# REVISING LOGIC: WHERE THE EMPIRICAL MEETS THE A PRIORI

by

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Abstract

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In this work, I defend an empiricist conception of logic. Hartry Field has recently argued that logic must be a priori because we could not gather evidence against certain basic logical rules. Though I agree that logic is probably a priori in Field's sense, I distinguish strongly empirical reasons of the sort that Field considers from modestly empirical reasons. These latter kinds of reasons are those we make when our concern is to revise conservatively over the evidence. To demonstrate my claim, I appeal to Hilary Putnam's famous suggestion that we revise classical logic in favor of quantum logic to help make sense of certain quantum mechanical anomalies. I show that when the evidence doesn't clearly determine how we should revise either way, as in Putnam's example, we are entitled to revise our logic by appeal to pragmatic rules. Since in Putnam's example, the choice is between revising logic and adopting a particular physical interpretation of quantum mechanics, I argue that revising logic is just as empirical as is the more physical option: both seek to explain and make sense of the evidence.

Next, I must show that pragmatic rules are not, themselves, unrevisable for empirical reasons. To do this, I turn to a recent argument by Jerry Katz that states that pragmatic rules could not be revisable without being self-defeating. I argue that Katz's claim is true assuming that justification is factual, but that this is an assumption that empiricists have independent reasons to reject. I defend a non-factualist conception of justification, and show how non-factualism offers a way out of Katz's problem for the radical empiricist.

Finally, I show how my version of empiricism can go some way toward solving the long-standing problem of justifying deduction. I argue that since I have shown that logic is revisable for at least modestly empirical reasons, logic is always at risk of being revised in favor of different rules that better fit the evidence. I conclude that empiricism can better account for the justification of logic than can the more rationalist account of justification put forth by Paul Boghossian.

## Preface

The goal of this dissertation is to examine the justificational status of logic. In particular, I will defend the idea that logic is empirical, all the while acknowledging the correctness of recent arguments by Hartry Field to the effect that logic must be a priori. To do this, I will try to make plausible a notion of empiricism that is compatible with logic's being a priori in Field's sense. It may seem strange to argue that logic is both a priori and empirical. I will discuss this overall strategy in the conclusion when I consider, albeit briefly, the historical debate leading up to my position. For now, suffice it to say that I am not the first to try to argue for a thesis along these lines.[1]

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[1] See Bostock [1990] and Mittelstaedt [1986]. Bostock's point with respect to why logic should be seen as empirical is very similar to mine. He argues that we can always ask of any of our concepts (be they logical or not) whether they are the best one's to use given our information about the world (to use his term, we can always inquire as to their "fit" with the world). Nevertheless, according to Bostock, if we choose to retain the classical conception of truth, then from that conception, we cannot but see classical logic as correct. What makes logic a priori, according to Bostock, then, is very different than what makes logic a priori according to Field. For Bostock the source of logic's a priority would lie in its analytical connection with the concept of truth, whereas for Field, logic's a priority is guaranteed by more epistemic considerations. Mittelstaedt's claim is more particular, based on specific considerations about quantum logic, and his position does not bear much, if any, resemblance to the view being advocated here. According to him, quantum logic is empirical in the sense that we can read the logic directly from physical reality (just as the original Berkhoff and von Neumann paper claimed, by investigating the alegebraic structures of Hilbert spaces). Nevertheless, Mittelstaedt argues that the pragmatic preconditions of a scientific language in general are presupposed by quantum mechanics, and cannot be justified by it. By pragmatic preconditions, Mittelstaedt means the very general framework in which propositions can be thought of as either true or false, proven or unproven. Mittelstaedt argues that these preconditions, in

I will begin by clarifying what it means for a warrant to be a priori. Most philosophers do, I think, believe that a priori warrants for belief should not be revisable for empirical reasons. Nevertheless, a growing number of philosophers have recently attempted to undermine this notion, arguing that a priori warrants can be revised for empirical reasons. In Chapter 1, I will defend what I take to be the majority notion: a priori warrants should not be revisable for empirical reasons. I will argue that those who claim that a priori warrants should be revisable for empirical reasons are defending a notion which, at worst, is incoherent, and at best, is philosophically uninteresting. I will also say a little about what I will mean by 'logic', though my comments in this regard will be brief and purely introductory.

