focus on a fully general version of relativism about validity attribution in order to argue that it results in a Carroll-style worry.

The problem I want to raise is based on the idea that there is a kind of tension in a fully general relativistic version of logical pluralism. It's part of the view that validity attributions won't by themselves have truth values. But it's also part of the view that validity attributions are

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<sup>16</sup>In what follows, I'm not going to place too much emphasis on the question of whether the puzzle I'm raising for Field is really “of the same type” as the problem in Carroll's dialogue. This is not to say that the question isn't important, but a key reason why it might matter whether one puzzle is of the same type as another is that this can provide insight into how and where we might find common assumptions in (or solutions to) both. I won't be ignoring this latter issue, and I will note various assumptions in the argument I'm making and I'll highlight ways the reasoning can be challenged. I'll also note some points of similarity between the puzzle I'm raising for Field's view and the regress in Carroll's dialogue. There may be one straightforward point of dissimilarity. The points in Carroll's dialogue seem to have something to do with inference or justified inference. I don't really think the argument that I'm making against Field's view has anything much to do with inference or justified inference. Even though his relativism is developed in the context of him giving an account of epistemically evaluative claims, I think the argument that I am making is directed towards the relativistic assumptions in Field (rather than anything that has to do with epistemic justification per se). If this is right, then it seems like a point of dissimilarity with Carroll's dialogue because Carroll's dialogue doesn't seem to concern anything involving relativism (whether about epistemic justification or otherwise).

supposed to have truth values *relative to policies*.<sup>17</sup>

So there is a kind of asymmetry between validity attributions and their explicitly relativized counterparts (the former won't have truth values independently of a policy). But I'll argue that this asymmetry cannot be maintained. The details of the argument depend on how policies are understood, so for that reason, I'll first make the argument against a form of relativism that understands policies in terms of propositions (or truth-evaluable entities). After that, I'll go on to argue that the problem will not disappear when policies are understood in non-propositional terms or even purely dispositional terms.

### The propositional construal of policies

On a fully general relativism about logic, no argument is valid independently of a policy, and no premise by itself will imply its conclusion. That means that in the following argument (D), (D1) will not by itself imply (DC).

(D1) Every dish is washed

(DC) This dish is washed

But if (D1) does not by itself imply (DC), it would be arbitrary to say (D1) implied (DC) when relativized to a policy. Policies, on the view under consideration, are propositions (or truth evaluable entities). If policies are being understood in this way, then we might characterize a universal instantiation policy in terms of the following proposition (UI-prop):

(UI-prop) All universal claims imply each of their instances.

But this results in a problem. If (D1) didn't imply anything by itself, then the proposition (UI-prop) wouldn't imply anything by itself either (because (D1) and (UI-prop) are both just

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<sup>17</sup>This is part of the view at least if we are applying the Gibbard-style framework from Field (2009a). Field (2009a) says "worlds" determine the truth values only of non-evaluative claims; because "normative facts" aren't included in the worlds, then a world plus a norm is required in order to generate truth values for evaluative claims". If this were applied to validity attribution, it would mean a world is not enough for a validity attribution to get a truth value (a world plus a norm would be needed). But the explicitly relativized claims aren't supposed to be evaluative (since they are analyzed in terms of a model-theoretic or syntactic notion of compatibility). (2009a:261) So there is no guarantee that worlds will fail to determine truth values for the explicitly relativized claims. A stronger form of relativism about logic can be envisioned where even the explicitly relativized claims lack norm independent truth values, but I won't consider this view here.

universal claims). And if (UI-prop) doesn't imply anything by itself, then (D1) isn't going to imply anything relative to (UI-prop) either. In other words, if there aren't facts about what (D1) simply implies (independently of a policy), there won't be facts about what (D1) implies relative to a policy either.<sup>18</sup>

### Non-propositional characterizations of policies

What about non-propositional characterizations of policies? Field in particular does not think of policies in terms of propositions, and he describes policies as being something like commands.

Policies are sometimes stated in normative language ("You shouldn't believe a conjunction without believing the conjuncts"), but here the normative claims are generated by the policy: in the example, the policy is something like an imperative ("Don't believe a conjunction without believing the conjuncts"), and the "shouldn't" formulation just means that if you act in the way suggested you are violating the policy (Field 2009a: 259)

This is interesting because for any logical principle, we can think of a command that might be treated as an imperatival analogue of the principle. In the above passage, Field discusses an imperatival analogue of a conjunction elimination principle, but we can also do the same for a universal instantiation principle.<sup>19</sup> For example, we might describe this in terms of the following (UI-com):

(UI-com) Close your beliefs under all relevant instances of UI.<sup>20</sup>

This provides us with a conception of policies where they are not understood as propositional (or truth-evaluable) entities. But even if commands are not truth-evaluable, this won't change the

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<sup>18</sup>The point here is not developed in terms of a regress, but there is still something going on here that is clearly analogous to the dialogue in Carroll. According to the relativistic conception of logic, a validity attribution by itself isn't sufficient to determine a truth value. In the same way, the tortoise thinks that the premises *A* and *B* aren't sufficient by themselves to logically force him to accept the conclusion. According to the relativistic conception of logic, once you have a policy, you will be able to get a truth value for a validity attribution. In the same way, the tortoise talks *as though* the logical force will step in once another premise is granted. In both cases, there is a kind of instability. Once it has been presumed that the first stage is insufficient (for a truth value or logical force or what have you), the second stage isn't going to be sufficient either.

<sup>19</sup>This is like the example from the discussion of Kripke in ch. 3.

<sup>20</sup>I say "relevant" instances of UI because of computational limitations and practical considerations. Since there are infinitely many universal claims, and each has infinitely many instances, only some relevant subset of UI instances will matter for meeting the requirements of the command at issue. This is related to issues about norms that would purportedly be based in logical consequence. For examples of how such norms might be formulated, see MacFarlane (2004), Field (2009c), and Sainsbury (2002)

main point.<sup>21</sup> To see this, it will be helpful to consider a less complicated command that doesn't have anything to do with belief closure. It will also be helpful to choose an example that can be related to the previous example concerning dishes being washed. So let's consider the following command (D-com):

(D-com) Wash all the dishes.

The key question is whether following this command would require that you wash a particular dirty dish. Presumably it should, but it won't if a fully general relativism about logic is assumed. We already noted that if a fully general relativism about logic is assumed, (D1) by itself won't imply (DC). More explicitly, the claim that all the dishes are washed won't by itself imply that a particular dish is washed. So even if one of the dishes isn't washed, that won't by itself constitute a counterexample to the claim that every dish is washed. But if a dirty dish doesn't by itself constitute a counterexample to the claim that all the dishes are washed, then I should be able to follow a command to wash all the dishes even when I've left one dirty. In other words, if claims don't by themselves have any implications, then commands by themselves won't have any requirements for what it takes to follow them.

Exactly the same points will apply to more complicated commands like (UI-com). To see this, it will be helpful to look at an instance of UI that is related to (UI-com) (in the same way that (D) is related to (D-com)).

(B1) All relevant instances of UI are such that my beliefs are closed under them.

so,

(BC) (D) is such that my beliefs are closed under it.

The relevant question at this point is whether following (UI-com) will require that I close my beliefs under (D). It *seems* like following (UI-com) would require that I close my beliefs under (D). (D) is a relevant instance of UI, and (UI-com) says that I need to close my beliefs under all relevant instances of UI. But suppose we accept a fully general relativism about validity attribution. Then,

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<sup>21</sup>The point I'm making is like the one discussed in ch. 3 section 4 where Kripke considers a "rule-as-command" type response to his own argument against Quine. Kripke's reply might be criticized on the grounds that it presupposes a conception of rules or policies where they are understood as mentally or verbally articulated and the same can be said about the argument in the main text here. For that reason, I'll take up the issue of non-representational conceptions of rules below.

just as (D1) by itself won't imply (DC), (B1) won't by itself imply (BC). More explicitly, the claim that all relevant instances of UI are such that my beliefs are closed under them won't by itself imply that (D) is such that my beliefs are closed under it. In other words, my beliefs not being closed under (D) won't by itself constitute a counterexample to the claim that all relevant instances of UI are such that my beliefs are closed under them. But if my beliefs not being closed under (D) doesn't by itself constitute a counterexample to the claim that all relevant instances of UI are such that my beliefs are closed under them, then I should be able to meet the requirements of a command to close my beliefs under all relevant instances of UI even when my beliefs aren't closed under (D).

This means that a theory where policies are construed in terms of commands will not give us a way of making sense of the idea that validity attributions have policy-relative truth values. If the claim that (D) is valid is going to get the value true relative to a command like (UI-com), then following (UI-com) should require me to close my beliefs under (D). But we've just seen that (UI-com) by itself won't require this when a fully general relativism about logic is assumed.<sup>22</sup>

### Non-propositional and non-representational characterizations of policies

Someone might object to the reasoning in the previous section in the following way. They might say that even though the reasoning is careful not to assume a propositional conception of policies, it is still assuming a conception of policies where they are understood as somehow *representational*. What does it mean to say that a policy is representational? This idea is articulated in different ways by different theorists, but it might be thought of in the following way. We can say that a policy is representational when following the policy implies that an agent is acting under some kind

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<sup>22</sup>It's worth nothing that Field also discusses a conception of policies that is construed in terms of plans.

I am influenced by the opening chapter of Gibbard 2003, which takes norms of rationality to be plans. Among the advantages of the non-propositional choice is flexibility: it makes it easier for, e.g., both the policies one follows and the policies one approves of to get into the discussion. (Field 2009: fn. 12)

But this won't make a difference in regards to the point in the main text. Consider a plan that might be associated with the previous axiom "all universal claims imply each of their instances". For example, it could be a plan to close my beliefs under all relevant instances of UI. If (D) was a relevant instance of UI, would I need to close my beliefs under it in order to follow the plan? It's not clear why. If (D1) by itself didn't imply its conclusion, why would the plan by itself require that I close my beliefs under (D)? And if the plan by itself doesn't require that I close my beliefs under (D), why would (D1) imply (DC) relative to the plan? Whether we are talking in terms of commands or plans, the argument runs the same way.

of verbal or mental formulation of the policy. Non-representational policies are not uncommon and some theorists have argued that Carroll's dialogue can be read as an argument for thinking that some policies have to be understood in non-representational terms. For example, Devitt says

any system *has* to have some processing rules that govern it without being represented and applied. For, if there is a rule that governs by being represented and applied, *there has to be another rule that governs the application*. That rule might also govern by being represented and applied but then its application has to be governed by a further rule; and so on. If this regress is to end and any rule is to govern by being represented, there must be some rules that govern without being represented, without being *encoded*; that is a moral of Lewis Carroll's famous dialogue between Achilles and the Tortoise (1895). The upshot of this point is that, given any system that is governed by a rule, it is *an empirical question* whether the system represents and applies the rule or the rule is simply embodied in the system without being represented.

Computers demonstrate the point nicely. Software rules encoded in RAM can govern the operations of a computer only because there are rules built into the hardware that enable them to do so. (Devitt 2005: 46)

Field himself articulates a conception of logical policies where they needn't be understood in representational terms.

[W]hen I speak of rule-following I *don't* mean to suggest that the person has the rule 'written into his head'. There may be rules 'written into the head', but for those to be of use some part of the brain has to read them, and reading them is done by following rules; obviously these needn't be written in the head, on pain of regress. (Field 2001: 388)

So if the problem is ultimately based on a representational conception of policies, then it might be avoided by appealing to a conception of policies that is both non-propositional *and* non-representational. But what would a policy be if it is neither propositional nor representational? A natural way of thinking about the idea is in terms of dispositions. The idea would be that at least for some of the policies we follow, this may simply be a matter of how we are disposed to proceed. For example, it may be that my following a UI policy is nothing over and above certain facts about the way I am disposed to manage my credences in universal claims and their instances.<sup>23</sup>

What should we say about this? Ultimately, I think this point about non-representational

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<sup>23</sup>The non-representational conception of policies needn't be one where policies are identified with dispositions. Perhaps a policy could be understood as something that explains the dispositions, but in the main text I'll proceed in terms of an understanding where the policies just are the dispositions.

conceptions of policies is irrelevant because the argument I offered is actually neutral regarding the issue of whether policies are represented verbally or otherwise in the mind of an agent. Regardless of whether a policy is understood in representational terms, there will be relationships between what a claim implies and what a policy requires.<sup>24</sup> To see this, it will be helpful to return to the example of a policy for washing all the dishes.

We might suppose that a policy for washing all the dishes is understood in wholly non-representational terms. For example, we might suppose that the policy is understood in purely dispositional terms where the policy is not verbally represented or represented in the mind of an agent in any way. Nonetheless, there will still be a question about what follows from the fact that I have the requisite dispositions. To think about this more carefully, we can consider a related instance of UI that concerns those very same dispositions.

(D1\*) All dishes are such that I'm disposed to wash them.

(DC\*) This dish is such that I'm disposed to wash it.

As before, on a fully general relativism, (D1\*) won't by itself imply (DC\*). More explicitly, even if I follow the policy, i.e., even if I am disposed to wash all the dishes, this won't by itself imply that I am disposed to wash a particular dish. But if that is the case, then my lack of disposition to wash a particular dish won't by itself constitute a counterexample to the claim that I'm disposed to wash all the dishes.<sup>25</sup>

The same points will hold for logical policies construed in purely dispositional terms. For example, we might explain what it is to follow a universal instantiation policy in purely dispositional terms, where following the policy doesn't require that the agent act under any mental or verbal formulation of it. With this understanding of logical policies in mind, we can consider a final instance of UI.

(B1\*) All relevant UI instances are such that I'm disposed to close my beliefs under them

(BC\*) (D) is such that I'm disposed to close my beliefs under it

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<sup>24</sup>Instead of talking in terms of what a policy requires, the points here could be put in terms of what a policy prohibits. (This latter way of talking is in line with the way that Field (2009a) and Gibbard (1990) discuss policies.)

<sup>25</sup>I'm being a bit simplistic in the way I'm discussing dispositional analyses of rule following here. But I don't think the matter would change with more sophisticated disposition theories of rule following (which I will look at in 5.3).

As before, it will follow from a fully general relativism about logic that (B1\*) will not by itself imply (BC\*). But if (B1\*) does not by itself imply (BC\*), then my not being disposed to close my beliefs under (D) won't by itself constitute a counterexample to the claim that I'm disposed to close my beliefs under all relevant instances of UI.

This still leaves us with a problem concerning how validity attributions can have relative truth values. If a UI policy is understood in purely dispositional terms, then under what conditions will a validity attribution, like "(D) is valid", be true relative to the UI policy? Presumably the claim that "(D) is valid" would be true relative to the policy when it's part of following the policy that you are disposed to close your beliefs under (D). But we have just seen that this is something that doesn't hold on the fully general relativism about logic. Even if you follow the policy, i.e., even if all relevant instances of UI are such that you are disposed to close your beliefs under them, this won't by itself imply that you are disposed to close your beliefs under (D) (even though (D) is a relevant instance of UI).

It's probably worth saying something about why there might be a temptation to think that the argument in the previous section could be presupposing a conception of policies where they are represented verbally or otherwise in the mind of an agent. One reason might be because the argument does presuppose that policies are apt for description via phrases like "a policy for closing beliefs under universal instantiation". But this doesn't imply that policies are conceived of as having any linguistic or conceptual structure. To see this, it is helpful to refer to Devitt's conception of rule following. On Devitt's view, a rule may be apt for description via phrases like "a rule for addition".<sup>26</sup> But that doesn't mean the rule needs to be represented in the mind of an agent. The rule may be "embodied without being represented", even though we can characterize the rule in our own language and thought.

### 5.2.2 A restricted form of relativism about logic

The arguments in section 5.2.1 were directed towards a version of relativism about logic that is fully general. This fully general version of relativism about logic was the result of treating validity

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<sup>26</sup>see Devitt (2006 ch. 3)

attributions in accordance with the model for evaluative claims described in Field (2009a) where evaluative claims are treated as not-straightforwardly-factual. I prefaced the arguments in 5.2.1 with some comments indicating skepticism about whether that model was intended to be applicable to validity attribution in the first place. But what about the restricted form of relativism about logic described in Field (2015)?<sup>27</sup> This form of relativism about logic also involves a kind of non-factuality, and while Field does not explicitly endorse this form of relativism, he discusses several features that would be relevant for identifying validity attributions that don't admit of a non-factual status. For example, he says a validity attribution would be objectively wrong if it conflicted with the facts about what preserves truth. He says an "attribution of validity to an argument that definitely fails to preserve truth seems objectively wrong". If some validity attributions are objectively wrong, then a relativism about logic would have to be understood in a restricted way (where it only applies to certain validity attributions, i.e., the ones that lack objective truth values). I will argue below that this restricted form of relativism about logic still results in a Carroll-type puzzle.

So how might a restricted form of relativism be articulated?<sup>28</sup> Field (2015) says that there may be room for a kind of failure of objectivity in the case of disputes about logical validity. Even if some validity attributions do have objective truth values, he says there may still be some cases (perhaps regarding Curry paradoxical statements) where disputes about validity are "irreducibly a matter of normative policy". Recall, he says

It may be, for instance, that a view that locates the failure of the Curry argument in modus ponens and a view that locates it in conditional proof can't be distinguished in terms of how closely validity corresponds to truth preservation. I don't say that this is the actual situation, but suppose it is. In that case, the difference between the views is irreducibly a matter of normative policy (Field 2015: 61)

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<sup>27</sup>Field (2009b) also describes a restricted form of logical pluralism. In that essay, he excludes the possibility of logical pluralism for logical disagreements that are reducible to disagreements about what counts as a theorem. For example, he says "[i]n 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." (Field 2009b: 359)

<sup>28</sup>There is more than one way to envision a restricted form of relativism about logic. In the following, I will only look at the example that Field describes regarding modus ponens and conditional proof. Other ways of restricting a relativism about logic may treat modus ponens as objectively valid. For example, someone could suppose that it's not an objective matter whether the law of excluded middle is valid. I don't claim that the arguments I am making here will extend to all ways of restricting relativism about logic.

I want to look at this Curry paradox case, and argue that there couldn't be a failure of objectivity even here. To show this, I'll look at an instance of MP that occurs in a derivation of a version of Curry's paradox where  $K = \text{If } T(K) \text{ then Santa exists}$ . We can call the instance (MPC):

- (c1)  $T(K)$
- (c2) If  $T(K)$ , then Santa exists
- (c3) So, Santa exists

On this form of relativism, it is supposed to be irreducibly a matter of normative policy whether (MPC) is valid. In other words, it's not an objective matter whether (MPC) is valid. However, it is supposed to be a thoroughly objective matter whether (MPC) is valid *relative to a policy for closing your beliefs under relevant instances of MP*.

So there is still going to be an asymmetry on this view. And it might seem fair. Just because we say it's not an objective matter whether (MPC) is valid, why should we have to say it's not an objective matter whether (MPC) is valid relative to a policy for reasoning by modus ponens? Shouldn't a modus ponens policy apply to instances of MP? And isn't (MPC) an instance of modus ponens? Appearances to the contrary, I don't think this asymmetry can be maintained. If it isn't an objective matter whether (MPC) is valid, then it can't be an objective matter whether (MPC) is valid relative to a modus ponens policy either. To illustrate why this is the case, it will be helpful to consider the following argument (MPR):

- (4) (MPC) is an instance of MP.
- (5) If (MPC) is an instance of MP, then (MPC) is valid relative to an MP policy.
- (6) (MPC) is valid relative to an MP policy.