Whatever else a priority might be, it is first and foremost supposed to warrant belief and so to be a form of justification. In order to make good on my goal of showing how logic might still be empirical in spite of Field's arguments, I will need to be very clear about the metaphysics of justification. In particular, one supposed difficulty with the sort of empiricism that I will defend about our basic belief forming methods can be alleviated by being clearer about our metaphysical commitments regarding justification. In Chapter 2, therefore, I will briefly examine some of the more popular positions with respect to the metaphysics of justification. In addition to being relevant when it finally comes time to defend the possibility of revising our most basic methods for empirical reasons, the metaphysics of justification will be important in evaluating the prospects for justifying logic, in the final chapter.

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making ontological assumptions, may still have something empirical in them, thus tempering their pure a priority.

Having laid the groundwork for my discussion in the first two chapters, in Chapter 3, I will address Field's arguments. His view is that logical rules and principles are empirically indefeasible, and so must be a priori, given the understanding of a priority developed and argued for in the first chapter. I will argue that though Field's argument is quite correct for one understanding of 'empirically indefeasible,' there is another slightly more modest way to understand this phrase which does allow logical rules and principles to be overturned for empirical reasons. Logical rules and principles, to use the terminology I will introduce in the third chapter, may not be strongly empirically defeasible, but they are modestly empirically defeasible.

My more modest conception of empiricism will depend on giving proper credit to the role of pragmatic criteria in belief formation and revision. In particular, I will argue that beliefs are still empirical (and so not a priori), if only modestly so, they are revisable for purely pragmatic reasons. But this move only makes sense for the empiricist if pragmatic criteria are themselves revisable for empirical reasons (if only modestly empirical reasons). After all, my claim is that deductive logic, contrary to Field's argument, is still empirical in some sense of the term (a sense to be made clear in what follows), but this would hardly be worth the trouble if pragmatic reasons themselves turned out to be a priori and not empirical in this other sense. I will take it as a datum that empiricists about logic are committed to the thesis that nothing is unrevisable, not even logic or our most basic belief assessing rules. Given this, no defense of empiricism could rest with showing that logic is empirical in a way which just relies on some other method's a priority.

Thus, properly defending the sense in which logical rules and principles might still be thought to be empirical will necessitate a discussion of the revisability of pragmatic principles. In Chapter 4, I will argue that pragmatic principles, like logical principles, are revisable for pragmatic reasons. To show this, I will argue that no one has presented good reasons for thinking that it's not possible to revise pragmatic principles for pragmatic reasons. More specifically, Jerry Katz has not done so with his argument that our basic pragmatic rules must themselves be unrevisable. But neither can we find problems for pragmatic reasons to revise pragmatic principles from the equivalent of Field's complaints regarding the possibility of revising logic for empirical reasons.

In Chapter 5, I will explore the consequences of my modest empiricist position for the classic difficulty of justifying logic. I will argue that the empiricist can give some account of the justification of logic. At the very least, I will claim she can do better than can other theories. In particular, I will examine a recent argument by Paul Boghossian that only his theory can show how we might justify logic. I will argue both against his criticisms of the view that I will present, and that his own solution to the problem does not do as well as the empiricist's solution.

Other than giving an overview of this work, there is one point I want to make in this introduction: my arguments in what follows are all defensive. What I mean by this is that my intention is to defend empiricism about logic from Field's, Katz's and Boghossian's arguments. This is important because the bar for assertion is, I think, much lower when one is merely defending a view's coherence: I need only show that the empiricist can defend her view against the arguments that have been arrayed against it. Consequently sometimes my conclusions in the chapters to follow are more negative.