We might appeal to (MPR) to explain how it is that (MPC) can be valid relative to a policy for closing your beliefs under relevant instances of MP. But it's not clear how this argument provides an adequate explanation when a relativism about the validity of modus ponens is assumed. The problem shows up in the following way. If it's irreducibly a matter of normative policy whether (MPC) is valid, then it will also irreducibly be a matter of normative policy whether (MPR) is

valid. But if it's irreducibly a matter of normative policy whether (MPR) is valid, then there won't be a fact of the matter about whether (MPR) counts as an adequate explanation of why (MPC) is valid relative to an MP policy.

I want to consider two objections to my reasoning on this point. I've claimed that (MPR) would fail (for the relativist) as an adequate explanation of how (MPC) could be valid relative to an MP policy. My claim was predicated on the assumption that (for the relativist) there is no fact of the matter about whether (MPR) is valid. Each objection that I'll consider challenges my claim that the non-factuality of the validity of (MPR) would compromise the explanatory sufficiency of (MPR). In effect, both objections claim that (MPR) can be a suitable explanation in spite of the fact that there is no fact of the matter about whether it is valid. The first objection claims that (MPR) can be a sufficient explanation on the grounds that (MPR) is truth preserving. The second objection claims that (MPR) can be a sufficient explanation on the grounds that both parties treat (MPR) as an acceptable instance of MP.

So what about the first objection? The idea behind the objection is that even if there is no fact of the matter about whether (MPR) is valid, there may still nonetheless be a fact of the matter about whether (MPR) preserves truth. I don't dispute the fact that (MPR) preserves truth, but according to the objection, this fact is supposed to account for how (MPR) is explanatorily sufficient in spite of there being no fact of the matter about whether (MPR) is valid. But I think more needs to be said. Once it's granted that there is no fact of the matter about whether MP is valid, it becomes unclear why it would be explanatorily relevant that an argument preserves truth. In order to see why this is the case, we might regiment the claim that (MPR) preserves truth in the following way:

(8) If the premises of (MPR) are true, then the conclusion of (MPR) is true.

To illustrate why it's unclear how this truth preservation fact could be explanatorily relevant (given the relativism), we can situate (8) as a premise in the following instance of MP, (MPR').

(7) The premises of (MPR) are true.

(8) If the premises of (MPR) are true, then the conclusion of (MPR) is true.

(9) The conclusion of (MPR) is true.

As noted, the objection claims that (MPR) is explanatorily relevant on the grounds that (MPR) preserves truth. But to say that (MPR) preserves truth is to say nothing more than what is represented by claim (8) (the second premise of (MPR')). We can suppose that both (7) and (8) are true, but given the restricted relativism, there won't be a fact of the matter about whether (7) and (8) imply (9). If there is no fact of the matter about whether (7) and (8) imply (9), then it's not clear how the fact that (MPR) preserves truth would contribute to the suitability of (MPR) as an explanation of (6) (i.e., as an explanation of the explicitly relativized fact that (MPC) is valid relative to an MP policy).

The second objection also argues that (MPR) can be a suitable explanation (in spite of there being no fact of the matter about whether it is valid). According to the second objection, there is no problem with the explanatory suitability of (MPR) because both parties agree that (MPR) is one of the instances of MP that is perfectly fine. The idea is that just because the adherent of conditional proof restricts MP, that doesn't mean every instance of MP is ruled out. This might be spelled out in the following way. The adherent of conditional proof might define a notion of acceptability. Then they can isolate instances of MP where the conditional premise has an antecedent A such that "A or not A" is acceptable. In other words, we can conceive of the adherent of conditional proof as having a restricted rule: if "A or not A" is acceptable then so is "A and 'if A then B' imply B".<sup>29</sup> The idea is that this is all you need to get that (MPC) is valid relative to an MP policy.

But I think more needs to be said on this point. The second objection appeals to the idea that the adherent of unrestricted CP follows a restricted MP rule. But if the rule is characterized as it is above, it is being characterized in propositional terms (as a conditional).<sup>30</sup> That means that we can regiment an instance of the rule (that pertains to (MPR)) in the following way:

(11) If "(4) or not (4)" is acceptable, then so is "(4) and (5) imply (6)".

To illustrate why it's not clear how this restricted rule could be explanatorily relevant (given the relativism), we can look at the following instance of MP, (MPR')

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<sup>29</sup>The expression "imply" needs to be understood as lacking a kind of universality that is often associated with notions of logical implication.

<sup>30</sup>More specifically, it is something like a conditional schema, or a conditional formula that is tacitly bound with universal quantifiers.

(10) “(4) or not (4)” is acceptable.

(11) If “(4) or not (4)” is acceptable, then so is “(4) and (5) imply (6)”.

(12) “(4) and (5) imply (6)” is acceptable.

(11) is a regimentation of the relevant instance of the restricted MP rule (that pertains to (MPR)). We can suppose that (10) and (11) are true, but given the relativism, there won't be a fact of the matter about whether (10) and (11) imply (12). In other words (given the relativism), even if we grant the facts about the restricted MP rule and the acceptability of “(4) or not (4)”, there won't be a fact of the matter about whether any of this implies that (MPR) is acceptable. But if there is no fact of the matter about this, then it's not clear how citing facts about a restricted MP rule will contribute to the explanatory sufficiency of (MPR).

We might consider non-propositional characterizations of the restricted rule, but I don't think this will change the main point. We can suppose that acceptance of the restricted MP rule is wholly a matter of how an agent is disposed to manage their beliefs. For example, we might suppose that following the restricted MP rule is a matter of an agent being disposed to accept the conclusion of selected instances of MP (if they encounter them and accept the premises).<sup>31</sup> In the case at hand, this means that if an agent following the restricted MP rule encounters (MPR) and accepts the premises, then the agent will be disposed to accept the conclusion. This may seem to account for a sense in which (MPR) is explanatorily suitable, but I think this intuition is misleading (given the relativist assumption that there is no fact of the matter about whether MP is valid). This can be illustrated with the following instance of MP.

(13) The agent encounters (MPR) and accepts the premises.

(14) If the agent encounters (MPR) and accepts the premises, the agent will be disposed to accept the conclusion.

(15) The agent will be disposed to accept the conclusion.

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<sup>31</sup>A dual of this condition can be added as well where an agent will be disposed to reject at least one premise of selected instances of MP (if they encounter them and reject the conclusion). There are also various ways that a theory of dispositions might be modified or refined. Perhaps the points in the main text would not hold for alternative ways of thinking about logical dispositions, but I will focus on only one way of thinking about the matter in the main text.

The first premise is simply a claim about an agent who encounters (MPR) and accepts its premises. The second premise is a conditional claim that should accurately describe the dispositions of the agent (given that the agent is disposed to reason in accordance with the restricted MP rule). But if there's no fact of the matter about whether MP is valid, then there won't be a fact of the matter about whether (13) and (14) imply (15). More specifically, if an agent follows the restricted MP rule, and encounters (MPR), and accepts the premises of (MPR), there won't be a fact of the matter about whether any of this implies that they are disposed to accept the conclusion of (MPR) (given the relativism about the validity of MP). But if there is no fact of the matter about this, then citing the fact that an agent follows a restricted MP rule won't account for why (MPR) is explanatorily relevant.

### **A further related worry for restricted forms of relativism about logic**

There is further problem that can be developed in terms of the way that locutions like “relative to a policy” are elaborated. Field (2009a) understands this in terms of a notion of compatibility, and if we follow this idea in the case of validity attribution, the analysis might look something like this: “(MPC) is valid” is true relative to an MP policy just in case rejecting (c3) while accepting (c1) and (c2) is incompatible with an MP policy.<sup>32</sup> But how should this notion of compatibility be understood? Field suggests that it can be thought of in terms of model-theoretic compatibility.

Tentatively, let's assume that different model theories will give different results about what is compatible with what. For example, we can assume that according to a model theory with modus ponens, if someone accepts (c1) and (c2) but rejects (c3), that will be incompatible with a policy for reasoning by MP. But according to certain model theories without modus ponens, if you reject (c3) while accepting (c1) and (c2) that won't be incompatible with a policy for reasoning by MP.<sup>33</sup>

<sup>32</sup>As mentioned in the exegetical section, Field (2009a) unpacks the “relative to a policy” locution when it occurs in statements that are explicitly about actions or beliefs (e.g. “It's right to withdraw the troops relative to policy x”). Since a validity attribution doesn't involve any reference to actions or beliefs on its surface, the analysis can only make sense by adverting to the states of acceptance that are involved in the norms that get expressed by validity attributions on Field's view.

<sup>33</sup>For example, according to an MP policy, the following conditional might true: if you accept (c1) and (c2) but reject (c3), you are in violation of the policy. But in a model theory like FDE, you may assign the value B to the antecedent and F to the consequent. Then the conditional will get the value B. If B is read as “true and false”, then this will imply that accepting (c1) and (c2) and rejecting (c3) is not incompatible with an MP policy. This is not the

Given this, model-theoretic notions of compatibility do not determine any particular truth value as being the one that gets assigned to the claim that “rejecting (c3) while accepting (c1) and (c2) is incompatible with a policy for reasoning by (MP)”. At best, we get one truth value according to some model theories, and another truth value according to other model theories. Since, by assumption, it is irreducibly a matter of normative policy whether (MPC) is valid, nothing independent of normative policies will privilege a model theory with *modus ponens* over one without *modus ponens*. So, for the claim that “(MPC) is valid”, no particular truth value is determined by notions of model theory and what a model theory says regarding relationships of incompatibility with policies.

### 5.3 Underdetermination

There is another problem for versions of logical pluralism that are based on the style of relativism in Field. It's part of the view that (at least some) validity attributions will only have a policy-relative truth value. And Field provides a purely dispositional account of what it is to follow a logical policy. This results in an underdetermination puzzle. If a validity attribution is going to get a truth value relative to the logical policies that an agent follows, then facts about the agent's dispositions will have to determine facts about which logical policies they follow. But it isn't clear in the first place how dispositional facts could determine any facts about the logical policies that an agent follows.

This kind of concern should be familiar from Kripke's “Wittgenstein on Rules and Private Language”.<sup>34</sup> In that discussion, he develops arguments concerning finitude and error against dispositional analyses of rule following in the case of addition. I won't describe Kripke's arguments in depth here, but I take it that if Kripke's arguments in the case of addition are sound, then they can be extended to the case of logical rules as well (e.g. following a policy for inferring by *modus*

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same result that you get from a classical model theory.

<sup>34</sup>Perhaps surprisingly, given the title of his book, much of Kripke's discussion is not phrased explicitly in terms of “rule following” but rather in terms of what a speaker means by a linguistic expression. For example, when Kripke discusses the issue of finitude in regards to dispositional theories, he describes the view he aims to criticize as one where facts about a speaker's dispositions constitute the facts about what the speaker means by an expression like “plus”. In the following discussion, I will presume that any points phrased in terms of what a speaker means, can, for all intents and purposes here, be interchanged with points that are discussed overtly in terms of rule following.

ponens or universal instantiation).

This can be seen by drawing attention to analogies between arithmetic computation and logical inference. For example, just as an agent will have made only finitely many addition computations in their life, an agent will also have only made finitely many inferences in accordance with modus ponens. Moreover, just as there are arithmetic queries that are too complex for finite agent to comprehend, there will also be instances of modus ponens with formulas and terms that are likewise too computationally complex for an ordinary agent to deal with.<sup>35</sup> Given these limitations, if there is any problem with the idea that an agent's arithmetic dispositions could determine whether they followed an addition rule as opposed to some quus-like variant, there will likewise be a problem for how an agent's inferential dispositions could determine whether they are following classical logic as opposed to some quus-like variant of classical logic. For example, consider a number  $n$  that is so large that whenever an expression has  $n$  symbols it is beyond the boundaries of our computational limitations. For any logic  $L$ , there will always be some other logic  $L^*$  that differs from  $L$  only over certain sentences with levels of complexity beyond  $n$ . In that case,  $L$  would differ from  $L^*$  only in places that are beyond the boundaries set by our computational limitations. For any inference the agent makes,  $L$  and  $L^*$  will be in agreement about whether it is permitted.<sup>36</sup>

If these kinds of considerations are right, then for any particular agent, there is nothing about their dispositions that would constitute a fact of the matter about whether they follow a universal instantiation policy (as opposed to some quus-like version of it). But in that case, it wouldn't be clear how a validity attribution could get a truth value relative to an agent's policies (assuming their policies are construed in purely dispositional terms). An agent's policies would underdetermine a truth value for the claim that "UI is valid" because the facts about the agent underdetermine whether they are following a UI-rule as opposed to a quus-like surrogate for a UI-rule.

The natural response for a disposition theorist would be to resist the arguments in Kripke's Wittgenstein. As noted previously, a common complaint is that the versions of dispositionalism that get treated in Kripke's Wittgenstein are overly simplistic. Some defenders of dispositional

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<sup>35</sup>For example, an arithmetic query may involve an expression with  $10^{10}$  numerals and an instance of modus ponens may have  $10^{10}$  conjuncts in its first premise.

<sup>36</sup>I'm describing things partly in terms of inferential behavior here, but the idea in the rule following considerations is that the issue will not change if we look at any dispositions or psychological reality that might underly an agent's behavior. I'll go into more detail about this below.

analyses argue that they can overcome the worries raised in Kripke's discussion by making various refinements to the dispositional view.<sup>37</sup> The idea is that once the relevant refinements are made, a dispositional account of rule following can be provided where facts about an agent's dispositions actually *can* determine facts about which logical policies they follow.<sup>38</sup> In the following, I will focus on some more theoretically sophisticated dispositional analyses of rule following, and I'll argue that they do not resolve the original issue. I'll first look at a view from Shogenji, and then I'll look at a view from Warren.<sup>39</sup>

### 5.3.1 Shogenji's Dispositional theory of rule following

Shogenji explicitly takes up some of the objections in Kripke's Wittgenstein and argues that they can be resisted on a dispositional view rightly understood. If Shogenji could produce a successful response to the criticisms in Kripke's Wittgenstein, it would allow for facts about which rules an agent follows for "plus" to be grounded in facts about how they are disposed to use the expression "plus". In the same way, there would be an account of how an agent's dispositions could ground determinate facts about what logical policies they follow. That would remove the underdetermination worry for the idea that validity attributions can get truth values relative to policies that an

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<sup>37</sup>In personal correspondence, Field suggests elements of Shogenji's view (1993) as an ingredient for how he might respond to Kripke's objections to dispositional analyses of rule following. There are other dispositional theories that might be considered as well (for example, see Horwich (1998) and Warren (2018)). Kusch (2005), a popular defender of the line in Kripke's Wittgenstein, also thinks that a dispositional analysis may provide a way of responding to the skeptical worries.

<sup>38</sup>Sometimes solutions of this sort are described as "straight" solutions to the rule-following problem. Straight solutions need not be given in terms of dispositions, but I will only focus on dispositional solutions in what follows.

<sup>39</sup>There is a related issue about how validity attributions can get truth values relative to policies that an agent actually follows. It is tempting to think that people will often be following incompatible policies or policies that result in triviality (i.e. that they imply everything). (See Azzouni (2007/2013) for an argument for this claim.) At the very least, Field discusses examples of agents that are confronted with the fact that their policies lead to triviality. He says "people faced with the semantic paradoxes may end up with different choices about whether to keep their logic and revise their basic rules for truth, or to keep their basic rules for truth and alter their logic." (Field 2009a: 282) If the policies we actually follow were inconsistent or trivial, and truth values of validity claims were relativized to those policies, validity attributions would have inconsistent or trivial truth values. Someone might try to avoid this result by appealing to consistent idealizations of policies (rather than the policies an agent actually follows). But if we follow some of the themes in Field, there will still be Carroll-style worries for this kind of response. One issue is that if there is no privileged notion of consistency, then relativizing to consistent policies will still leave us with some indeterminacy that is based in the notion of consistency. Moreover, the kinds of Gibbard-style idealizations that Field (2009a: 264) describes are regimented in terms of conditional claims. For example, regarding "precise" policies, he says "if the norm prohibits believing p in circumstances C but in no other circumstances, it determines the value true for 'You should refrain from believing p' in those worlds in which C is true and the value false in the others." But if we follow the idea that it is irreducibly a matter of normative policy whether modus ponens is valid, the conditional and its antecedent won't be sufficient by themselves to imply the consequent. So, granting the relativism, it's not clear how precise policies could play the explanatory role of determining truth values.

agent follows.

### Exegesis of Shogenji's view

Shogenji says that various forms of skepticism can be found in Kripke's discussion, but he only aims to address a form of skepticism that he calls "modest skepticism". He characterizes modest skepticism as a challenge for a theorist to cite facts that will both determine what a speaker means by an expression and also rule out the possibility that the speaker operates with a non-standard (quus-like) meaning for the term. He describes this form of skepticism as one that can be levied against someone who holds a "Past Usage Theory" of meaning. On a Past Usage Theory of meaning, what a person means by an expression is determined by how they have used that expression in the past. He says

The modest sceptic challenges this Past Usage Theory by pointing out that because of the finitude of past usage, the theory fails to exclude the possibility that her interpretation scheme was QuEnglish according to which '+' means quus rather than plus. In other words, the Past Usage Theory underdetermines the interpretation scheme. (Shogenji 1993: 490)

Shogenji actually characterizes modest skepticism in more than one way, and this will be relevant in the discussion below, but a key feature of modest skepticism is that there are adequacy conditions for a solution to it. I'll focus on two of these conditions that Shogenji calls "correctness" and "uniqueness". For correctness he says

given the relevant facts, the semantic theory must deliver the standard interpretation scheme, according to which what we take to be the correct use of an expression is indeed correct. (Shogenji 1993: 492)

For the uniqueness condition, he says

given the relevant semantic facts, the semantic theory must allow no non-standard interpretation schemes, according to which what we take to be the wrong use of an expression is correct. (Shogenji 1993: 492)

Shogenji's discussion centers on the issue of whether dispositional (as opposed to use) theories of meaning can respond to modest skepticism. Before considering his preferred dispositional theory,

he looks at a simple version of a dispositional theory where correct usage for a term is based on a speaker's past dispositions.

In its simplest form, the Dispositional Theory says that the correct usage of an expression is the one the speaker was disposed to in the past. For example, uttering '46' as an answer to '19+27' is considered correct if and only if the speaker was disposed in the past to utter '46' as an answer to '19+27'. (Shogenji 1993: 294)

Shogenji develops his more theoretically nuanced version of dispositionalism by looking at two objections to the simple version of dispositionalism. One is based on considerations concerning the finitude of a speaker's past dispositions, and the other is based on considerations concerning a speaker's dispositions to make errors. Here is Shogenji's understanding of the finitude objection.

The sceptic points out that no human speaker is disposed to add indefinitely large numbers, and hence the speaker's disposition underdetermines the interpretation of '+' [5, pp.26-28]. In other words, the Dispositional Theory fails to satisfy the uniqueness condition of an adequate solution to modest skepticism. (Shogenji 1993: 494)

He also describes an objection that is based on a speaker's disposition to make errors.

Suppose a speaker mistakenly said '19+27=48' in the past because she was tired. It follows that she was disposed to make this mistake, for what she did must be what she was disposed to do. Consequently, on the basis of this faulty disposition, the Dispositional Theory assigns to her expression '+' a non-standard interpretation according to which '19+27=48' is correct. (Shogenji 1993: 494)

To formulate a version of dispositionalism that deals with these objections, Shogenji introduces a notion of simple (or "basic") routines and their recursive application. To explain simple routines, he cites operations or processes that can be broken down into smaller basic operations. In particular, he says the operation of addition is composed of smaller "basic routines" such as adding single digit numbers and carrying.

the operation of addition consists of a small number of basic routines such as additions of single-digit numbers and carrying. By applying these routines repeatedly, we can in principle add numbers of any magnitude. (Shogenji 1993: 495)

In the passage below, Shogenji also discusses simple routines as things which can be applied recursively, and he says that infinitely many results can be achieved through recursive application of

finitely many “simple routines”.