For instance, in Chapter 4, I conclude that we have seen no good reasons for thinking pragmatic reasons for revision are impossible. But a more ambitious conclusion would be to show that pragmatic reasons for revision are genuinely possible. This is something I do attempt to suggest, but really, I'm content to rest with the less ambitious claim.

My argument for empiricism about logic will have succeeded, I feel, if I can show that there is a coherent and plausible sense of 'empirical' on which logic is revisable for empirical reasons. Mostly, this will consist in my showing that none of the perceived threats to my notion are really valid concerns. Nevertheless, I do think that there are a lot of good reasons to believe in the view that I explain and examine in what follows, and I will, as far as I am able, try to make this position independently plausible.

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Revising Logic: Where the A Priori Meets the Empirical

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## Chapter 1: 'A Priority' and 'Logic'

The issue I am concerned to explore is whether logic is a priori. I am particularly interested in one argument, most recently articulated by Field, which claims to show that logic must be a priori; specifically, that it is somehow incoherent for logic to be anything else. My intent, mainly, is to show that though Field is right, logic is still empirical. Unfortunately, just being clear about my focus question, 'Is logic a priori?' is no easy matter, so before I can do any of this, I have to do some stage setting. In this chapter I will make clear the sense of 'a priori', that I intend to discuss. This will take a bit of argument, because there seem to be divergent opinions regarding what, exactly, it means for a warrant to be a priori. What I will do in the first section is make clear which version of a priority I will be concerned with, and show why it is the only version worth either defending or arguing against. In the second section, I will say a little bit about what I will mean by logic. My comments about logic will be much briefer than those about a priority, and here I will leave things much more unsettled. My intent in this first chapter with respect to what we should mean by 'logic' is simply to clarify the sort of issues that will be important in later chapters.

## I) A Priority

As many have noted, there are two different ways that a warrant might be thought to be a priori.[1] Most minimally, we might say, as Field does, that if our warrant to use the rules of some logic is a priori, then, "...it is reasonable to infer according to the rules of that logic without any empirical evidence for the legitimacy of those rules."(Field [1998] p. 1) Let us call this conception (W) of the a priori: it says that a warrant is a priori when we are entitled to the warrant when there is no empirical evidence to support it. The second, stronger conception, which I will call conception (S), says that the warrant in question must also be empirically indefeasible: it says that a warrant is a priori when we are entitled to the warrant in the absence of empirical evidence, and when the warrant is not revisable in light of empirical experience.

The only conception of a priority that I think worth arguing against is (S): that is, I will argue against the view that logic is empirically indefeasible. Field also thinks that the only interesting conception of the a priori is the stronger (S). And there are certainly other philosophers who agree that (S) is to be preferred.[2] However, it cannot be denied that a lot of recent advocates of the a priority of logic think they're defending (W). That is, they take themselves to be arguing for the a priority of logic in a sense that allows a priori warrants to be empirically defeasible. Against these philosophers, and there are

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[1] See for instance Kitcher [2000], Casullo [1988], and Field [1998, 1996 and 2000]

[2] For instance, Field [1996, 1998, 2000], Katz [1998]. This seems to be the conception which most foes of the a priori prefer, e.g, Quine [1951, 1970, ch. 6], Kitcher [1984, 2000]

many[3], there is an immediate appearance of argumentative impropriety on my part. In general, those who wish to advocate on behalf of some idea are the ones who get to choose which version of the idea they wish to defend. If someone attacks a conception that the defenders of the conception don't agree to, this is rightly repudiated as an attack on a straw man. So why, in the case of the a priori, should things be any different? In this section, I will try to defuse the appearance of impropriety by arguing that there are serious questions about the weaker conception of the a priori.