It is well known that finitely many simple routines can produce infinitely many results by their recursive applications, i.e., by repeatedly applying the simple routines to the results of their previous applications. (Shogenji 1993: 495)

How does this figure into Shogenji's response to the previous two objections? In his response to the objection concerning error, Shogenji goes into more detail about the notion of basic routines. He considers a *prima facie* worry about how the introduction of basic routines (e.g. adding single digit numbers) would make no progress on the issue concerning error since individuals are sometimes disposed to make mistakes when they add single digit numbers. To control for this, Shogenji suggests that simple routines be understood relative to “normal” circumstances or circumstances that are not “unfavorable”. He thinks that in favorable circumstances, there will be no mistakes, and he lists things like being drunk or tired as examples of unfavorable circumstances. He says

the Dispositional Theory faces the objection that the speaker can be disposed to the wrong routines. Notice, however, that since our problem is the determination of simple routines, such as adding single digit numbers, we can assume that the competent rule-follower makes an error only under unfavourable conditions — i.e., being tired, distracted, drunk, etc. (Shogenji 1993: 495)

So the application of a basic routine will count as correct when it aligns with how the speaker is disposed in favorable conditions. He says

if the speaker is disposed to a certain routine under normal conditions, we regard it as her correct routine. (Shogenji 1993: 496)

The idea is that this will siphon out cases where someone attempts to add single digit numbers and comes up with non-standard results.

Regarding the objection based on finitude, Shogenji notes that it is not immediately resolved by the introduction of basic routines and the idea that they can be recursively applied. He assumes that the issue is not immediately resolved because the addition function has infinitely many outputs and we are not able to apply basic routines infinitely many times.

[T]he introduction of recursive rules by itself does not solve our problem since the addition of indefinitely large numbers requires indefinitely many applications of the basic routines, of which no human speaker is capable. In other words, there still remains a

conflict between the finite disposition of the speaker and indefinitely many applications of basic routines. (Shogenji 1993: 495)

While Shogenji notes that human speakers are unable to recursively apply basic routines indefinitely, he says the recursive application of basic routines is nonetheless the right way to apply them.

The correct way of applying basic routines is their recursive application (Shogenji 1993: 498)

Granting that the problem of error is resolved in the case of basic routines, it is true that the right extension for “+” can be fixed by the recursive application of basic routines. But it may not be obvious why this observation would be of any relevance regarding the finitude objection since, as Shogenji points out, a person cannot recursively apply basic routines forever. Moreover, someone may apply basic routines recursively for a while, and then go on to perform some quus-like operation. Shogenji anticipates this kind of concern.

The finitude of the speaker's disposition allows the sceptic to argue against the Dispositional Theory in essentially the same way as he did in the quus argument; he can insist on a non-standard interpretation of ‘+’, according to which the referred function is calculated by requursive rules, where the requursive application of basic routines demands a quus-like digression at a sufficiently late stage of their application. Since no human speaker is disposed to apply any routines indefinitely many times, we cannot determine on the basis of the speaker's disposition whether she was following recursive rules or requursive rules. (Shogenji 1993: 497)

So what facts about a speaker would determine whether or not the recursive (as opposed to requursive) application is the right method for determining the correct usage of “plus”? Shogenji does not answer this question. He says the search for such facts should be abandoned. He says

I suggest we abandon the idea of designing conditions, actual or counterfactual, to obtain the right application. In fact I suggest we abandon the idea of searching for facts at all in determining the right application. My alternative solution is trivial: ‘The correct way of applying basic routines is their recursive application.’ Instead of mentioning any facts, this solution incorporates the right application — recursive application — into the semantic theory itself; i.e., the application of basic routines must be recursive, regardless of any facts about the speaker. (Shogenji 1993: 498)<sup>40</sup>

Why accept this? According to Shogenji, this meets the adequacy conditions he spells out.

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<sup>40</sup>If this is an acceptable solution, then it's natural to wonder why something like this couldn't have been said at the very beginning. I'll go into more detail about this in a footnote below.

The recursive application of basic routines results in a value for the plus function for each ordered pair of numerals. Moreover, this gets the right result granted that basic routines are determined correctly.

### **Criticism of Shogenji's response to the finitude objection**

Shogenji fails to meet the central challenge in Kripke's Wittgenstein. In the last quoted passage from the previous section, Shogenji grants that there are no facts about a speaker that constitute their meaning plus as opposed to quus. But this is the central assumption of the skeptic in Kripke's dialogue. This is made explicit towards the very beginning of the dialogue on rule following.

An answer to the sceptic must satisfy two conditions. First, it must give an account of what fact it is (about my mental state) that constitutes my meaning plus, not quus. (Kripke 1982: 11)

Given this, Shogenji is just not addressing the central skeptical challenge in Kripke's dialogue. The skeptic in Kripke's dialogue raises a challenge to account for facts about a speaker's mental state that would determine what the speaker means, and Shogenji grants that there are no facts about a speaker's dispositions that would determine whether the recursive as opposed to requursive application of basic routines fixes correct usage. Kripke very explicitly in the above passage indicates that the skeptical challenge involves not just an account of facts, but facts about a speaker, in particular facts about their mental state. This is what is most interesting and disconcerting about the challenge in Kripke's dialogue. If the skeptic in Kripke's dialogue is right, then there is no fact about me that determines whether I mean plus by my use of "plus".

So what would Shogenji say to a skeptic who supposes that the correct usage of "plus" is fixed by the requursive as opposed to recursive application of basic routines? He describes just such an objection.

Some might suspect that our solution has yet to exclude requursive rules; i.e., the Initial Dispositional Theory merely declares that the application of dispositionally determined routines must be recursive, without giving any factual ground to support that. If that is acceptable, then it may appear that the sceptic can also declare, without any factual ground, that the application must be requursive. (Shogenji 1993: 499)

Shogenji says that this objection might be acceptable in certain circumstances, but he thinks it isn't an appropriate criticism for a view that is only aiming to address modest skepticism.

I agree that this could be a legitimate point in some other context, but the sceptic cannot use it in the discussion of modest scepticism. (Shogenji 1993: 499)

There is nothing wrong with addressing a form of scepticism that is different from the one exemplified in the previous quote from Kripke. But there is a problem with Shogenji's failure to respond to the requursion challenge because the requursion challenge is motivated in a way that is wholly analogous to the modest skeptical challenge. The modest skeptical challenge was motivated in the context of a discussion of use based theories of meaning. There, the stated problem was that an individual's finite use of "+" will fail to exclude a quus-like interpretation of "+" (see the first cited passage above from Shogenji p. 490). Given the way that Shogenji motivates modest skepticism in the context of this discussion, modest skepticism explicitly concerns the issue about whether facts about a speaker (in this case their finite use) would underdetermine an extension for their use of the "+" symbol. So when Shogenji discusses modest skepticism in this passage, it parallels the issue indicated in the previously cited passage from Kripke (where the challenge is to cite a fact about a speaker that would constitute what they mean). In the requursion objection, it is pointed out that facts about the speaker's basic routines (understood as fixed by normal situations) underdetermine an extension for the speaker's use of the "+" symbol. So, the issues are analogous. Nothing about the speakers past usage will tell us whether they mean plus as opposed to quus, and nothing about the speaker's basic routines will tell us whether the recursive as opposed to requursive application is correct.<sup>41</sup>

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<sup>41</sup>There is a place where Shogenji comments further on why the requursion objection is supposed to be different from modest skepticism. The comments invoke some technical points concerning a distinction between semantic theories and interpretation schemes, but I don't think the distinction makes any difference to the overall point I am making in the main text. Shogenji says,

[t]he *requursion* objection that a digression at a sufficiently late stage of the application of basic routines may actually be correct amounts to suggesting an *alternative semantic theory* which uses the same semantically relevant facts — the speakers' initial dispositions — but which maps the facts to non-standard interpretation schemes. (Shogenji 1993: 499)

But again, this is exactly analogous to the modest skeptic's objection to past usage theory. Past usage theory is a semantic theory that maps the speaker's past usage to the correct interpretation of "plus", and the modest skeptic objects to past usage theory on the grounds that past usage doesn't rule out quus-like interpretations for "plus". In other words, the modest skeptic is drawing attention to the fact that there is an alternative semantic theory that maps the speaker's past usage to a non-standard interpretation scheme. The modest skeptic contends that there is no fact about a speaker's usage that mandates a choice of past usage theory over past quusage theory.

The take home point is that basic routines do not provide us with an avenue for making any significant progress over the original puzzle with past usage theory. Just in the same way that past usage fails to rule out non-standard interpretations, so do basic routines. The point is not that Shogenji isn't aware that basic routines by themselves fail to determine the right application for "+". He mentions this in the passage above where he says "the introduction of recursive rules by itself does not solve our problem since the addition of indefinitely large numbers requires indefinitely many applications of the basic routines". The point is that even though Shogenji says the requursion objection is not one he aims to address, the requursion objection is not significantly different from moderate skepticism (as described in the passage from p. 490) *which he does aim to address*.<sup>42</sup> That being said, the points about the determination of basic routines can be seen as a response to the objection concerning correctness, so I will turn to that issue now.

### Criticism of Shogenji's response to the correctness objection

Shogenji says that the correct application of basic routines is determined by their results in normal situations. He cites things like drunkenness and being tired as examples of non-normal situations, and he also gives an illustration of how non-normal situations might be understood more generally. He suggests the possibility that non-normal circumstances be thought of as ones where our answers are not stable, where instability implies that your responses to single digit addition problems are not predictable. For example, if a person is heavily drunk, their answers are less predictable. In response to "3+5" they may say "9", "10", or who knows what.

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<sup>42</sup>It's worth saying something about why Shogenji thinks he has successfully responded to moderate skepticism. He thinks he has responded to moderate skepticism since he argues that he has met the adequacy conditions he described as required for a sufficient response. According to the adequacy conditions, an adequate response needs to define a mapping from a set of facts to the right interpretation for "plus". If Shogenji is right that basic routines can be appropriately determined by appeal to normal conditions, then I think he can successfully define a mapping from basic routines to the right interpretation for "plus". To build the mapping, it just needs to be stipulated that basic routines are to be applied recursively. But why does success here matter? It doesn't address the worry raised in Kripke's Wittgenstein because it does not give an account of what facts about a speaker would constitute what they mean by their use of a linguistic expression. The issue can be brought out by the fact that we can map any fact to the correct interpretation for "plus". In particular, I can map the facts about my past usage to the correct interpretation scheme for plus. Just let  $f$  be a function that takes my past use of "plus" as an input, and returns the standard interpretation of "plus" as an output. Such a mapping wouldn't be counted as acceptable by Shogenji because of a further adequacy condition requiring generality. He says "[i]t is the requirement that the semantic theory must satisfy the correctness and the uniqueness conditions for every conceivable language." (Shogenji 493) Shogenji's mapping does meet this further requirement because it is systematic. But systematic or not, this does not respond to the central form of skepticism concerning finitude in Kripke's Wittgenstein. Moreover, there are systematic mappings that will get the wrong result (e.g. requursive mappings), and Shogenji accepts that a speaker's dispositions will not rule out requursive mappings.

There can be many such ways of specifying normal conditions, but I will describe only one, namely: 'Among actual conditions under which the speaker executed basic routines, normal conditions are those under which her executions were stable.' It is based on the observation that the execution of a basic routine is unstable under non-normal conditions. For example, when the speaker is given the problem '5+3' while she is heavily drunk, her answer will be unpredictable since a slight variation in her conditions might make her answer '6', '9', or any other neighbouring number. On the other hand, if her conditions are within the normal range, a minor variation in her conditions will be absorbed, without making any observable difference in her answer. (Shogenji 1993: 496)

This does seem to make some progress, but I don't think the illustration of what counts as a non-normal circumstance gets the right result. The problem is that the characterization of non-normal circumstances won't include cases where we make systematic and predictable errors.<sup>43</sup> Shogenji mentions that the points about stability are only supposed to be an illustration of how normality might be specified, and he mentions that details may need to be altered to avoid counterexamples.<sup>44</sup>

### 5.3.2 Warren's Dispositional theory of rule following

Warren (2018) also defends a dispositional analysis of what it is for an agent to follow a rule. One reason why Warren's theory is interesting is because it can be cited to provide a response to the previously mentioned worries concerning systematic and predictable error. Like Shogenji, Warren also develops his view in response to Kripke's criticism of dispositional theories. I'll break up the exegesis of Warren's view into three sections, and then I'll provide some objections.

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<sup>43</sup>Psychologists are known for producing examples where things like this happen. A commonly cited example is the Wason card selection test (1966/1972). There are also some well known illusions where things like this occur. For example, there are contexts where people systematically fail to recognize the double occurrence of the word "the" in the string "I love Paris in the the springtime". I don't know of a particular example where this happens in the case of addition, but it would be surprising if there aren't analogues of the same thing in mathematics. Certainly there are moves that can be made here. For example, one way of responding to the Wason card selection type points is to appeal to dispositions to accept corrections when they are pointed out. I'll actually look at a response like this when I discuss Warren's view, but for now I will maintain focus on Shogenji's view.

<sup>44</sup>He says his characterization of basic routines is supposed to show that there is no circularity or conceptual difficulty for a disposition theoretic response to the problem of error, and he says the burden of proof falls on the skeptic to show that no such story can be told. Whether or not a counterexample free characterization of normal conditions can be given, I don't think anything that Shogenji has said shows that there is no circularity in disposition-theoretic proposals (even if he is right about who has the burden of proof). This is not to say that it has been shown that there is an unavoidable circularity in disposition based approaches. It is just to say that the matter seems unresolved by what Shogenji has said, and the particular characterization that he has given does in fact seem prone to counterexamples.

### Exegesis of Warren on the problem of Finitude

One of Warren's main goals is to address Kripke's finitude objection to dispositionalism. His discussion of the objection centers on the example of arithmetical queries involving extremely large numerals. The *prima facie* worry is that for those queries, an agent will have no disposition to respond. Warren illustrates this worry by considering a hypothetical agent (who he calls "Ludwig"). He says

according to Kripke, Ludwig, like all of us, does not actually have a disposition to respond when queried about the sum of *extremely* large numbers (Warren 2018: 4)

A standard response for dispositionalists is to incorporate a *ceteris paribus* clause, but Warren does not pursue this option.<sup>45</sup> He thinks that a better and more informative approach can be given where dispositions to respond to questions with extremely large numerals are understood as being somehow composed out of more basic dispositions.

A more informative approach sees our dispositions to respond to lengthy arithmetical queries as somehow composed of various simple dispositions that are used in executing a general process of computation that we are disposed to undertake in response to "+"-queries. (Warren 2018: 6)

Warren is not however interested in versions of this view of rule-following that assume any "internal instructions" that an agent might have given themselves and that are employed in their arithmetic computations. Warren cites Blackburn (1995) and Shogenji (1993) as theorists who have provided examples of the sort of view he is interested in, but he thinks they have not developed their accounts in detail or defended their accounts from objections. He says

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<sup>45</sup>The idea with a *ceteris paribus* clause is to think about what a person would do all other things being equal. The main hurdle for theories incorporating a *ceteris paribus* clause is to make sense of what it means for all other things to be equal. Kripke anticipates this style of objection, and he raises doubts about how the notion would be usefully fleshed out. He says

how should we flesh out the *ceteris paribus* clause? Perhaps as something like: if my brain had been stuffed with sufficient extra matter to grasp large enough numbers, and if it were given enough capacity to perform such a large addition, and if my life (in a healthy state) were prolonged enough, then given an addition problem involving two large numbers  $m$  and  $n$ , I would respond with their sum, and not with the result according to some quus-like rule. But how can we have any confidence of this? How in the world can I tell what would happen if my brain were stuffed with extra brain matter, or if my life were prolonged by some magic elixir? (Kripke 1982: 27)

The *ceteris paribus* clause strategy is taken up and defended by Fodor (1991). See also Kusch (2005) for a criticism of Fodor's view.

[a]ttempts at this have been made by Simon Blackburn and Tomoji Shogenji. Neither Blackburn nor Shogenji developed their approach in detail or defended it from objections, so let me make a fresh start at clarifying and developing what I see as the key idea behind their approach by explicitly distinguishing between simple dispositions and complex dispositions (Warren 2018: 6)

So Warren provides his own articulation of what he takes to be the key distinction. For simple dispositions, he says

(SIMPLE)  $S$  has a simple disposition to  $\phi$  in situation  $C$  iff  $S$   $\phi$ s in  $C$  directly, not by way of performing any intermediate actions or activities (Warren 2018: 6)

Regarding complex dispositions, he says

(COMPLEX)  $S$  has a complex disposition to  $\phi$  in situation  $C$  iff  $S$   $\phi$ s in  $C$  as a result of performing some intermediate actions or activities that  $S$  is disposed to undertake in  $C$  (Warren 2018: 7)

The idea is that for many addition queries, an agent will lack any simple disposition to reply with the sum, but will nonetheless have a complex disposition to reply with the sum. This is supposed to be because an agent can have a disposition which is somehow built up out of simple dispositions (where the relevant simple dispositions would be things like adding single digit numbers, carrying, etc.). Warren describes this with regards to the hypothetical agent “Ludwig”.

[F]or many of these cases he will have complex dispositions to reply with the sum. This is because he is disposed to execute an algorithm for calculating the sum in such cases, and this disposition is realized through various simple dispositions to add single digit numbers, carry if appropriate, and then move on to the next pair (or triple) of single digit numbers to repeat the process again. Since Ludwig’s execution of this algorithm consists not in any internal instructions, but rather in *his dispositions to act in particular ways*, there need be nothing for Kripkenstein to sceptically reinterpret. (Warren 2018: 7)

As is evident from the above passage, part of the promise of the view is supposed to reside in the fact that, since there are no “internal instructions”, there is nothing for a skeptic to re-interpret. But Warren notes that there is still a puzzle regarding issues of finitude for views like Shogenji’s. He says that Shogenji’s view will still have to rely on some kind of *ceteris paribus* clause because we do not have a simple or complex disposition to carry out extremely long arithmetic computations. For example, before finishing an extremely long addition problem, at some stage of the computation,

an agent is inevitably going to die or pass out from exhaustion. Warren says

in order to deal with truly astronomical numbers using complex dispositions, we still need a *ceteris paribus* hedge. This is because while Ludwig will have complex dispositions to reply with the sum in many cases where he has no simple dispositions to reply with the sum, he won't have complex dispositions to reply with the sum for truly astronomical pairs of numbers, since in such cases he will die or give up before he can finish executing the algorithm (Warren 2018: 7)

That being said, Warren still thinks that there is a way to resolve the issue along dispositional lines. Even if we get tired or die before the completion of a particular stage, Warren thinks there is still a sense in which we have a disposition to complete that stage and move on to the next one. He says there is still some sense in which the “unbounded execution of the process” is “encoded” in an agent's dispositions. His main idea is that an agent's dispositions are somehow linked. For example, he thinks that for any two adjacent stages of an addition computation, an agent will have the disposition to complete the second stage immediately after the first (and be disposed to complete the second stage only after the first stage has been completed). He says

The basic problem we encountered was that Ludwig's disposition to execute step 9,999,999 of the process, for two astronomical numbers, could be destroyed. In fact, one of the things that could destroy it was *his having previously executed the preceding 9,999,998 steps*.

Still, even in such cases, there is a perfectly coherent sense in which unbounded execution of the process is already encoded in Ludwig's starting dispositions, and this is what we need to capture. Ludwig's disposition to carry out step 17 of the addition algorithm is *linked* to his disposition to carry out step 18, since in the relevant situations, Ludwig is disposed to execute step 18 immediately after executing step 17, as well as being disposed to execute step 18 only after step 17 has already been carried out in some form or other. Given this, we can make sense of the *iterated application* (or “exercise” or “manifestation”) of Ludwig's linked dispositions even if Ludwig, being horrifically slow at addition, always manages to fall asleep before completing step 17 and so doesn't have a complex disposition to execute the complete addition algorithm for pairs of large numbers. (Warren 2018: 8)

Warren uses this notion of linked dispositions to make a distinction between “singular” and “composite” dispositions. He understands singular dispositions in the following way

(SINGULAR)  $S$  has a singular disposition to  $\phi$  in situation  $C$  iff  $S$  has a simple (or complex) disposition to  $\phi$  in  $C$  (Warren 2018: 8)

He understand composite dispositions as

(COMPOSITE)  $S$  has a composite disposition to  $\phi$  in situation  $C$  iff  $\phi$ ing is (or would be) the output of the iterated application of  $S$ 's linked simple (or complex) dispositions, in  $C$  (Warren 2018: 8)

So, for Warren, the fact that an agent follows an unbounded addition rule is based in their having the requisite composite disposition to add. For Warren's hypothetical agent "Ludwig", he says

Ludwig's ability to execute the addition algorithm is not a matter of him giving himself certain instructions (as Kripke suggested) nor of having complex dispositions to reply with the sum to any addition question (as Blackburn and Shogenji, in effect, suggested), but rather of his having appropriate *composite* dispositions.

The central idea is that even when Ludwig lacks any singular disposition to reply with the sum of two astronomical numbers when queried, he still has the disposition to execute step 1 of the addition algorithm for these numbers, while also having, *for each particular step*, the disposition to execute that step and move to the next step. But this doesn't imply that he has a complex disposition to execute all of the steps together, since his particular dispositions for astronomical steps can be destroyed if he dies or works to exhaustion or whatnot. But for astronomical numbers  $n$  and  $k$ , Ludwig currently has the simple disposition to execute step 9,999,999 of the process and move to the next step: if he were asked to continue the process of adding the numbers at that step, he would execute that step and move to step 10,000,000, *and the same goes for every single step of the process*. (Warren 2018: 9)

Granted that the issue of error can be resolved, Warren thinks that this resolves the issue of finitude.

He says

Ludwig's composite dispositions, specified in terms of his singular dispositions, determine the answer he is "disposed" to give when presented with any query about "+" (Warren 2018: 12)

### **Exegesis of Warren on the problem of error**

Warren brings together multiple considerations to develop his response to the problem of error. The error issue is based on the observation that individuals are sometimes disposed to make mistakes, and this is supposed to be a *prima facie* problem for the dispositional view of rule following. If dispositions are said to determine meaning (or the facts about what rules an agent follows), and

we have dispositions to make mistakes, then it would seem that dispositions must always therefore fail to determine the right meaning or rule.<sup>46</sup> Warren notes that individuals can even be disposed to make systematic errors.

Kripke's problem of error provides another challenge to the extensional adequacy of dispositionalism. The problem is generated by the fact that people have dispositions to make mistakes, even to systematically make mistakes. (Warren 2018: 12)

Warren thinks disposition theorists can respond to the issue of error by making various refinements and additions to the disposition based theory. He says

by improving on several extant suggestions and combining them with novel ideas, we can arrive at a satisfying response involving multiple layers that screen out different kinds of errors and, together, do enough to solve the problem of error in its entirety. (Warren 2018: 13)

He also makes an observation that is important for understanding his view. He points out that for any particular arithmetic query, a person will not provide the same answer to that query in every situation. This is because there are many possible situations where the person will respond to the query (some where they respond one way, others where they respond in another way). There are many reasons why a person might respond to the same query differently in different situations. Reasons can include things as various as their level of sobriety or fatigue. He says

There are many situations where Ludwig falls into error over simple sums, say when he is drunk, or hung over, or distracted, or tired, or on a planet with mind-altering gas. This points to an ambiguity noted at the start of section 2, following Kripke, I have been acting as if in all relevant cases where Ludwig is asked "what is  $n + k$ ?", he responds in a uniform fashion. But this is silly: the class of situations in which Ludwig can be asked about " $n + k$ " is large and highly heterogeneous. (Warren 2018: 13)

Warren takes this fact into consideration and uses it to draw a distinction that is relevant for

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<sup>46</sup>In response to this kind of worry, it's natural for someone to think of the competence-performance distinction in Chomsky (1965: 3). He distinguishes between the knowledge that a speaker has of their language and the application of that knowledge in the speaker's language production. The application of a speaker's knowledge may be hostage to all sorts of extraneous factors like limitations of memory and distractions in a speaker's environment. Given the distinction, we might think of mistakes merely as cases where an agent's performance does not reflect their underlying competence or linguistic knowledge. But whether this distinction can help us to resolve the rule-following issues will depend on how a speaker's linguistic knowledge or underlying competence is characterized. Is it propositional knowledge? Or something to be understood in purely dispositional terms? Perhaps this distinction can be employed in a response to the worries in Kripke's Wittgenstein, but the devil will always be in the details. In the main text, I'll just be focusing on Warren's view which may provide one way of thinking about what (logical or arithmetic) competence ultimately amounts to.

understanding his response to the error issue. He distinguishes between “specific” and “A-general” dispositions. He defines specific dispositions in the following way.

(SPECIFIC)  $S$  has a specific disposition to  $\phi$  in maximally specific situation  $w$ , if in  $w$ ,  
 $S \phi$ s

Regarding “A-general” dispositions, he says

(A -GENERAL)  $S$  has an A-general disposition to  $\phi$  in a class of situations  $C$ , if in any  
 specific situation  $w$  in  $C$ ,  $S \phi$ s

Warren mentions that an individual can't have an A-general disposition to respond with the sum in all situations where he responds to a particular arithmetic inquiry (because the class is too large and undisciplined). He says

while Ludwig surely has specific dispositions to reply with “ $m$ ” in many specific situations in which he is asked about “ $n + k$ ”, it is highly unlikely that he, or any of us, has an A-general disposition to respond with “ $m$ ” (or anything else) in the class of situations in which he is asked about “ $n + k$ ”, since this requires that he gives the very same answer in every situation in which he could be asked about “ $n + k$ ”. But that class of situations is far too unruly for this. (Warren 2018: 14)

Taking the lead from other theorists (e.g. Shogenji), Warren invokes a notion of normal situations to restrict the class of relevant situations. Warren notes that some theories that invoke normal situations try to characterize normalcy in a way that rules out the possibility of an agent giving the incorrect answer in normal situations. But Warren does not try to do this. He says that normal situations are ones where an agent has neither internal nor external factors interfering with their general cognitive functioning. This will rule out situations where an agent is e.g. drunk or tired. He says

(NORMALCY) A situation  $w$  is normal with respect to  $S$  just in case neither external nor internal factors are interfering with  $S$ 's general cognitive functioning in  $w$

Obviously, this is somewhat vague and hazy. That is fine, because unlike others in the literature who use normalcy or related notions, I do not claim that errors are impossible in normal situations. (Warren 2018: 15)

Warren says that it isn't necessary for errors to be eliminated in normal situations because all that is needed is for an agent's dispositions to distinguish between the correct and incorrect cases.

To solve the problem of error, all that really matters is that our dispositions clearly distinguish the “correct” answer from “incorrect” answers, and this doesn't require perfection. (Warren 2018: 15)

To show how this can be done, he introduces a notion of an “M-general” disposition. An agent has an M-general disposition to  $\phi$  in a class of situations just in case they  $\phi$  in an “overwhelming majority” of those situations. He says

It is enough that the “correct” answer is given in the *vast majority* of normal situations. Accordingly, let's introduce a weaker type of general disposition to contrast with the more standard notion of A-generality:

(M-GENERAL)  $S$  has an  $M$ -general disposition to  $\phi$  in a class of situations  $C$ , if in the overwhelming majority of specific situations  $w$  in  $C$ ,  $S \phi$ s (Warren 2018: 15)

For every addition query that an agent might respond to, and for every stage where the agent needs to make a simple computation to deliver a response to the query, we can consider the class of normal situations where the agent performs a simple computation at that stage. The idea is that the agent has an  $M$ -general disposition to execute the operation that is required by the addition algorithm in this class. The agent can be said to have the requisite  $M$ -general disposition because the agent executes the operation required by the addition algorithm in the overwhelming majority of situations in the class. Since the situations are stipulated to be ones that are normal (ruling out internal and external interference), it is supposed to be unproblematic that an agent will respond in alignment with the addition algorithm in the overwhelming majority of the relevant situations.

Warren thinks that with these elements put into place, the issue of error has already been resolved. But he thinks that there is reason to mention a further factor that might be used to screen out errors. He says

I think it plausible that if we fill out Ludwig's disposition table by entering a numeral “ $m$ ” for row  $n$  and column  $k$  just in case Ludwig has an  $M$ -general disposition to respond with “ $m$ ” when queried about “ $n + k$ ” in normal situations, we will *already* have eliminated errors. But even intuitively, this is not the only factor that we use to screen out errors, so it is worth adding one more screen. (Warren 2018: 15)

The final screen concerns the idea that we can sometimes identify errors by checking and rechecking our responses. He uses the term “stable” to describe a situation where someone goes through

a process of checking and rechecking their answers and continues to get the same result.<sup>47</sup> He says

This final screen involves factoring in what happens when Ludwig repeats the process, in effect checking and rechecking his work and retracting previously given answers when they fail the check. We can dramatize this by imagining that in each normal situation in which Ludwig is given an addition problem, he is asked to independently check and recheck his work numerous times. If this sequence of checks and rechecks tends to produce the same answer over and over again, let's say that the answer is *stable*. (Warren 2018: 16)

He also provides a more idealized characterization of what it means for a response  $\alpha$  to be stable in terms of whether the ratio of non- $\alpha$  to  $\alpha$  answers tends towards zero.

(STABILITY) An answer  $\alpha$  to a question is *stable* just in case as the number of independent repeated trials increases, the ratio of non- $\alpha$  answers to  $\alpha$  answers tends to zero (Warren 2018: 16)

Warren does not think that the notion of stability alone can resolve the issue concerning error, and he notes a standard criticism of views that try to resolve the issue by citing a notion like stability. According to that criticism, stability does not address the issue because a nervous agent may produce less and less accurate responses as they continue to check and re-check. But Warren notes that the notion of normalcy can be used to siphon out cases like this because nervousness will count as an interference factor. So, he thinks that stability, in conjunction with the other features of his view, will resolve the error issue. He says

Could stability beat back the problem of error all on its own? Probably not, for as some defenders of Kripkenstein have pointed out, for those who are nervous or tired or drunk, answers might tend to get less and less accurate as they check their work. In these cases, presumably, no stable answer will be forthcoming (or an intuitively incorrect answer will be stable). However, normal situations are tailor-made to eliminate these problem cases, so while normalcy alone may not solve the problem of error, and stability alone may not either, together they surely do *if used in conjunction with M-generality as opposed to A-generality and when considering only simple dispositions*. This is a multiple screenings approach: there is no single factor that screens out all errors, the work is done by several factors working in tandem. (Warren 2018: 16)

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<sup>47</sup>Warren also discusses the possibility citing an agent's dispositions to accept corrections from other agents, but aims to address the puzzle without reference to such dispositions.

### How the elements of Warren's view fit together

The elements of Warren's view are connected in a way, so that the resolution of the finitude issue depends on the resolution of the error issue. To understand this, it's key to notice that Warren's resolution of the finitude issue is based on the notion of composite dispositions, and composite dispositions are understood in terms of the iterated application of an agent's linked singular dispositions. Since the error issue can be raised at the level of singular dispositions, the error issue has to be resolved in order for the iterated application of linked singular dispositions to get the right result. Warren indicates this dependency when he describes the error issue as one that can be raised after his presentation of the solution to the finitude issue. He says

I argued above that Ludwig's disposition to execute the addition algorithm in response to any "+"-query, understood in terms of his composite dispositions, solves the problem of finitude. In response, Kripke now asks: when Ludwig makes a supposed mistake in applying this algorithm, what determines that he is making a mistake in executing the addition algorithm instead of *correctly* executing some deviant quaddition algorithm? (Warren 2018: 16)

According to Warren, this worry is addressed because the issue of error is resolved in the case of singular dispositions. He says

Fortunately, the solutions to the respective extensional challenges combine to provide a non-question begging account. Ludwig is computing the addition function and not the quaddition function because his composite dispositions themselves ultimately consist of various singular dispositions, for which we have already solved the problem of error. We should require that all of these composing singular dispositions are themselves *M*-general and stable over normal situations. So when Ludwig slips up and forgets to carry the "1" in the 17th step of an addition problem, he is making a mistake in computing the sum, rather than correctly computing the quum, because he has a stable, *M*-general disposition to carry the "1" in the 17th step of this problem in normal situations. If he were computing the quaddition function, he wouldn't have this disposition. So as long as both complex and composite dispositions are characterized in terms of simple and singular dispositions that avoid error, no further problems of error or indeterminacy will arise at a higher-level. (Warren 2018: 17)

So, the the various elements of Warren's view come together to form a response to both the finitude and the error puzzle. The finitude issue is addressed via the notion of composite dispositions. The error issue is addressed in the case of linked singular dispositions, and this is supposed to carry over as a solution for the error issue in the case of composite dispositions (given

the way that composite dispositions depend on singular dispositions). In the following sections, I'll make some critical comments.

### The indeterminacy of $M$ -general dispositions in non-finite classes

For someone to have an  $M$ -general disposition to  $\phi$  in a class of situations, they have to  $\phi$  in the “overwhelming majority” of those situations. But what does it mean for an agent to  $\phi$  in the overwhelming majority of situations when the relevant class of situations is infinite? A natural way of modeling the notion of a majority is with something like a relative frequency where the numerator is greater than half the denominator. But the relative frequency will be undefined in this case because the numerator and denominator will have infinite values. This is problematic because it makes it so that there is no fact of the matter about whether the numerator is greater than half the denominator. And if  $M$ -general dispositions are defined in terms of this undefined value, there will likewise be no fact of the matter about whether the agent has the relevant  $M$ -general dispositions. This only reinforces the skeptic's contention that there is no fact about an agent that determines whether they mean plus by “plus”.

Warren makes a comment that could be seen as a response to this criticism when he discusses  $M$ -general dispositions. He notes that the relevant class of situations can be infinite in the case of addition and that therefore the notion of  $M$ -general dispositions isn't perfectly determinate.

We aren't literally counting situations here, so this notion isn't perfectly determinate, especially since  $C$  can be infinite (Warren 2018: 15)

It seems to me that this compromises the usefulness of  $M$ -general dispositions for resolving the issue of error, but Warren follows up with a comment which aims to provide some insight into what it would mean for an agent to have an  $M$ -general disposition to  $\phi$  (even when the relevant class of situations isn't finite). Without any qualification about the cardinality of the relevant class, Warren says that when an agent has an  $M$ -general disposition to  $\phi$  in a class of situations, it will be rational to be nearly certain that the agent will  $\phi$  in an arbitrary situation from that class. He says

when  $S$  has an  $M$ -general disposition to  $\phi$  in  $C$ , given an arbitrary situation  $w$  in  $C$ , it

is rational to be nearly certain that  $S \phi$ s in  $w$ . (Warren 2018: 15)

Whether this avoids the indeterminacy issue would seem to depend on whether the notion of rational certainty allows for determine answers in the relevant cases. It's not clear how this would work if we follow the view in Field (2009a) where notions of epistemic evaluation are not straightforwardly factual.<sup>48</sup> But even if the indeterminacy issue is addressed here, the implementation of a notion of rational certainty is not acceptable in this context. The notion of rational certainty is an epistemic notion, and is therefore not appropriate to employ in a response to Kripke's puzzle because Kripke explicitly characterizes his puzzle in metaphysical terms. Kripke says

the problem may appear to be epistemological — how can anyone know which of these I meant? Given, however, that everything in my mental history is compatible both with the conclusion that I meant plus and with the conclusion that I meant quus, it is clear that the sceptical challenge is not really an epistemological one. It purports to show that nothing in my mental history of past behavior — not even what an omniscient God would know — could establish whether I meant plus or quus. (Kripke 1982: 21)

If rational certainty were an acceptable notion to employ in response to Kripke's puzzle, then the puzzle could presumably be addressed by providing an account of how we are epistemically justified in believing that some agent means plus by "+". But such a response misses the mark if Kripke is right that his puzzle is metaphysical in nature.<sup>49</sup> Moreover, if a notion of rational certainty is doing the heavy lifting in the theory, then it's not clear why any appeal needs to be made to dispositions or normal situations in the first place; all we would need to do is give an account of how we are rationally certain or epistemically justified in believing that an agent means what is intuitively meant by their use of "plus".<sup>50</sup>

I don't think the matter changes with the introduction of the notion of stability. Since we are considering non-finite classes, there will be non-finitely many situations where an agent stably

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<sup>48</sup>Because we might imagine a skeptic who agrees with us on all the relevant facts about an agent, but who isn't rationally certain that the agent will add in an arbitrary normal situation. The disagreement wouldn't be straightforwardly factual in the sense previously described from Field; there wouldn't be a norm independent notion of rationality that privileged our perspective over the skeptic's perspective.

<sup>49</sup>I don't mean to presume that epistemic justification and rational certainty should be treated on a par. But if Kripke's puzzle is supposed to be metaphysical (whatever that amounts to), it can't be addressed in terms of what we are justified in believing or in terms of what we are rationally certain of.

<sup>50</sup>There are even more reasons to think that a response in terms of rational certainty doesn't address the issue that is being raised by Kripke. The skeptic in Kripke's dialogue asks what fact about an agent could constitute their meaning plus as opposed to quus. But it might very well be rational to be nearly certain that an agent means plus by "plus" even if there is no fact of the matter about whether they mean plus or quus.

responds with the answer that is intuitively correct. But for some answer  $\delta$ , that is intuitively incorrect, there will also be non-finitely many situations where the agent stably responds with  $\delta$ . In other words, there will be just as many situations where the ratio of non- $\delta$  answers to  $\delta$  answers tends towards 0. And given that normal situations fail to rule out cases where an agent makes mistakes, they won't rule out cases where, even when there are no interfering factors, an agent stably responds with  $\delta$ . So stability and normality do not alter the main point here (regarding the indeterminacy of  $M$ -general dispositions in non-finite classes).

### The relevance of $M$ -general dispositions

Suppose for the sake of argument that there is no problem with talking about overwhelming majorities in the relevant non-finite classes. There will still be a question about how  $M$ -general dispositions play a role in constituting the facts about what rules an agent follows. Even if we can define what it is for a speaker to have an  $M$ -general disposition, a skeptic might define an alternative disposition. For example, instead of making an appeal to what happens in the majority of a non-finite class of situations, a skeptic might make appeal to what happens second most often in a non-finite class of situations. Paralleling the structure of the definition of  $M$ -general dispositions, the skeptic can say that a speaker has an  $LM$ -general disposition to  $\phi$  in a class of situations  $C$  just in case  $\phi$ -ing is what the agent does second most often in the situations in  $C$ .

( $LM$ -GENERAL)  $S$  has an  $LM$ -general disposition to  $\phi$  in a class of situations  $C$ , if  $\phi$ -ing is what the agent does second most often in situations  $w$  in  $C$

If an agent can have an  $M$ -general disposition, then there shouldn't be any controversy over whether they can have an  $LM$ -general disposition as well. But then the skeptic can ask about the relevance of these dispositions. For example, the skeptic can ask what fact about an agent determines whether their  $M$ -general as opposed to their  $LM$ -general disposition plays a role in constituting the facts about what rules they follow?