Advocates of conception (W) focus on whether our non-experiential reasons are sufficient, in some cases, to warrant beliefs. So, for instance, Tyler Burge says, "A priority concerns the nature of the rational support for an attitude, not the nature of its vulnerability to criticism."(Burge [1998] p. 3) According to rationalists like Burge, the rational support we have for some warrant is based on conceptual connections. But there are alternative, reliabilist ways of conceiving of weak a priority as well. These emphasize not conceptual relations but reliable belief producing processes. For instance, Alvin Goldman [1999] and Georges Rey [1998] both suggest that some internal cognitive mechanisms could embody mathematical and logical processes. If these processes were reliable producers of true beliefs, then they could be seen as a priori warranters, to borrow Goldman's phrase. I'll have more to say on the difference between what I will call conceptualist and reliabilist a priority in chapter 2. For now, I just want to point out that reliabilists, too, focus on the positive support for a belief, e.g., the fact that some beliefs are produced by non-experiential cognitive processes which are reliable producers

[3] See Bonjour [1998], Burge [1993, 1998], Rey [1998], Goldman [1999], and Casullo [1988] who are all clear that (W) is the notion whose coherence and/or existence they wish to defend.

of true beliefs, and claim that defeating empirical evidence doesn't effect that reliabilist justification. For instance, Goldman argues, "Notice that the mere fact that a source of warrant is subject to empirical defeat does not show that it itself is empirical."(Goldman [1999] p. 7) But this emphasis on positive non-experiential warrants, be they conceptual or reliabilist in nature, has blinded these philosophers to a serious problem with their conception. How, exactly, do we distinguish, to use Burge's terminology, the rational support for an attitude from its vulnerability to criticism? I will argue that the nature of our positive support for some belief, or justification more generally, is one side of an interdependence whose other side is vulnerability to defeaters. Justification and defeasibility do not come apart so easily.

The reason I think this is best approached, at first, anyway, by analogy: suppose someone were to claim that the room he was in was a room whose warmth was dependent solely on the heater. Suppose further that he added that, sometimes, the room was made colder by the air outside the room. So, he argues, the warmth of the room is dependent solely on the heater, but nonetheless, it turns out that in some cases, the room will be cooled down by cold air on a winter night. This argument is crazy: the warmth of the room is clearly dependent on both the heater and the air outside the apartment (and other things, no doubt). What the argument ignores is that the warmth of the room is determined by both the heater and the windows: both the heat from the heater and the cold from the window occasion a change in the overall warmth of the room.

Likewise, advocates of the weakly a priori ignore that justification, in being effected by empirical defeaters, is thereby determined in part by empirical elements. A warrant can not be dependent for its justificational force solely on non-experiential

considerations, and still be vulnerable to experiential defeats. If a warrant is vulnerable to experiential defeats, then experience is relevant to the justificational force that that warrant has. It is exactly like the room and the heater: just as the warmth of the room is not determined solely by the heater when it is admitted that cold air comes in sometimes from the outside, so, too, the justification for the warrant is not determined solely non-experientially when it is admitted that experience can sometimes defeat or diminish the justification.

Let me be clear about my argumentative strategy. I do not claim that I have some knock-down argument that shows that a priori warrants can't be empirically defeasible. Clearly, many distinguished philosophers think they can. What I have is a suspicion, just sketched by analogy above, that there is something amiss about their notions of a priority. In particular, my worry is that justification is a broad class, which can be augmented by confirmations or diminished by defeats, and careful attention to this fact means that one particular, very popular, conception of the a priori doesn't seem to make a lot of sense. In section (1a), I will briefly discuss two supposed examples of empirically defeasible a priori warrants, and suggest that both trivialize the a priority. I will return, in section (1b), to a more general discussion of the relationship between justification and defeasibility.

a) How empirical defeasibility trivializes the a priori

In this section, I will suggest that conception (W) strips the a priori of its philosophical interest, particularly when the question is the a priority of logic. I will

begin by rehearsing some arguments from Field and Kitcher which suggest in general why (W) waters down the notion of the a priori beyond respectability. Then, I will look at some actual supposed examples of empirically defeasible a priori warrants, and show how they trivialize the notion in unacceptable ways. Both examples make clear a point that Kitcher emphasizes: that (W) cannot do at least some of the work that the a priori has traditionally been asked to do. In addition, I will argue that both examples show how (W) makes a priority a less interesting issue, though in very different ways. The first example blurs the traditional line between the empirical and the a priori, and the second example reduces the interest we should take in the a priori by trivializing the whole idea of what counts as an empirical defeater.