So it isn't clear how this leaves us in a position which is relevantly different from the original skeptical puzzle. The skeptic originally asked what fact about an agent determines whether they mean plus as opposed to quus, but now the skeptic can simply ask what fact about an agent

determines whether the rule they follow is constituted by their *M*-general as opposed to *LM*-general disposition. There's nothing in the account that tells us this other than that *M*-general dispositions happen in more as opposed to less possible cases.<sup>51</sup>

### **the output of *M*-general dispositions**

Still granting, for the sake of argument, that we can make sense of “overwhelming majorities” in the relevant non-finite classes, there are still further complications. Warren notes that in many cases an agent won't proceed in accordance with addition (for various reasons ranging from anxiety to fatigue). The restriction to normal situations is supposed to exclude these kinds of cases, but there are still other cases where an agent may not respond in accordance with addition. For example, it is not clear that cases of deception can be ruled out via Warren's definition of normalcy because there may be no internal or external interference in a case where an agent is being deceptive. Perhaps it could be stipulated that these types of cases are to be excluded, but there are other cases where there is intuitively no internal or external interference but an agent nonetheless fails to proceed in accordance with addition. For example, an agent may fail to proceed in accordance with addition for precisely the reason that there is no internal or external influence on their behavior; an agent may fail to proceed in accordance with addition because they are bored. Perhaps there is a way of characterizing boredom where it always counts as a case of internal interference, but there are other cases to consider as well. There are situations where an agent may simply change their goals in the middle of their computation of a response to an addition query. For example, they may, on a whim, begin to multiply at some point and it's not really clear to me how all these factors could be screened out without circularity (i.e. without saying something like “do not include situations where an agent starts by adding, but later does something else”). This is not to say that these kinds of cases couldn't be ruled out without circularity; it's just that some of the more obvious ways of ruling these kinds of cases out do involve circularity. Moreover, the case of changing goals could presumably be quite complicated to rule out because of the various ways that goals might be

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<sup>51</sup>There may be parallels between the issue here and issues regarding skepticism about induction where an induction skeptic challenges someone to justify an inductive inference method over a counter-inductive inference method. In the same way that induction might proceed as an inference that accords with what has happened in the overwhelming majority of observed cases, the induction skeptic might ask why we shouldn't infer in accordance with what has happened second most often in the observed cases.

realized in an agent's behavior (short term vs. long term, implicit vs. explicit, etc.).

### Shmiterated dispositions

Warren says that an agent " $S$  has a composite disposition to  $\phi$  in situation  $C$  iff  $\phi$ ing is (or would be) the output of the iterated application of  $S$ 's linked simple (or complex) dispositions, in  $C$ ". But if we can talk about the iterated application of an agent's linked dispositions, then a skeptic may seek the opportunity to talk in terms of the shmiterated (as opposed to iterated) application of an agent's linked dispositions. If there is both an iterated and shmiterated application of an agent's linked dispositions, then the linked dispositions by themselves will not determine that the agent is following a plus-like as opposed to a quus-like rule.<sup>52</sup>

### Whether $M$ -general dispositions can be based in facts about an agent

For Warren, an agent can have an  $M$ -general disposition to make a simple computation that accords with the demands of the addition function because they make this computation in sufficiently many of the relevant normal situations. But this doesn't change the basic dialectical situation that Kripke sets up in WRPL because we can ask "is there a fact about the agent that determines whether he makes this computation (as opposed to some other) in sufficiently many normal situations"? It's not appropriate to respond to this question by saying that the agent has the relevant  $M$ -general disposition because an agent having the relevant  $M$ -general disposition just is the fact that the agent makes this computation in sufficiently many normal situations.<sup>53</sup>

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<sup>52</sup>Perhaps this objection is only fixating on a particular way that Warren has phrased his thesis. The objection plays on the phrase "iterated application of linked singular dispositions", but perhaps Warren does not really need to talk in terms of how linked singular dispositions are applied. That might be the case if the linked dispositions by themselves will determine the right extension for indefinitely many cases. But in that case, there shouldn't be any need to talk about the "iterated" application of an agent's linked dispositions. Or, at the very least, if the account is going to be characterized in terms of the "iterated" application of linked dispositions, then it may be advisable to say something about why pesky questions regarding the "shmiterated" application of the relevant dispositions are inappropriate.

<sup>53</sup>I'm supposing in this objection that the facts about how an agent would calculate in the range of possible normal situations should be explained in terms of facts about how the agent's actual, and not merely possible, brain, or mind, is set up. This may be thought of as analogous to a view where the facts about what happens to salt, when it's dropped in water, in possible normal situations, are explained in terms of the underlying structure of salt. Does Warren hold a view like this where facts about what an agent would do in possible normal situations are explained in terms of the underlying structure of the agent's mind? He says that the "unbounded execution" of a computation process is "encoded" in an agent's dispositions. But if having an  $M$ -general disposition is nothing over and above a fact about what you would do in a class of normal situations, then we can ask whether there is something that

## Concluding Segment

I argued that a vicious regress results for forms of logical pluralism that are based on Field's evaluative relativism. In the exegesis, I explained both his relativistic conception of evaluative claims and his evaluative conception of validity attribution. I suggested that there is more than one way that these two elements of Field's view might be combined to formulate a version of logical pluralism, and I argued that a vicious regress will result independently of certain assumptions about the scope of the resulting relativism about logic. Issues regarding the aim of vicious regress arguments were also shown to be relevant. In particular, some of the objections were premised on the idea that the regress argument might only apply to a form of policy relativism about logic where the policies are understood as truth evaluable (or somehow representational). These objections highlighted certain assumptions in the regress argument that needed to be defended, and they are also related to a point in chapter two that I mentioned I would come back to concerning implicit conventionalism and vicious regresses. One of the potentially advantageous features of an implicit conventionalism about logical truth was that it might avoid vicious regress worries. But the points here should show that it cannot be taken for granted that vicious regress worries will be avoided when conventions are understood in purely dispositional terms.

I also raised an underdetermination issue about versions of logical pluralism that are based on a relativism to policies. I looked at a way of understanding what it means for an agent to follow a policy where this is explained in terms of facts about their dispositions. But if validity attributions get truth values only relative to the policies that an agent follows, and following a policy is explained in terms of dispositions, then facts about an agent's dispositions will need to determine

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explains why you have a particular M-general disposition. One answer is to say that it's just a primitive fact. If that is right, then having an M-general disposition isn't explained by how your mind is structured in the actual world, and then it wouldn't be appropriate to say that the addition function is encoded in the agents brain (or mind). Another answer would be to say that having an M-general disposition is explained in terms of how your brain (or mind) is structured in the actual world. But then it's not clear how the original dialectical situation has been changed. What fact about my mind determines whether I have an M-general disposition to add as opposed to quad? In any case, it may just be that Warren thinks it is fair to respond to Kripke by citing facts about how an agent merely possibly (as opposed to actually) is. The skeptic in Kripke's dialogue asks "whether there is any *fact* that I meant plus, not quus." (Kripke 1982: 11). In this passage there is no explicit restriction to facts that are not merely possible, but it may be more natural to interpret the challenge in Kripke's Wittgenstein as one where only actual (and not merely possible) facts may be cited in a solution. Whether or not that is the right way to understand the challenge in Kripke's Wittgenstein, it would be interesting if a disposition-theoretic response to Kripke's puzzle implied that an agent's merely possible computations in normal situations could not be explained in terms of facts about how the agent's actual brain (or mind) is set up.

facts about what policies the agent follows. I looked at two dispositional theories (from Shogenji and Warren) that aimed to account for how dispositions could play this policy determining role, and argued that neither is successful. This is the other point that I mentioned I would come back to in chapter two when I discussed Warren's implicit conventionalist thesis about logical truth. So the point about underdetermination is related to the issue about whether conventionalism about logical truth might be salvaged by a conception of conventions where they are understood as implicit. Even if Quine's worries about the idea of implicit conventions being explanatorily idle can be overcome, underdetermination issues cast doubt on the viability of an implicit conventionalist metaphysics for logic because facts about an agent's dispositions will underdetermine truth values for logical truths regardless of whether any vicious regress can be avoided.

## Chapter 6

# Shapiro's Logical Pluralism

Stewart Shapiro (2014) defends a version of logical pluralism that is also based on a form of relativism. One of the things that is distinctive about Shapiro's logical pluralism is that it is based on an underlying pluralism in the philosophy of math. I'll give an exegesis of Shapiro's view in section 6.1. It will be divided into two parts because Shapiro's account has both a semantic and a non-semantic component. I'll describe the non-semantic component of Shapiro's view in 6.1.1, and I'll describe the semantic component of Shapiro's view in 6.1.2. Section 6.1.2 will be organized into two unnumbered subsections. The first covers Shapiro's view on the semantics of logical connectives, and the second covers Shapiro's view on the semantics of expressions like "logically valid". Shapiro describes his view about the semantics of "logically valid" as a form of indexical contextualism.<sup>1</sup>

In section 6.2, I'll argue that Carrollian regress points can be cited to formulate a criticism of Shapiro's logical pluralism. In 6.2.1-2, I'll develop the puzzle with respect to the non-semantic characterization of Shapiro's view. I'll also argue (in 6.2.3) that the puzzle can still be developed when the details of Shapiro's semantic theory are taken into consideration. In section 6.2.4-5, I'll argue that the regress for Shapiro's view should be understood as vicious. In 6.2.6, I'll argue that a vicious regress could still be developed if Shapiro's views were understood in terms of a non-

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<sup>1</sup>The characterization of Shapiro's views on the meaning of "logically valid" (and the logical connectives) has to be understood relative to certain qualifications he makes regarding indeterminacy of linguistic meaning. I'll discuss these qualifications below.

indexical contextualism about “logically valid”. I’ll end section 6.2 with some comments on the extent of Shapiro’s relativism (in 6.2.7).

In section 6.3, I’ll argue that Shapiro’s view also faces an underdetermination puzzle. In this section, there will be many points of overlap with what was discussed in 5.3, but the details will depend on Shapiro’s views on the nature of contexts (and the theoretical role that contexts play in his semantic theory).

## 6.1 Exegesis of Shapiro

As mentioned, I’ll divide the exegetical points into two main sections. Section 6.1.1 will cover aspects of Shapiro’s logical pluralism that are related to his views in philosophy of math, and the points in this section will not primarily concern anything regarding the semantics of linguistic expressions. Afterwards, in section 6.1.2, I’ll discuss details of Shapiro’s view that explicitly concern the semantics or meaning of words like “valid” and “not”.

### 6.1.1 Exegesis of non-semantic aspects of Shapiro’s Logical Pluralism

Shapiro’s logical pluralism is based on the idea that there are perfectly legitimate non-classical mathematical theories. He cites Heyting arithmetic with Church’s thesis (among other theories) as an example.

[T]he prime exemplars will be the intuitionistic theories — Heyting arithmetic with Church’s thesis, intuitionistic analysis, and smooth infinitesimal analysis (Shapiro 2014: 88)

Heyting arithmetic can be thought of as Peano Arithmetic with intuitionistic (rather than classical) proof rules.<sup>2</sup> For present purposes, what matters with respect to Church’s thesis is that if you add it to Peano Arithmetic (with classical proof rules), inconsistencies can be derived. Shapiro notes that these theories have a sense of legitimacy since mathematicians work with them and prove interesting results about them.

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<sup>2</sup>Or Peano Arithmetic with an intuitionistic semantics for the logical connectives.

(HA+CT) has been well-studied by mathematicians, of all logical persuasions, and I presume that they would recognize their subject when they see it. (Shapiro 2014: 69)

But these theories also have a sense of legitimacy that is given in terms of a philosophical view in mathematics that Shapiro refers to as the “Hilbertian” perspective. On the Hilbertian perspective, the legitimacy of mathematical axioms is not based on whether they accurately capture a pre-theoretic subject matter, but rather on whether the axioms are consistent.<sup>3</sup> On the Hilbertian perspective, any consistent set of axioms is said to pick out a mathematical structure. As Shapiro describes the Hilbertian perspective, the axioms of a mathematical theory

are not to be understood as self-evident truths about an intuitive or pre-theoretic subject matter, but instead as implicit definitions that characterize a structure. (Shapiro 2014: 66)

and

any consistent axiomatization characterizes a structure, something at least potentially worthy of mathematical study. (Shapiro 2014: 65)

This provides the basis for understanding Shapiro's pluralism about logic. The idea is that to the extent that these non-classical mathematical theories are legitimate, a sense of legitimacy has to be granted to their associated logics.<sup>4</sup>

In particular, there are a number of interesting and important mathematical theories that employ a non-classical logic, and are rendered inconsistent if classical logic is imposed. This suggests that logical consequence is relative to a theory or a structure. (Shapiro 2014: 19)

In the above passage, it can be seen that Shapiro's logical pluralism is based on a kind of relativism. Relative to classical mathematical theories, an instance of the law of excluded middle is a logical consequence of the empty set. But relative to intuitionistic theories (e.g. HA+CT), an instance of the law of excluded middle is not a logical consequence of the empty set. To provide

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<sup>3</sup>A couple things should be mentioned about the notion of consistency that Shapiro employs in his project. It should not be understood as a once and for all notion of consistency. The idea is that while HA+CT may be consistent according to an intuitionistic logic, it is not consistent according to classical logic. Non-triviality can also play the role of consistency here to allow for paraconsistent structures.

<sup>4</sup>A key question to ask is what kind of legitimacy the associated logics have. Priest distinguishes between pure and applied logic in *Doubt Truth to Be a Liar* and draws an analogy between pure and applied geometry. (2005, ch. 12) I would grant that any logic is legitimate qua pure logic, but pluralism about pure logic does not imply pluralism for applied logic.

a metaphysical characterization of relativism, Shapiro cites Wright's (2008) account of relativism and he borrows Wright's notion of "folk relativism".

Our first and primary sense [of] "relativism" about a given subject matter (or word)  $\Phi$  is what Crispin Wright [2008, 158] calls folk relativism. Its primary slogan is: "There is no such thing as simply being  $\Phi$ ." (Shapiro 2014: 7)<sup>5</sup>

So Shapiro takes the validity of arguments in mathematics to be relative in the sense of Wright's folk relativism.

in the slogan of folk-relativism, there is no such thing as "simply being valid." Rather there is validity-in-classical-theories, validity-in-intuitionistic-theories, etc. (Shapiro 2014: 115)

### 6.1.2 Exegesis of the semantic aspects of Shapiro's Logical Pluralism

Shapiro's relativism does not have any specific semantic consequences, but it does have the following general semantic implication.

If  $\Phi$  is relative, in this sense, then in order to get a truth-value for a statement in the form " $a$  is  $\Phi$ ," one must explicitly or implicitly indicate something else. (Shapiro 2014: 7)

If we apply this to Shapiro's relativism about logical validity, it means that statements involving expressions like "valid" or "logical consequence", at least when occurring in mathematical practice, will need a parameter to get a truth value.

There is more than one way to account for the details about how an expression like "valid" might depend on a parameter for its extension.<sup>6</sup> For example, it has to be spelled out what a parameter is and how the extension of the expression depends on it. Shapiro discusses this in terms of an indexical contextualist semantic theory for "valid", but the details of his view are intertwined with his views about the semantics of logical connectives. In order to explain Shapiro's overall semantic theory, I will first discuss his view on the semantics of logical connectives, and then I'll discuss his view on the semantics of expressions like "valid".

<sup>5</sup>The first bracket is not in the original text, but added to make the passage grammatical.

<sup>6</sup>Below I'll talk as though expressions like "valid" have extensions, but this might all be rephrased in terms of sentences having truth values. I don't mean to indicate that Shapiro favors one way of talking over the other.

**Shapiro's view on the semantics of logical connectives**

So what does Shapiro say about logical connectives like “or”? Will they mean the same thing in both intuitionistic and classical mathematics? For Shapiro, a question like this cannot be answered without qualification because he thinks expressions like “means the same” involve a kind of interest relativity.<sup>7</sup>

[T]alk of whether the meaning of the logical terminology is the same or different in different contexts/theories is itself context-sensitive and, moreover, interest-relative—in part because such talk is vague. (Shapiro 2014: 92)

There is just no fact of the matter, independent of conversational goals, whether the meaning is the same or different, in the different languages/theories. (Shapiro 2014: 108)

So, on Shapiro's view, at least relative to certain conversational goals, logical connectives (or sentences formed with them) can have the same meaning across classical and non-classical mathematical contexts.

On the supposition that the logical connectives mean the same across mathematical contexts, Shapiro describes his view of the logical connectives as a form of “non-indexical contextualism”.<sup>8</sup> According to Shapiro's characterization of non-indexical contextualism about a given expression, the content of that expression will be the same across contexts, but its extension can vary (depending on features of the context of utterance).

Non-indexical contextualism about a given term is the view that its content does not vary from one context to another, but its extension can so vary. The context of utterance provides the relevant parameter for fixing this extension. (Shapiro 2014: 112)

So relative to certain conversational goals, the logical connectives will have the same content across mathematical contexts, but vary their extension.

What of the logical terms themselves? By the supposition in play throughout this section, those have the same meaning in every theory in which they are used. So,

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<sup>7</sup>Shapiro's views about this are motivated by considerations concerning Quine. For example, he says “For largely Quinean reasons, I am skeptical that we can assign unique, sharp meanings to each sentence, let alone each word, even in the relatively pristine case of live mathematics”. (2014: 92)

<sup>8</sup>This is on the assumption that meaning is identified with a theoretical notion of content.

assuming that “content” is the same as “meaning” here, we do not have a form of indexical contextualism. Indeed, the present supposition just is the rejection of indexical contextualism for the logical terms. From the eclectic perspective of this project, the “extensions” of the logical terms presumably *do* vary in the different contexts-cum-theories, at least to the extent that it makes sense to speak of the extension of a logical term. In any case, an instance of, say, excluded middle is correctly deemed valid in classical contexts and correctly deemed invalid (although not outright false) in intuitionistic contexts—whatever this says about the “extensions” of “or” and “not”. (Shapiro 2014: 119)<sup>9</sup>

### Shapiro's view on the semantics of expressions like “valid”

When discussing Shapiro's view about the meaning of expressions like “logical consequence” and “valid” in mathematical practice, I'll talk as though logical connectives have the same meaning across mathematical theories. This assumption may not always be appropriate if sameness of meaning is relative to goals, but Shapiro discusses his views about the meaning of expressions like “logical consequence” under the supposition that logical connectives have the same meaning.

Under our present supposition that logical terms have the same meaning in all of the contexts, we have at least a folk-relativism concerning “valid” and “logical consequence” and a corresponding pluralism. For example, let  $\Phi$  be a statement of the intermediate value theorem and consider the instance of excluded middle  $\Phi \vee \neg \Phi$ . On the assumption in question, that sentence has the same meaning in classical analysis and in intuitionistic analysis. It expresses the same proposition in both contexts, if you will. Yet  $\Phi \vee \neg \Phi$  is a logical consequence of the empty set in the former and not a logical consequence of the empty set in the latter. In other words the same argument is valid in one context and invalid in another. (Shapiro 2014: 115)

Shapiro's relativism about validity is referred to in the above passage, and he gives a specific account of how expressions like “valid” will depend on parameters for their extensions. The parameters are understood in terms of “logics” that are associated with mathematical contexts. Shapiro describes mathematical contexts as things that will include a mathematical theory, and mathematical theories are described as things that come with a logic.

I propose that each “context” includes a specific mathematical theory or structure. It would be the mathematical theory being advanced at any given time by a mathematician or a group of mathematicians. In line with the foregoing eclectic orientation, each such

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<sup>9</sup>Shapiro refers to indexical contextualist theories in this passage, and I'll look at his characterization of this idea below.

context has a specific logic: classical logic for classical theories, intuitionistic logic for the intuitionistic ones, etc. (Shapiro 2014: 89)

So, on Shapiro's proposal, every context will include a theory, and every theory will include a logic (at least on the assumption that the context in question "involves something resembling deductive reasoning").

Every coherent perspective—every language, every form of life, every context—has an underlying logic, assuming only that it involves something resembling deductive reasoning. (Shapiro 2014: 5)

Presumably there will be a question about how a logic gets fixed on the basis of features that resemble deductive reasoning, but however the details of this get spelled out, the parameter of interest in a given case will be described as the "logic" of the context.

Given this specification of parameters, there is still a question about how the content or extension of an expression is supposed to depend on the parameters. Shapiro clarifies these details by reference to an "indexical contextualist" theory about the meaning of expressions like "valid" and "logical consequence".

we seem to be in for some sort of indexical contextualism for "valid" and "logical consequence," possibly with a covert variable for a logic, structure or theory. (Shapiro 2014: 118)

As Shapiro describes the view, an indexical contextualist semantic theory for "valid" will be one where the content of valid will vary depending on features of the context of use.

Indexical contextualism about a given term is the view that the content expressed by the term is different in different contexts of use. (Shapiro 2014: 112)

This results in a view where the content of "valid" can vary between two logicians who are advancing mathematical theories that have different underlying logics.

The classical logician says that classical reductio is valid; the intuitionist says that classical reductio is invalid. By the assumption of this section, they are talking about one and the same argument form. By the perspective of this study, both are correct. That is, both utterances are true, in their respective contexts. In line with the contextualism, they do not really disagree, since "valid" has a different content in their respective utterances. For the classicist, it is "classically-valid"; for the intuitionist, it

is “intuitionistically-valid.” (Shapiro 2014: 118)

So for this view of “valid,” when one mathematician says LEM is valid, and another says it is not valid, their respective uses of the word “valid” need not have the same content. The idea is that the content of “valid” can vary depending on the logic of the context of use. And since the logic of a context is explained in terms of the underlying features of a context that resemble deductive reasoning, this means that as those underlying features change, there can be a resultant change in the content of “valid”.<sup>10</sup>

Given all of this semantic apparatus, there are still questions about what it would mean for a context to involve something that resembles deductive reasoning. Answering these questions would involve theoretical issues about how to understand the nature of inference and deductive reasoning. But there is also a theoretical choice point regarding who's reasoning is relevant when it comes to determining the logic of a context of use. For example, someone could suppose that the logic of the context is based in an individual's reasoning (perhaps the agent of the context). But someone else might suppose that the logic of the context is based in the reasoning of a larger group or community (perhaps a community that is somehow tied to the agent of the context).

So there is a question about who's reasoning is relevant for determining the logic of a context of use. There is nothing problematic about this in and of itself. This type of theoretical

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<sup>10</sup>There is an issue here about parity of reasoning and theoretical uniformity. If the content of “valid” shifts across contexts of use with different reasoning methods, shouldn't the content of the logical connectives shift as well? Presumably reasoning methods would be just as relevant to the content of “valid” as they would be to the content of the logical connectives (and theoretical uniformity would demand that like cases be treated in a like fashion). But this would be a problem for Shapiro's view. If the content of “not” shifts across contexts of use (in a parallel fashion to “valid”), that would contradict any supposition that “not” means the same in classical and intuitionistic contexts. Presumably Shapiro's example of mathematicians discussing the intermediate value theorem is relevant here. That data gives an example where one and the same theorem is described as valid here, but not valid there, so it provides a prima facie example where “valid” shifts its content (but “not” doesn't). Nevertheless, these seem like peripheral technical usages of the relevant terms, and there are alternative ways of characterizing the data that don't give the same result. For example, the situation can be described as one where the sentence is provable in one system and not provable in another; the notion of content remaining identical is not essential to our description of what is going on. Moreover, even if the data is best described in a way favorable to Shapiro's hypothesis, it is still unclear that peripheral examples like this should cast doubt on the view that “valid” and “not” should be given a uniform meta-semantic analysis. A last point about this is that it may not be exactly right to say that “if reasoning methods shift the content of “valid” they therefore must also shift the content of “not””. A theorist might argue that a shift in the content of “valid” could happen that is due to a shift in the rules for “if...then...”. In a case like this, it could be supposed that the content of “not” will not shift at all. But it would still be a case where theoretical uniformity demands that the content of some logical constant shifts (in this case “if...then...”). In any case, problems of theoretical non-uniformity could be avoided if both the logical connectives and “valid” were given a non-indexical contextualist analysis. Then a shift in reasoning methods could affect each type of expression in an analogous way, but I will discuss the possibility of whether Shapiro could opt for a non-indexical contextualism about “valid” below.

choice point occurs in contextualist semantic theories more generally. For example, on an indexical contextualist semantics for epistemic modals there will be a question about whether the relevant epistemic standards belong to the speaker, some other contextually determined individual, or some contextually determined group. As far as I can see, based on the following passage, Shapiro leaves it undecided whether it is the reasoning of an individual or a group that matters.

I propose that each “context” includes a specific mathematical theory or structure. It would be the mathematical theory being advanced at any given time by a mathematician or a group of mathematicians. (Shapiro 2014: 89)

Theoretical choice points aside, this would fill in many of the details about how a context of use is supposed to determine content on Shapiro's indexical contextualism for “valid”. Suppose that different contexts of utterance are associated with different speakers who are advancing different theories (one PA and one HA) at the times of their respective utterances. Then each context will be associated with different methods of reasoning. Given this, the content of “valid”, relative to each context of use, can be determined by the methods of reasoning associated with that context of use. Then, depending on how we spell out the link between reasoning methods and linguistic content, “valid” could have one content in the PA context of use and another content in the HA context of use.<sup>11</sup> With these details in mind, I'll now turn to some critical points regarding Shapiro's view.

## 6.2 A Lewis Carroll-style puzzle for Shapiro's version of Logical Pluralism

In what follows, I'll argue that a Lewis Carroll-style puzzle can be raised for Shapiro's view. I'll first describe the issue in a way that concerns notions of arbitrariness rather than vicious regress. While a regress is what figures most prominently in Carroll's dialogue, I think the underlying point can be understood in terms of considerations of arbitrariness. The tortoise's initial plea for a further

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<sup>11</sup>I think it's important to note that it matters how the relation between reasoning methods and linguistic content is spelled out. Views can be distinguished depending on which reasoning methods they say are relevant for determining the content of a logical operator. For example, should only elimination and introduction rules determine the content for a disjunction operator? Will the absence or presence of an assumption of the law of excluded middle shift the meaning of “not”? Do so-called “meta-rules” factor into the meaning of logical connectives?

premise can be understood as inappropriate on the grounds that it would be arbitrary to think that claim  $C$  would be helpful if  $A$  and  $B$  weren't already enough. A regress can be developed because if  $A$  and  $B$  aren't sufficient by themselves, then  $A$ ,  $B$ , and  $C$  won't be sufficient by themselves either (because  $C$  doesn't add anything that is significantly different). After raising an issue for Shapiro's view where notions of arbitrariness are invoked, I'll also discuss how these points can be related to considerations of vicious regress.

### 6.2.1 A criticism of Shapiro's pluralism that is based on considerations of arbitrariness

Recall that there is no such thing as an argument being "simply valid" on Shapiro's view.

[T]here is no such thing as "simply being valid." Rather there is validity-in-classical-theories, validity-in-intuitionistic-theories, etc. (Shapiro 2014: 115)

If this is right, then in the following argument (K), the premise (K1) will not simply imply its conclusion, (KC).

(K1) Everything is zero or greater

so,

(KC) One is zero or greater

While the premise will not simply imply its conclusion, it is supposed to imply its conclusion relative to certain contexts. As noted before, on Shapiro's view, a context will include a logic. So presumably the idea is that the premise would imply its conclusion when relativized to a context that includes a classical logic for the quantifiers.

But if the premise taken by itself doesn't imply its conclusion, it's not clear why it would imply its conclusion when relativized to a context that includes a classical logic for the quantifiers. It may be tempting to say that classical quantified logic will admit a universal instantiation principle and to leave it at that, but it's worth thinking carefully about what a logic is. On a natural way of thinking about a logic, it will be thought of as a collection of axioms with at least one rule of inference. On a standard picture, axioms will be universally quantified claims, but in that respect,

they will be just like the previous premise “Everything is zero or greater”. So if the previous premise doesn't simply imply its conclusion, it would be arbitrary to say there is something that the axioms simply imply. And if the axioms don't simply imply anything, it's not clear why the previous premise would imply its conclusion when relativized to the axioms. Of course, given that we are thinking of a logic as a collection of axioms *plus at least one rule of inference*, it has to be asked whether a rule of inference could play a role in accounting for how (K1) would imply its conclusion when relativized to a context with a classical logic.

Whether a rule of inference would be of any help should depend on how a rule is characterized, and there are many ways to understand the idea of an inference rule. On one understanding, a rule of inference will be characterized as a universal claim. For example, a rule might be stated as something like “Every proof with a universal sentence can be extended by writing an instance of the universal sentence (respecting scope).” On this understanding of a rule, the original issue will still be present. If the previous premise (“Everything is zero or greater”) doesn't simply imply anything, it would be arbitrary to say that rules do (since on this interpretation, rules are just further universal claims). And if rules don't simply imply anything, it's not clear why (K1) would imply its conclusion relative to a rule.

A question of interest is whether rules could be understood in some other way that might change the matter. According to the previously considered conception of logical rules, where they are understood as universal claims, a rule is understood as something with a truth evaluable content. But as mentioned in previous chapters, there are conceptions of rules where they are not understood as having a truth evaluable content. For example, we might distinguish between a claim that the door is shut and a command to shut the door. The claim and the command may have some similarity in terms of their content, but the command is not truth evaluable. So logical rules might be understood on the model of commands. For example, as noted before, a universal instantiation principle might be associated with the following command (UI-com):

(UI-com) Close your beliefs under all relevant instances of UI.<sup>12</sup>

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<sup>12</sup>As in the previous chapter, when talking of beliefs being closed under an argument, I just mean that either the conclusion is accepted or at least one of the premises is rejected. Also, some finessing has to be done here because the relevant command can't be to either reject a universal claim or accept every instance of it. To the extent that it makes sense to characterize a UI rule as a command, it will have to have some kind of restricted scope regarding which instances of UI an agent is commanded to close their beliefs under. So the word “relevant” should

Could this imperatival characterization of rules change the main issue? I don't think so. If an imperatival characterization of rules is going to change the main issue, then we need to explain why (K1) would imply (KC) relative to an imperative (where the relevant imperative is something like (UI-com) above). If (K1) is going to imply (KC) relative to the imperative (UI-com), then following the imperative (UI-com) must require that one close their beliefs under (K). It's natural to think that following (UI-com) would require you to close your beliefs under (K) because (UI-com) concerns all relevant instances of UI, and (K) is a relevant instance of UI. But if Shapiro's relativism about logic is assumed, it just isn't true that following a command like (UI-com) will require an agent to close their beliefs under (K).

The reasoning is exactly the same as it was in section 5.2.1. In order to follow (UI-com), all I should need to do is make sure that for all relevant instances of UI, my beliefs are closed under them. But consider the following argument (L).

(L1) All relevant instances of UI are such that my beliefs are closed under them

so,

(LC) (K) is such that my beliefs are closed under it

On Shapiro's view, (K1) by itself won't imply (KC). But if (K1) by itself doesn't imply (KC), it would be arbitrary to say that (L1) by itself implies (LC) (because (K1) and (L1) are both just universal claims). And if (L1) by itself doesn't imply (LC), then my beliefs not being closed under (K) won't by itself constitute a counter example to (L1). But if my beliefs not being closed under (K) doesn't by itself constitute a counterexample to the claim that all relevant instances of UI are such that my beliefs are closed under them, then a command to close my beliefs under all relevant instances of UI won't by itself require that I close my beliefs under (K). And if the command (UI-com) doesn't by itself require me to close my beliefs under (K), then it's not clear why (K1) would imply (KC) relative to the command.

So, in effect, even if rules are characterized as commands, the relativism regarding what a claim implies will spill over into a relativism regarding what a command requires. If (K1) doesn't by

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be understood as marking the needed restriction. Often theorists talk about "permissions" as opposed to commands, but I'm considering commands rather than permissions because they are a paradigm of content bearing but non-truth evaluable entities.

itself imply its conclusion, then it won't imply its conclusion relative to an imperative like (UI-com) either. There are other ways of characterizing rules where rules are understood as neither truth evaluable nor representational, but since I discussed this issue in 5.2.1, I'll move on to discuss how this kind of point might be developed into a regress.

### 6.2.2 How the previous points can be developed in terms of a regress

I've already mentioned (in 3.4) that there is some awkwardness that results for attempts to develop a regress in terms of examples concerning UI. So I'll show how the same kinds of points can be developed into a regress, but I'll use modus ponens as an example. Consider the following instance instance of modus ponens, (A\*), that was discussed in the case of Beall and Restall (in 4.3.3).

(A1\*) Two and two is not five.

(A2\*) If two and two is not five, then two and two is four.

(AC\*) So, two and two is four.

Given Shapiro's view, (A\*) isn't simply valid. At best, it's valid relative to some logics (and not valid relative to others). But if (A\*) isn't simply valid, is it even possible to explain how (A\*) would be classically valid or valid relative to classical logic? I'll argue that the answer is no. The details will always depend on how the notion of classical validity is understood, but I'll develop the point in terms of a model-theoretic conception of classical validity. The point will be exactly the same as what was seen in the case of Beall and Restall. According to the model-theoretic conception of classical validity, an argument will be classically valid if it preserves truth in all Tarski models. In the case at hand, that means that if (A\*) preserves truth in all Tarski models, then (A\*) is classically valid. We can grant the antecedent of this conditional claim, and this will set us up with an explanation that can be regimented in terms of another instance of modus ponens (B\*).

(B1\*) (A\*) preserves truth in all Tarski models.

(B2\*) If (A\*) preserves truth in all Tarski models, then (A\*) is classically valid.

(BC\*) So, (A\*) is classically valid.

But if (A\*) isn't simply valid, then (B\*) isn't simply valid either. That means that the premises of (B\*) don't simply imply their conclusion. But if the premises of (B\*) don't simply imply their conclusion, then it's not clear how (B\*) could be a sufficient explanation of the fact that (A\*) is classically valid.

Someone might say that (B\*) is still a suitable explanation in spite of the fact that (B\*) isn't simply valid. They might say that even if (B\*) isn't simply valid, it still preserves truth. But even (B\*) does preserve truth, a regress can be developed in exactly the same way that was seen in 4.3.3. I won't repeat the entire argument here.

### 6.2.3 How the regress can be developed in terms of Shapiro's semantic theory

Before I explain why this sort of regress should be counted as vicious, I want to show how the same sort of regress can be developed in terms of the details of Shapiro's indexical contextualist theory. The points are the same as what was described in 4.4 regarding Caret's indexical contextualist version of logical pluralism so I'll refer back to this section at certain points so as to avoid repetition. There is a main reason why I want to show how a regress can be developed in terms of the details of Shapiro's semantic theory. Shapiro responds to an objection concerning vicious regress, and the details of his response are framed in terms of his semantic theory.

An indexical contextualist theory is supposed to give us an explanation of the truth conditions of sentences relative to contexts of use. In particular, it is supposed to do this in terms of a notion of content (where the content of an expression can be different relative to different contexts of use). This means that an indexical contextualist theory of "valid" should be able to give us an explanation of how an argument like (A\*) will fall under the extension of an expression like "valid" when that expression is used in a classical context. Moreover, the explanation of how (A\*) falls under the extension "valid" in a classical context of use should be given in terms of the content that "valid" has in a classical context of use.

It might be thought that once you have the content, the extension comes for free. For isn't it just part of the indexical contextualist theory that once the expression gets a content in a context of

use, the world is the only thing that needs to be consulted for the determination of an extension?<sup>13</sup> Normally there is no problem with this story about how extensions get determined in an indexical contextualist framework, but things cannot be that simple if Shapiro's relativism about logic is assumed.

On an indexical contextualist theory of "valid", the argument (A\*) is supposed to fall under the extension of "valid" in a classical context of use *c* iff (A\*) falls under the extension of the content of "valid" in *c*. Since *c* is a classical context of use, the content of "valid" in *c* will be something like *classically valid*.<sup>14</sup> When will (A\*) fall under the extension of such a content? (A\*) will fall under the extension of the content *classically valid* iff (A\*) is classically valid. In other words, (A\*) will fall under the extension of the content *classically valid* iff [In all Tarski models where the premises of (A\*) are true, so is the conclusion of (A\*)].<sup>15</sup> Since an indexical contextualist theory provides us with bi-conditionals like this, it allows us to regiment an explanation of how (A\*) falls under the extension of the content *classically valid* (e.g., in terms of the following argument (G\*)).

(G1\*) If [In all Tarski models where the premises of (A\*) are true, so is the conclusion of (A\*)], then (A\*) falls under the extension of the content *classically valid*.

(G2\*) In all Tarski models where the premises of (A\*) are true, so is the conclusion of (A\*).

(GC\*) So, (A\*) falls under the extension of the content *classically valid*.

This is what is delivered by the indexical contextualist theory. But if Shapiro's logical pluralism is true, the argument isn't simply valid. It's valid relative to some contexts, but not valid relative to others. But that means the indexical contextualist theory does not deliver an explanation of how "valid" gets an extension relative to a context.

In other words, even if you grant that "valid" can have a specific content like *classically*

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<sup>13</sup>Perhaps plus other things like times which can be built into the context of utterance.

<sup>14</sup>I'm using italics to talk about content here. Perhaps the italics should be thought of as something like a placeholder since content is a theoretical notion that can be articulated in more than one way. For another way of talking about the matter, it could be said that the content of "valid" (in a classical context of use) is something like the content of the more specific expression "classically valid". When Shapiro discusses the notion of content in indexical contextualist theories, he uses quotes (2014: 118), and my use of italics can be understood as playing the same role that the quotes are playing in Shapiro's discussion.

<sup>15</sup>This provides us with a bi-conditional that is like the one (Gy) in section 4.4.

*valid* in a classical context of use, this content is still insufficient to determine an extension (when Shapiro's relativism is assumed). The indexical contextualist story about how an extension is determined for the content *classically valid* is given in terms of an argument like (G\*). But on Shapiro's view, the premises of (G\*) don't simply imply their conclusion. The premises don't imply their conclusion absent further parameters. And the indexical contextualist theory doesn't give us any further parameters other than the ones that are mentioned explicitly in arguments like (G\*) (and (G) from 4.4.2). This means that no extension will be determined for the expression "valid" even after all the mechanics from the indexical contextualist theory have been introduced and exhausted.

Someone might suggest that even if (G\*) isn't simply valid, it may still nonetheless preserve truth. But then a response can be developed that is exactly like the one delivered in 4.4. Even if we grant that (G\*) is truth preserving, that amounts to nothing more than the fact that if the premises of (G\*) are true, then the conclusion of (G\*) is true. That gives us another instance of modus ponens, and we can grant that the premises of this further instance of modus ponens are true. But that doesn't add any explanatory value to the argument (G\*) because this further instance of modus ponens won't be simply valid either. Someone can suppose that the further instance of modus ponens preserves truth, but a regress will be generated in the same way that was seen in 4.4. So the specific contents provided by the indexical contextualist theory will be insufficient to play the role they are supposed to play in determining the extension of expressions like "valid".<sup>16</sup>

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<sup>16</sup>There is another way that a regress might be developed. Someone might suggest that even if we can't say that (A\*) simply falls under the extension of *classically valid*, we can still say that (A\*) falls under the extension of *classically valid* relative to a classical context. In other words, (G\*) may not be valid relative to certain non-classical logics, but it will surely be valid relative to a classical logic (and then we can say the conclusion of (G\*) holds relative to classical contexts.

But a regress can be developed for someone who takes a position like this. If relativizing to a classical context didn't determine an extension with the initial relativization, then there's no reason to think it will be helpful to relativize to a classical context a further time. Moreover, if relativizing to a further context was necessary at the first stage, then relativizing to a further context will be necessary at the second stage as well. And this reasoning can be continued indefinitely.