The first general point to make is one emphasized in Field [1998]: that it is "almost incontrovertible" that logic is a priori according to (W). For how could we have empirical evidence in favor of logic? As Field points out, in order for logic to be supported by empirical evidence, we would have to believe that we had some empirical evidence which seemed to favor the logic in question. But since the assessment of evidence requires evidential systems, which in turn use the rules and principles of some logic or other, we would have to already feel justified in using a logic to assess the evidence in question. That is, in order to have something that we regard as evidence for logic, we would have to already feel justified in using logic. If logic can be justified at all, then it must be justified prior to our having empirical evidence in its favor. This being the case, the a priority of logic in the sense of (W) seems virtually guaranteed.[4]

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[4] See Field [1998] pp. 1-2

But this hardly solves the question of whether empiricism about logic is or can be right. There are residual areas of disagreement with respect to the epistemic status of logic between empiricists and their critics that are not addressed by simply agreeing that logic is a priori in the sense given by (W). Many have worried that there is something deeply problematic about the idea that logic is revisable for empirical reasons. Here, I am simply pointing out that (W) does not really get to the heart of the difference between empiricists and their critics regarding the epistemic status of logic.

Of course, we might just say that logic is a priori in (W)'s sense, and simply define the further issues between the empiricist and her critics as differences over the revisability of logic. That is, we might just stipulate that by 'a priori' we mean (W), and so admit that logic is a priori, while still maintaining that there are questions about the empiricist's program. But I think the issue is a little more difficult than the merely verbal one that this solution would suggest. The problem, I think, is that this glosses over how much will have been lost in the traditional debate between empiricists and rationalists. This point is one which Kitcher [2000] has emphasized.[5] A priori knowledge has traditionally had a particular functional significance for philosophers who appeal to it. As Kitcher puts it, "...a priori principles can be taken for granted in our future empirical investigations. Our scientific hypotheses are revisable—we have to be on the lookout for possible experiences that call them into question—but with respect to our a priori

[5] Although the upshot of his discussion is that the real distinction is between what he calls tradition dependent conceptions of knowledge, like those the reliabilists favor, and tradition independent ones, which have as their goal the synchronic reconstruction of the totality of our knowledge. That is, he does think that what I'm calling (W) flawed and for exactly the reasons that I'm pointing out. It's just that he seems to think that the whole rationalist-empiricist debate is itself something that is better left by the wayside.(See Kitcher [2000]. )

knowledge we don't have to harbor such worries."( Kitcher [2000], p. 77) From Kant to Carnap, a priori warrants have seemed constitutive of our experience in such a way as to guarantee that they won't conflict with experience. If we simply adopt (W), it seems we will no longer be clearly singling out an epistemically special or privileged class of beliefs, distinct from ordinary empirical beliefs.

After the above quote, Kitcher makes clear exactly how different (W) really is from the traditional conception a priority. According to Kitcher, it seems likely that those who base their conceptions of a priority on conceptual connections would have to admit that it is weakly a priori that that probability of rolling a 6 with a perfectly symmetrical, homogenous die is 1 in 6. After all, if the sides are all symmetrical and the material really is homogenous, we shouldn't need evidence to be warranted in thinking that the chance of our rolling any particular number on the die is 1/6. But as Kitcher points out, many probabilistic claims in what we ordinarily regard to be a posteriori disciplines make appeal to symmetry and invariance arguments. For instance, biologists make probabilistic claims about sex ratios in various populations, and physicists make claims about particles in particular mathematical spaces, all by appealing to basic principles of symmetry and invariance. So if we allow that the probability of rolling a 6 is weakly a priori, then we must allow that these other claims can be weakly a priori as well. To use Kitcher's term, this will result in an "explosion" of a priori knowledge.[6] Now, maybe advocates of (W) are amenable to this. But at the very least, they must admit that when

[6] See Kitcher [2000], pp. 78-80

claims in physics and biology start turning up a priori, much of the traditional debate has been lost. (W) will thus blur the distinction between the empirical and the non-empirical.[7]