### 6.2.4 Why the regress should be counted as vicious

This type of regress will meet the conditions for viciousness described in Passmore (1961). Whether a regress counts as vicious in Passmore's sense depends on the manner in which the regress is generated (as was discussed in 4.2.2). It must be generated in a way where each stage places us in a position that leaves the original issue wholly unresolved.

In the indexical contextualism case, the original problem is explanatory in nature.  $(A^*)$  does not fall under the extension of "valid" independently of a context of use. But  $(A^*)$  is supposed to fall under the extension of "valid" relative to a classical context of use. The indexical contextualist theory purports to explain how this is possible by introducing a notion of content. Relative to a classical context of use, the expression "valid" is supposed to have the specific content *classically valid*. This specific content is supposed to have an extension, and the expression "valid" is supposed to inherit its extension from the extension of the content. But does this provide us with an explanation of how  $(A^*)$  falls under the specific content *classically valid*?

All the indexical contextualist theory tells us is this:  $(A^*)$  falls under the extension of the content iff  $(A^*)$  is classically valid. But that is unhelpful if Shapiro's relativism is assumed. What the indexical contextualist theory gives us is something like the argument  $(G^*)$ . But on Shapiro's view,  $(G^*)$  isn't simply valid. It will be valid relative to some contexts, but invalid relative to others. That makes it clear that the introduction of the indexical contextualist contents won't place us in a situation that is significantly different from the one we were originally in at the first stage. At the first stage, we had the expression "valid", and we couldn't say (absent further parameters) whether  $(A^*)$  falls under its extension. Part of the promise of indexical contextualism is that we would be able to explain how  $(A^*)$  falls under the extension of "valid" relative to a classical context. But if we relativize to classical contexts, that only gives us the specific content *classically valid*. But even with this specific content, we are still in a position where we would have to relativize to further parameters for an extension. That means the explanation introduces exactly the same issue that was in need of explanation. The expression "valid" didn't have an extension by itself, but the content of the expression "valid" in a classical context of use doesn't have an extension by itself either.

This is the same idea that is described in Passmore. Something is brought in to analyze or explain a phenomenon, but it doesn't advance the situation because it re-introduces the very same issue that was initially to be explained. Context dependent contents are brought in to explain how extensions get determined for expressions, but the context dependent contents don't have extensions either. In other words, the second stage would leave you in exactly the same position that you were originally in. And surely there isn't going to be some further stage that ultimately changes the matter. The same will hold for each subsequent stage.<sup>17</sup>

### 6.2.5 Shapiro on vicious regresses

As mentioned in the discussion of Passmore, one way of resisting the idea that there is a vicious regress is by saying that a certain stage is privileged. In other words, someone can argue that the explanation does not re-introduce the same worries that were originally to be explained. Some passages in Shapiro can be interpreted as making exactly this kind of response. In particular, he says assessment sensitive semantic theories of "valid" will (but his indexical contextualist theory won't) face a vicious regress. It will be worth looking at what he says about assessment sensitive theories to see why he thinks there is a difference.

On an assessment-sensitive view, as Shapiro describes it, an expression will have the same content across contexts of use, but its extension is not fixed by the context of use.

*Assesment-sensitive relativism* about a term agrees with non-indexical contextualism that the content of the term does not vary from one context of use to another (Shapiro 2014: 112)

Instead of having an expression's extension fixed by the context of utterance, the extension is fixed relative to contexts of assessment.

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<sup>17</sup>There may be a further auxiliary hypothesis that needs to be considered. Suppose someone were to argue that once you have an infinite series, you then have an adequate explanation. Perhaps they want to argue that each element in the series explains the previous element in the series. I won't pursue this option for response in much detail, but I will note that it is wrong to say that each stage explains the previous stage. There's no extension at the first stage. And relativizing to a classical context of use at the second stage doesn't explain how there could be an extension. The same will hold for each subsequent stage because none of those stages add anything that is significantly different from what was added at the second stage. Should the matter change when we consider an infinite hierarchy of logics (where A implies B according to L, according to L', according to L'', and so on)? I don't think there will be a difference. It's part of Shapiro's view that nothing simply follows, so nothing simply follows from the hierarchy either. There will never be an explanation even when we consider the totality of an infinite hierarchy of logics.

For the assessment-sensitive relativist, the extension of the term is determined from a *context of assessment*, which can differ from the context of utterance. (Shapiro 2014: 112)

An occurrence of a sentence will have a unique context of utterance, but it will not have a unique context of assessment. Part of the idea behind assessment-sensitive accounts, is that if they are correct for a linguistic expression, there is a practice where people evaluate the expression relative to standards of their own context of assessment (which may or may not be the same as the context of utterance).

Shapiro raises a question about whether certain explicitly relativized claims (that would figure into an “assessment-sensitive” semantics) have their truth values simply, or only relatively so. He says

Consider a statement in the form:

(A) *Pa* is true in context of assessment *s*.

Statements in form (A) are likely to appear in the proposed semantics for *P*. Is a statement in form (A) supposed to be true *simpliciter*, or is *that* statement only true or false relative to something or other, such as a further context of assessment? (Shapiro 2014: 184)

In the following passage, Shapiro says that there is a regress worry for assessment-sensitive accounts.

If the entire language is to be given an assessment-sensitive semantics, then we seem to be off on a regress. And it looks vicious, because if everything, including the semantic theory, is assessment-sensitive, then we can't ever seem to assign a truth value to anything in any context. (Shapiro 2014: 184)

It's noteworthy that he describes the problem in terms of a difficulty for how a truth value can be assigned for a sentence. But he thinks contextualist semantic theories do not face the same worries.

It seems that regresses like this only threaten *assessment-sensitive* accounts of various words and phrases. (Shapiro 2014: 185)

Since I've argued that Shapiro's indexical contextualist theory won't do a better job at explaining how truth values (or extensions more generally) can be determined, it will be important

to see why Shapiro think the regress worry applies to assessment-sensitive (and not contextualist) semantic theories. He thinks contextualism is supposed to avoid a regress because on contextualist theories, “there is no further context to be consulted”. This is in line with the discussion of Passmore where it was noted that someone can respond to a vicious regress argument by saying that one stage will be privileged (i.e. that it will not bring in the same issue that was originally to be explained. Shapiro says

Suppose, for example, that our predicate  $P$  is to be given a contextualist treatment. Then the content of a sentence in the form “ $a$  is  $P$ ” is fixed from the context of use. Since there is no further context to be consulted, there is no potential regress. (Shapiro 2014: 185)

But this is not a satisfying explanation of why a regress would be stopped. According to the explanation, no further context would need to be consulted for the determination of an expression's extension. But as was argued above, if Shapiro's relativism is true, the extensions will not come for free. All that the indexical contextualist theory will deliver is a set of bi-conditionals which are supposed to be statements of truth-conditions or satisfaction conditions (of expressions or contents relative to contexts). But none of this explains how truth values or extensions will get determined. The machinery of the indexical contextualist theory will set us up with instances of modus ponens like  $(G^*)$ . And we can grant that the premises of  $(G^*)$  are true. But on Shapiro's view the premises of  $(G^*)$  won't simply imply that  $(A^*)$  falls under the extension of the specific content *classically valid*.

### **Shapiro's explanation of why only an assessment sensitive semantics will face a regress**

There are some passages where Shapiro goes into more detail about why he thinks a regress should or shouldn't count as vicious. I'll look at some of these passages, and argue that they do not provide a reason for thinking the previously outlined regresses are any less problematic than the regress he describes for assessment sensitive accounts of “valid”.

Shapiro defines a notion of “second-order absoluteness” to characterize a sufficient condition for vicious regresses. He says the viciousness of a regress depends at least on

whether a  $(n+1)$ st-order ERP has to be adjudicated before the corresponding  $n$ th-order ERP can be. (Shapiro 2014: 191)<sup>18</sup>

And he defines second-order absoluteness as follows:

*U* is *second-order absolute*, in a given context *CA*, if the “outer” context (the one “used” in the meta-theory) does not affect the truth-value, of the ERP involving *U* and *CA*. (Shapiro 2014: 195)

Shapiro says that if the discourse is second-order absolute, there is no regress.

If a folk-relative stretch of discourse is second-order absolute, then the potential regress mentioned at the start of this chapter is nipped in the bud. [...] Things are not quite that simple, however. (Shapiro 2014: 195)<sup>19</sup>

This gives us a condition for thinking that a regress won't be generated, but it seems like the previously described regresses give us examples of cases that are not second-order absolute. On Shapiro's view, if we consider claims like “(A\*) falls under the extension of “valid” in a classical context of use”, we have to consider a further context to adjudicate its truth value. That means we cannot rule out a regress, and we have all the same reasons as before for thinking that the case is vicious.

Shapiro also brings up examples that involve a lack of second-order absoluteness.

So the statement  $\neg T(G, CA_3)$ , that a Gödel sentence for Priest's system is not true, is itself not absolute. The classicist has it as false and the dialetheist has it as true (and false). So, in a sense, *G* is not second-order absolute. (Shapiro 2014: 198)<sup>20</sup>

So he brings up cases where a regress is not ruled out. But he comments on the idea that there can be non-problematic cases of regress. His reasoning for this involves a claim about keeping track of perspectives.

none of this is conceptually problematic-assuming we manage to keep our bearings (which is sometimes not too difficult to do). There should not be any conceptual confusion so long as we keep track of the various object-level and meta-level perspectives,

<sup>18</sup>ERP is an abbreviation for “explicitly relational proposition”.

<sup>19</sup>Shapiro alludes to some complication at the end of this passage, but I'm not quite sure what he is referring to. It may be that, even if there are some cases where second-order absoluteness is present, there are nonetheless other cases where second-order absoluteness is not present.

<sup>20</sup>Shapiro also brings up other examples aside from the one referenced in this passage.

including, of course, the perspective one is occupying when proving the meta-theoretic results themselves. (Shapiro 2014: 200)

The solution described here involves the idea that we can keep track of perspectives at different levels, but this doesn't work. Suppose I reason in accordance with (A\*). Then it would seem as though I am reasoning classically. But if there is no privileged extension for a claim like "(A\*) is valid relative to classical logic", then there is no privileged answer for whether I am reasoning classically. So, knowing this, what would it mean to keep track of my own perspective? Would it mean I need to keep track of what my own perspective is relative to what my own perspective is? Hasn't it already been admitted that there is no fact of the matter about what my own perspective involves? In any case, even if a vicious regress could be avoided by keeping track of perspectives, it's not clear why the same thing couldn't be said on behalf of someone who takes an assessment sensitive relativist position on the semantics of "valid".

### 6.2.6 What about non-indexical contextualism?

It's worth saying something about why a regress wouldn't dissolve if Shapiro's view were combined with a non-indexical form of contextualism for "valid". On this view, the content of "valid" would stay the same across different contexts of use, but the expression's extension could shift (depending on the logic of the context of use). But if we assumed that the context of use was classical, would no further context need to be consulted to determine the extension of "valid"?

It might be thought that the answer is yes because that is just what a non-indexical contextualist view is supposed to be. The non-indexical contextualist viewpoint is described as one where the content of an expression remains fixed, but the extension shifts depending on the context of use. Nonetheless, there is a problem for how the mechanics of this are supposed to work if Shapiro's relativism is assumed. For suppose that we are in a classical context of use. In this case, the content of "valid" will be the unspecific content *valid*. Will (A\*) fall under the extension of *valid* in a classical context of use? The non-indexical contextualist semantics tells us that (A\*) will fall under the content of *valid* in a classical context of use iff (A\*) is classically valid. But even if we grant that (A\*) is classically valid, this will not provide for a sufficient explanation of how (A\*) falls under the content of *valid* in a classical context. We can illustrate this in terms of the

following instance of modus ponens.

(G1\*\*) (A\*) is classically valid.

(G2\*\*) If (A\*) is classically valid, then (A\*) falls under the extension of the content *valid* in a classical context

(GC\*\*) (A\*) falls under the extension of the content *valid* in a classical context

But the premises of this argument won't simply imply the conclusion on Shapiro's view. They will imply the conclusion relative to some logics (but not relative to other logics). So the issue won't be changed by appeal to non-indexical contextualist theories.<sup>21</sup>

### 6.2.7 A further issue regarding the extent of Shapiro's relativism

Shapiro sometimes describes his relativism as applying to explicitly relativized claims. So it will not only be claims of the form "P is a consequence of G" that depend on further parameters for their truth-value. Claims of the form "P is a consequence of G in logic L" can also depend on further parameters for their truth-value. He says

C) P is a consequence of G in logic L.

Here the logic at hand, L, is given explicitly. At least some attempts to evaluate a statement in form (C), are bound to involve deductive reasoning. [...] As we shall see, sometimes the logic of the meta-theory has some bearing on the truth-value of statements in form (C). Sometimes it doesn't. (Shapiro 2014: 185)

In this passage, Shapiro says that the truth-values of statements like (C) are sometimes dependent on the logic of a meta-theory. This would be a folk relativism not just for what is valid, but also for what is valid in a logic.<sup>22</sup> To support the claim that statements in form (C) are

<sup>21</sup>Even if the premises of (G\*) imply their conclusion relative to certain logics, that requires a further parameter beyond what is given by the indexical contextualist theory.

<sup>22</sup>This seems to be in tension with other characterizations that Shapiro gives of his view. For example, in the following passage he seems to endorse the view that in mathematics, there aren't facts about what simply follows from a premise, but that there are facts about follows from a premise relative to a logic.

in the slogan of folk-relativism, there is no such thing as "simply being valid." Rather there is validity-in-classical-theories, validity-in-intuitionistic-theories, etc. (Shapiro 2014: 115)

This passage implies a kind of asymmetry between (on the one hand) facts about what follows from what and (on the other hand) facts about what follows from what according to a given logic. But if Shapiro is right about claims in form (C), then it is not only the case that there no such thing as "simply being valid". It's also the case

sometimes dependent on the logic of a meta-theory for their truth value, Shapiro presents multiple examples where a difference in meta-theory can result in a difference in evaluation for a claim about what is valid in a given logic.<sup>23</sup> For example, he says

Suppose  $A$  and  $B$  are distinct sentence letters, and consider the following statement  $U$ :

Let  $\Gamma$  be a set whose only member is  $A$  if the Goldbach conjecture is true, and whose only member is  $B$  otherwise. Then the argument  $\langle \Gamma, (A \vee B) \rangle$  is valid.

Let  $CA$  be intuitionistic sentential logic. Then  $T(U, CA)$  is the statement that  $\Gamma$  entails  $(A \vee B)$  in intuitionistic sentential logic. Now let  $CA_1$  be a standard, classical meta-theory, say classical set theory, and let  $CA_2$  be an intuitionistic meta-theory.

Reason in  $CA_1$  (our classical meta-theory) as follows. If the Goldbach conjecture is true, then  $\Gamma$  is  $\{A\}$  and so, of course,  $\Gamma \vdash (A \vee B)$  in intuitionistic, sentential logic. It is just an instance of  $\vee$ -introduction. If, on the other hand, the Goldbach conjecture is false, then  $\Gamma$  is  $\{B\}$ , in which case  $\Gamma \vdash (A \vee B)$  in intuitionistic, sentential logic. So, either way,  $\Gamma \vdash (A \vee B)$  in intuitionistic sentential logic. QED. Of course, we have used excluded middle (or, rather, bivalence), in the meta-theory  $CA_1$ . We presuppose there that either the Goldbach conjecture is true or it is false. But that move is kosher since the meta-theory  $CA_1$  is classical. We have not invoked excluded middle within the object-level logic in question, intuitionistic sentential logic. The only object level inference invoked is  $\vee$ -introduction. So we have that

$$T(T(U, CA), CA_1).$$

However, the intuitionist will not agree that either  $A \in \Gamma$  or  $B \in \Gamma$ , for that amounts to an instance of excluded middle on the Goldbach conjecture. So the intuitionist will not agree that the set  $\Gamma$  implies the disjunction  $(A \vee B)$ . So we will not have

$$T(T(U, CA), CA_2) \quad (\text{Shapiro 2014: 196-197})$$

If this is right, then the truth value of a claim about what is valid in intuitionistic sentential

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that *at least sometimes*, there's no such thing as simply being valid in intuitionistic logic. That means that *at least sometimes* there is no such asymmetry between facts about what follows and facts about what follows according to a given logic (because in both cases we cannot say that there simply are facts of that kind). I'm not sure what the most charitable way of understanding Shapiro's view is, but it may be better to characterize Shapiro's view by saying not only that there are no facts about what simply follows in mathematics, but also *at least sometimes* there are no facts about what simply follows in mathematics even according to a particular logic. It's worth asking whether it is merely sometimes or always that there are no facts about what simply follows in mathematics according to a given logic  $L$ . Shapiro says that it is only sometimes that claims in form (C) will depend on a meta-theory for their truth value. But the point about arbitrariness is supposed to show that there is a serious difficulty in the idea that we can sequester relativism to an initial stage (e.g. validity full stop). Shapiro gives examples where claims in form (C) are supposed to depend on a meta-theory for their truth value. In the one discussed in the main text below, it is a claim of the form "P is a consequence of G in intuitionistic logic. But presumably the same thing would have to hold for every logic if Shapiro's relativism is true. No matter what logic  $L$  we consider, if Shapiro's relativism is true, then there can't be any such thing as what simply follows from the axioms of  $L$ . And as mentioned in the previous discussion regarding arbitrariness, invoking a notion of the rules of  $L$  will not change the point.

<sup>23</sup>It's also worth mentioning that there are formal results about what can be proved from a set of axioms relative to different meta-theories. With a classical meta-theory, completeness results can be proved for the axioms of quantified intuitionistic logic with respect to Kripke models. But the same results cannot be achieved with an intuitionistic meta-theory. For more on this, see McCarty's "Undecidability and Intuitionistic Incompleteness". (1996)

logic will depend on the logic of the context of use.<sup>24</sup> But this only contributes to the previous regress worries. Explicitly relativized predicates like “intuitionistically-valid” will occur in the semantic clauses for contextualist theories. In other words, it only reinforces the idea that there is a worry for how an expression like “valid” can get an extension in a context of use on an indexical contextualist theory of “valid”. On an indexical contextualist theory of “valid”, an argument  $\sigma$  is supposed to fall under the extension of “valid” in a context of use  $c$  iff it falls under the extension of the content of “valid” in  $c$ . If the logic of the context  $c$  is intuitionistic, then the content of “valid” in  $c$  will be something like *valid-in-intuitionistic-logic*. When will something fall under the extension of such a content? An argument  $\sigma$  will fall under the extension of the content *intuitionistically valid* iff  $\sigma$  is intuitionistically valid.

But that means that the content of the expression “valid” isn't sufficient for an explanation of how an extension gets fixed. For suppose the argument  $\sigma$  is the argument mentioned in the previous passage from  $\Gamma$  to  $A \vee B$ . Given the mechanics of the indexical contextualist theory,  $\sigma$  is supposed to fall under the extension of the content of “valid” in an intuitionistic context of use iff  $\sigma$  is intuitionistically valid. But on Shapiro's view, whether  $\sigma$  is intuitionistically valid isn't something that can be determined absent further parameters. This means that no extension has been determined even after all the mechanics from the indexical contextualist theory have been introduced; something further will need to be consulted even after we have a specific content like *valid-in-intuitionistic-sentential-logic*. In other words, if Shapiro's relativism is right and there are context dependent truth-values for claims about what is valid in intuitionistic sentential logic, then the specific contents provided by the indexical contextualist theory will be insufficient to play the role they are supposed to play in determining the extension of expressions like “valid”. The specific content *valid-in-intuitionistic-logic* isn't going to determine an answer for whether  $\sigma$  falls under its extension because given the folk relativism about intuitionistic validity,  $\sigma$  is neither simply intuitionistically valid nor simply not intuitionistically valid.

Appealing to further contexts won't change the matter. If we appeal to a further context,

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<sup>24</sup>It's worth noting that this example doesn't actually support the view that the logic of the meta-theory can bear on the truth value of claims in the form “P is a consequence of G in logic L”. The example only shows that an agent's evaluation of these claims can depend on a logic that is operative in their reasoning. It is plausible to think that individuals employing different meta-theories will come to different conclusions about what is valid in sentential IL. But this doesn't imply that one of the meta-theories isn't privileged; nor does it imply that the meta-theory “has some bearing on the truth-value of statements in form (C)”.

this just means that we will have to consider a further question of whether  $\Gamma$  entails  $(A \vee B)$  in intuitionistic sentential logic *according to some further logic L*. L could be anything you like. It may or may not be intuitionistic. But whatever L happens to be, we can think of it as some combination of axioms plus at least one rule of inference. So if we are asking whether  $\Gamma$  entails  $(A \vee B)$  in intuitionistic sentential logic *according to L*, then we have to look at the axioms and inference rules of L to see what L says. But the problem here is the same as the one that was mentioned in the section on arbitrariness (6.2.1). If (K1) by itself doesn't imply (KC), then it would be arbitrary to say that something follows from the axioms of L (because the axioms of L, being universal claims, aren't significantly different from the claim (K1)). But if the axioms of L don't simply imply anything, appealing to them alone will not leave us in a position that is better than the one we were originally in when we were only considering whether  $\Gamma$  entails  $(A \vee B)$  in intuitionistic sentential logic. The only place left for us to look in L is at the inference rules. But as I argued in the section on arbitrariness, appealing to rules is of no help. This can be seen most clearly when the inference rules are characterized as universal claims. If (K1) by itself doesn't simply imply anything then the rules (characterized as universal claims) won't simply imply anything either. And as per the points from the previous chapter alternative characterizations of a rule will not change the matter either.

What this means is that appealing to L will not leave us in a position that is any different from the one we originally started out in (when we were simply considering whether  $\Gamma$  entails  $(A \vee B)$  in intuitionistic sentential logic). So if we needed to appeal to a further logic at this initial stage, then we will have exactly the same need to appeal to a further logic when we are at the stage where we have also brought in the logic L. So we would need to bring in a further logic L', and so on. Clearly there is a regress here, and it is vicious for the same reasons that were discussed in section 6.2.4.

### 6.3 Shapiro's Logical pluralism and underdetermination

In the following, I'll raise a separate difficulty for the idea that there are only contextually relativized facts about logic, i.e., facts of the form *A implies B relative to context C*. Shapiro explains these

facts in terms of the idea that contexts can have underlying logics. In particular, on his view, a context is supposed to have an underlying logic as long as it “involves something resembling deductive reasoning”.

Every coherent perspective—every language, every form of life, every context—has an underlying logic, assuming only that it involves something resembling deductive reasoning. (Shapiro 2014: 5)

But this is an assumption that can be challenged. Moreover, if it turns out that deductive reasoning is insufficient to determine a logic for a context, then we cannot appeal to deductive reasoning to explain how there can be contextually relativized logical facts of the form that Shapiro posits in his theory.

This is the same as the point that was discussed in section 5.3. The issue is developed in terms of the kinds of points in Kripke's Wittgenstein where an underdetermination worry is raised for dispositional analyses of rule-following. The idea here is that deductive reasoning would always underdetermine a logic for a context of use.

As noted above, on Shapiro's view, a context will have an underlying logic when it “involves something resembling deductive reasoning.”? The problem is that while a context may be associated with the reasoning of an individual or a group, it's not clear how reasoning could determine a logic because reasoning will always be finite.

Suppose we restrict our attention to the case of an individual's reasoning.<sup>25</sup> Why should the finitude of their reasoning cause any problem for the idea that it constitutes a logic for a context of use? The details of this should depend on what aspects of reasoning a theorist is citing as the basis for a logic of a context in the first place. One thing that a theorist might point to is the behavioral manifestation of an agent's reasoning. But this will only give us information about the initial and resulting stages of an agent's reasoning, i.e. information to the effect that they started out with some set of assumptions  $\Gamma$  and reasoned to a further claim  $\phi$ .<sup>26</sup> Since an agent will engage

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<sup>25</sup>I don't think the ultimate point will be changed if we think about the reasoning of a group as opposed to an individual.

<sup>26</sup>I'm going to abstract away from some important details regarding reasoning and inference in the following discussion. For one thing, reasoning involves a lot more than inference. For another thing, inferences can be based on acts of supposition, e.g., where one infers something on the basis of reasoning from a reductio. For that reason it's not clear that inference should be identified with transitions from a set of propositions to a proposition; see Dogramaci (2013). That being said, I'll only focus on instances of reasoning where an agent moves from a set of assumptions to

in only finitely many such inferences, this would only give us the information that they transitioned in accordance with finitely many ordered pairs of the form  $\langle \Gamma, \phi \rangle$ . And since a logic needs to cover infinitely many instances of its valid forms, the inferential behavior of an agent will not fix any particular logic.

This can be elucidated through an example with two logics  $L$  and  $L^*$ . Consider a number  $n$  that is so large that whenever a formula has  $n$  connectives, we could not even understand it for reasons of computational limitation. It might be that whenever a formula having  $n$  negations is a theorem of  $L$ , the negation of that formula is a theorem of  $L^*$ . Or, alternatively, if  $L$  has a rule like conjunction elimination, it might be that  $L^*$  has a rule just like conjunction elimination except that whenever a conjunction has  $n$  sub-formulas, you cannot infer either conjunct.<sup>27</sup> Since  $L$  and  $L^*$  will differ only in places that are beyond the limits of our computational abilities, an agent will only engage in inferences where  $L$  and  $L^*$  agree. For that reason, an agent's inferential behavior will under-determine whether either of these logics is the underlying logic of the context they are currently in.

None of this is to say that the behavioral manifestation of an inference is the only thing that a theorist should be allowed to point to as a feature that is responsible for determining the logic of a context. For example, many theorists would contend that an agent's inferential behavior is explained by the fact that they are following inferential rules. If an agent's inferential behavior is explained by the presence of rules that somehow underlie their behavior, then the previous worry can be avoided. For example, two instances of reasoning may outwardly appear the same, but be distinguished on the grounds that each inference results from a different rule being applied. If I infer  $\phi$  from  $\phi \ \& \ \psi$  (where  $\phi$  and  $\psi$  are atomic), and we only look at the initial and resulting stages of my reasoning, then nothing tells us whether I am applying  $\&$ -Elimination as opposed to some gerrymandered  $\&$ -Elimination\* (that is only like  $\&$ -Elimination at levels below our computational limitations). But why couldn't it be the case that my inference is the result of  $\&$ -Elimination being applied (and not  $\&$ -Elimination\*)? The fact that someone makes only finitely many inferences is neither here nor there. So perhaps a theorist can say that the logic of a context of use is determined

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a further claim without this happening via any acts of supposition (for the sake of simplicity in discussion).

<sup>27</sup>Some details may have to be altered to get the case exactly right, but the point is to have two systems that are identical up some point which is beyond our computational limitations.

by the inferential rules (which underlie an agent's inferential behavior).

I think this answer is only superficially helpful. Whether the answer is ultimately satisfying should depend on how rule following is understood, and theorists are divided over how they answer various questions regarding the nature of rule following. I'll discuss non-representational theories of rule following where rule following is understood in terms of dispositions, and I'll reserve discussion of representational theories of rule following to a footnote.<sup>28</sup>

Let's suppose we are looking at a view where the inferential rules that an agent follows are understood in terms of some set of inferential dispositions that the agent has. Then, according to the response being considered here, an agent's inferential dispositions will be the key factor for what determines the logic of a context of use. In this case, even though the behavioral manifestation of an agent's inferences can determine only a finite set of ordered pairs, that won't obviously be a problem since what is relevant, on this view, is the dispositions (which underlies the behavioral manifestation).

The problem with this response is that an agent's dispositions are finite as well. It's not just the case that an agent doesn't draw inferences in accordance with the infinitely many valid arguments in a given logic L. It's also the case that there will be valid arguments in L that an agent is not even disposed to infer in accordance with. For example, if we look at an argument with a billion premises, an agent just won't be disposed to infer in accordance with it because they won't be disposed to consider all the premises in the first place (they will die before they finish thinking about all of them). In other words, there will be computational limitations concerning the length of formulas and number of premises that can be evaluated. Given these limitations, for any logic L that was hypothesized as the logic that accords with an agent's inferential dispositions,

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<sup>28</sup>Suppose we understand inferential rules as things that are represented in the mind of an agent. There will always be a question about what determines whether the agent is representing the standard rule as opposed to some quus-like version of it. And even if there is a determinate fact about what rule the agent is representing, there will be a repeat of the arbitrariness worry from the previous section. For example, suppose a universal instantiation rule is represented in an agent's mind as something like "all universal claims imply each of their instances" (or perhaps as some corresponding sentence in a language of thought). If it were, there still wouldn't be anything that it simply implied on Shapiro's view (and it's not clear why alternative characterizations of a rule would change the matter). It also may be the case that the kind of argument that Kripke runs against Quine may be successfully applicable to a view like this. If an agent wasn't already able to infer (E2) from (E1), why would the assumption "all universal claims imply each of their instances" help? If they couldn't infer anything from the first universal claim, (E1), then why would they be able to infer anything from this more complicated universal claim? For valuable discussion on the relationship between the quus issue and Kripke's argument against Quine's empiricism about logic, see Padro (2015: 77-78).

we can always imagine another logic  $L^*$  that differs from  $L$  only in places where the agent has no inferential dispositions.

There are responses to this style of criticism against dispositional theories of rule following in Shogenji and Warren, but I won't comment on objections here because I have considered their responses (in sections 5.3.1 and 5.3.2 respectively). Instead, I will consider another way of articulating a non-representational view of rule following and consider whether it would change the main issue.

As noted in the previous chapter (in the last subsection of 5.2.1), Devitt (2006) has argued for a conception of rule following where a rule may govern an agent's behavior by being "embodied", but not necessarily represented, in the mind. Part of his explanation of this idea is in terms of an analogy to the distinction between software rules and rules that are merely built into a computer's hardware. It may be that this view differs from the theories of Shogenji and Warren because it is possible to construe Devitt's view as one where the rules explain an agent's dispositions, rather than the other way around. Devitt (2006) develops his idea with regard to linguistic rules, but he has also taken up this idea in unpublished work on inferential rules.

Devitt also draws an analogy with the idea that there are different ways to design a calculator that will give the right outputs for addition. Certainly two calculating machines could be designed differently but nonetheless have the same inputs and outputs. For example, one calculator might operate in a standard way so that it produces the right results for adding. But another calculator could operate in exactly the same way, but then add one and subtract one. In both cases, even though the calculators would have the same inputs and outputs, they would have a different internal structure. This is a nice example because it provides a picture of how machines that are disposed to give the same results can be seen as operating on the basis of different rules.

While this does provide a way of making an important distinction, it doesn't make the kind of distinction that would be needed in order to resolve the underdetermination issue for Shapiro. On Shapiro's view, truth values for validity ascriptions need to be determined by the logic of the context of utterance. But if we want the right truth values, we need the context to distinguish between the possibility of our following  $\&$ -elimination as opposed  $\&$ -elimination\*. If the context

includes rules in the style of Devitt, then we can distinguish between the rules  $m+n$  and  $(m+n)+1-1$ . But in the case of  $m+n$  and  $(m+n)+1-1$ , there aren't any extremely large inputs which will result in a divergent output. So what's needed is a distinction between a calculator operating with a "+" rule and not a "+\*" rule where a "+\*" rule is something like the quuaddition rule in Kripke's Wittgenstein.<sup>29</sup> In any case, Devitt develops his non-representational view of rule following for reasons independent of any aim to respond to the problems raised in Kripke's Wittgenstein. The point here is only that this analysis of rule following would still underdetermine a logic for a context of use.<sup>30</sup>

## Concluding Segment

I've argued that Shapiro's version of logical pluralism also results in a vicious regress. Many of the points from the previous two chapters carried over to the case of Shapiro's view. As in the case of Beall and Restall (understood from the perspective of Caret), I argued that Shapiro's view cannot avoid a vicious regress merely by introducing a semantic theory for "logically valid". I argued that this holds for the indexical contextualist theory of "logically valid" that Shapiro develops, and I also argued that the matter wouldn't be significantly altered with a non-indexical contextualist theory as well. As in the case of Field, I argued that Shapiro's view faces an underdetermination worry. For Shapiro, the truth value of a validity attribution is something that is determined relative to a context of utterance. A context of utterance is supposed to be able to play this truth value

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<sup>29</sup>It would need to be slightly different from the quus function because as Kripke defines the quus function, the outputs for plus and quus diverge for relatively small inputs (in particular when at least one is 57 or greater). But you can imagine a quus-like rule that diverges from plus only when the inputs are much larger.

<sup>30</sup>There's an issue about inferentialist theories of meaning that is related to the points being made here. If my view is right (that deductive reasoning cannot be a basis for the logic of a context), then does this rule out the possibility of an inferentialist theory of meaning for logical connectives? Someone might say that it does. If deductive reasoning cannot be the grounds for the logic of a context, then how could deductive reasoning ground a meaning for "and"? I would distinguish between a merely semantic version of inferentialism about the connectives and a metaphysical version of inferentialism about the connectives. In a merely semantic version of inferentialism about the connectives, inferential practice would determine the meaning of expressions like "and", but the facts about what follows from a conjunction would not derive from facts about the meaning of "and". In a metaphysical version of inferentialism, inferential practice would determine the meaning of expressions like "and", but also, the facts about what follows from a conjunction would be based in facts about the meaning of "and" (and perhaps "valid"). What I have said in the text would rule out a metaphysical version of inferentialism where the facts about what follows from what are based in facts about the meaning of the connectives and "valid". This is because inferential practice (given its finitude) could not determine a meaning precise enough to distinguish between a logic  $L$  and a gerrymandered counterpart  $L^*$ . But what I have said in the text would not rule out a merely semantic version of inferentialism about the connectives. That being said, on a merely semantic version of inferentialism, the meaning of "and" would not be precise enough to distinguish between the classical conjunction and a non-standard conjunction in a logic like  $L^*$ .

determining role because a context is supposed to determine a logic. Shapiro says that a context will be associated with a logic as long as the context resembles something concerning deductive reasoning, but I argued that this is not an assumption that can be taken for granted. I noted that the details of this point should depend on which features of deductive reasoning are supposed to play this logic determining role, and I looked at various views that a theorist might have about this. I argued that in each case, the finitude of an agent's reasoning casts doubt on the idea that reasoning can determine a logic for a context of use.

# Conclusion

So where do things stand? I've argued that certain versions of logical pluralism will result in a vicious infinite regress. I haven't argued that all versions of logical pluralism will face this difficulty. I directed my arguments towards a few specific versions of logical pluralism, and the arguments always centered on principles like modus ponens, universal instantiation, and disjunctive syllogism. It may be the case that the vicious regress considerations only apply to views that take a pluralist stance on specific logical principles. But setting aside points about scope, I hope my arguments have shown that logical monism can be seen as desirable on the grounds that it can avoid this type of worry.

I've tried to situate these main points in the context of careful discussion about the nature of vicious regress arguments. The main purpose for this was to have clarity about the presuppositions that are built into a vicious regress argument. This is important for someone making or critiquing this style of argument. Getting clear on the presuppositions of a vicious regress argument makes it easier to see where an assumption may need to be defended or where a point might be criticized.

Much of the discussion regarding the nature of vicious regress arguments took place in the first three chapters. While much of the discussion in these chapters was historical, the chapters were intended to provide a baseline for thinking about the structure of vicious regress arguments in later chapters. Some of the main points in the first few chapters were as follows. A vicious regress isn't necessarily to be understood in terms of epistemic justification. This was highlighted in the discussion of Carroll. Also, when a vicious regress argument is directed towards a form of relativism about logic, there is a question about how the version of relativism is formulated. For any version

of relativism about logical validity, logical validity will be somehow relativized to a parameter. So there will always be a question about how to understand the parameters that are posited in the relativist theory. This was noted in the discussion of Quine's criticism of Carnap. Carnap's view can be characterized as a form of relativism where logical validity is understood as somehow relative to linguistic conventions. So there is a question about how to understand the operative notion of linguistic convention, and it was noted that there is a more than one way to spell out the details. A main reason why this mattered was because there was an issue about whether a vicious regress could be avoided when linguistic conventions were understood in a certain way.

In the last three chapters, I developed some vicious regress arguments for different versions of logical pluralism. The material from prior chapters informed the discussion that surrounded these arguments. In chapter 4, I highlighted a few different assumptions that were part of the vicious regress argument I developed for Beall and Restall's version of logical pluralism. I looked at objections that challenged these assumptions and I provided a response in each case. In particular, I argued that there is in fact a regress, that the regress is in fact vicious, and that an analogous regress doesn't hold for logical monism. I noted that each one of these assumptions is critical for the main argument about how vicious regress points provide a consideration in favor of logical monism. The discussion of Beall and Restall was also an example of how a vicious regress argument cannot necessarily be dismissed on the basis of considerations about epistemic justification.

Some of the points in the chapter on Quine and Carnap were also shown to be relevant to the discussion of Field and Shapiro. On both Field and Shapiro's view, logical validity is relativized to a certain kind of parameter. So in the exegesis of both their views, there is a question about how to understand the relevant parameters. When I argued that their views face a vicious regress, I considered objections that were premised on the idea that a regress would only result when the parameters were understood as truth-evaluable entities. I responded by arguing that a vicious regress will result even when the relevant parameters are understood in non-propositional terms.

I also made some other points along the way. I argued that a logical pluralist cannot avoid a vicious regress by framing their view in terms of a semantic theory. I looked at objections of this form in the discussion of Beall and Restall's view (*vis-à-vis* Caret) and Shapiro's view. In

both cases, I argued that the introduction of a semantic theory will not prevent a vicious regress. I argued this by showing how a vicious regress can be developed directly in terms of notions that are part of a semantic theory (e.g. in terms of the notion of a context relative extension). I also made a point about underdetermination in relation to Field's version of logical pluralism.<sup>31</sup> The point was based on two details that were unique to the style of relativism from Field. The first assumption was that certain validity attributions will have truth values only relative to logical policies. The second was an assumption that policies are to be understood in terms of facts about an agent's dispositions. The idea behind the argument was that, given these two assumptions, logical policies would only be able to play the requisite truth value determining role if facts about an agent's dispositions can determine facts about what rules the agent follows. I looked at two different dispositional theories of rule following (from Warren and Shogenji) that aim to account for this, and I argued that neither can successfully avoid the puzzle.

While my main goal has been to argue that vicious regress points provide a consideration in favor of logical monism, I also hope that there are certain things that I've done that are independent of whether or not the reader agrees with the main points. I hope I have provided the reader with an opportunity to think carefully about the monism-pluralism issue in the philosophy of logic. I also hope the material encourages thought about how the details of a logical pluralist thesis would need to be spelled out and how a logical pluralist framework would be able to avoid vicious regress objections. Perhaps most importantly, I hope the material motivates the reader to think about the nature of logic and the roles that logic plays more generally in philosophy and practical life.

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<sup>31</sup>A similar point was also made in relation to Shapiro's version of logical pluralism, but in Shapiro's case, the argument concerned details of his semantic theory.

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\*The English translation was originally published in 1937 by Kegan Paul, Trench, Trubner & Co Ltd. References to *Logical Syntax of Language* in this dissertation are from the English translation reprinted by Routledge in 2001.

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