Up until now, the considerations against (W) have been very general. I want now to get a little more specific, and briefly examine two supposed examples of empirically defeasible a priori warrants. The first supposed example of an empirically defeasible a priori warrant that I will examine is the conceptually based one found in Burge [1993]. According to Burge, we are a priori warranted in believing what others tell us, though in individual cases, we may have empirical reasons to see that the warrant is not valid. Burge argues for a very general claim which he calls the "Acceptance Principle:" "A person is entitled to accept as true something that is presented as true and that is intelligible to him, unless there are stronger reasons not to do so."(Burge [1993] p. 467) This principle states that all persons who receive and understand content presented as true from other intelligible sources are entitled to believe that said content is true.[8] Burge calls his position a "default" position. It says, essentially, that unless there are reasons to the contrary, one is justified in taking another's intelligible testimony as true.

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[7] Of course, that the lines between the empirical and the non-empirical are blurry has always been at the heart of Quine's views. So there is a sense in which we should see this admission as a concession that Quine was right. In fact, I think the whole weak conception of the a priori is an attempt to preserve a priority (if only in name) while admitting that Quine was right about revisability. This is, as Kitcher [2000] puts it borrowing a phrase from Michael Devitt, 'fig-leaf a priorism.'

[8] Burge distinguishes justifications from entitlements. As an entitlement, it "need not be understood by or even accessible to the subject."(Burge [1993], p. 458). This entitlement, however, also has a justification. Justifications, for Burge, "involve reasons that people have and have access to."([1993], p. 459) According to Burge, both justifications and entitlements can be a priori, because, both justifications and entitlements can be had in virtue of conceptual connections which in no way depend for their justificational force on experience. So, for instance, with respect to the Acceptance principle: we are "entitled to acquire information according to the principle—without using it as justification..." but, equally, Burge does give a full blown justification for that entitlement. In both cases, Burge argues in a very neo-Kantian way, citing the obligations on interpretable speech that indicate the presence of rationality, and the subsequent constraints on rationality which make truth telling obligatory. Claiming that all of the connections that he

The first point that I want to make here is simple: both of Kitcher's worries, about the blurring of the traditional distinction between the empirical and the a priori, and about the way empirically defeasible warrants fail to be able to serve constitutive roles in our understanding, are exemplified in Burge's example. Burge's Acceptance Principle essentially shows that there are conceptual reasons to think that when a person says 'p' they also believe it, and hence, believe it to be true. That is, the Acceptance Principle gives conceptual credence to the idea that we can take others testimony as true because if they say something, then they believe it. But the thesis that people usually believe what they say is also, quite plausibly, a more empirically determinable thesis. We can imagine sociological and/or anthropological studies demonstrating that people in a given population do or don't believe what they say. For instance, even informal studies of used car dealers suggest the very substantial empirical thesis that used car salesmen don't believe what they say when they are discussing the cars on their lot. Burge's example supports Kitcher's claim that the weaker our conception of the a priori, the more seemingly empirical warrants turn out to be a priori. And the more seemingly empirical warrants turn out to be a priori, the more a priori warranted beliefs we will appear to have. Likewise, Kitcher's other point is equally vindicated: to the extent that weakly a priori warrants are empirically defeasible, they lose their ability to play central constitutive roles in our understanding of the world. Burge's principle is only a default principle: it does not tell us that we can rely on people believing what they say, and act as though they always do. That would be quite foolish in our interactions with used car

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cites in his explanation are purely conceptual, Burge argues that both the entitlement and the justification confer a priori warrants. (see [1993] pp. 470-2)

dealers, as Burge would himself no doubt admit. Rather, the principle is an "other things being equal" sort of guide to our understanding of the world and its inhabitants.

Let me be quite clear: I am not saying that something like the Acceptance Principle can't be a priori. Burge's discussion of the conceptual connections that justify the principle is quite subtle and astute, and seems plausible (for those who think that conceptual connections can do this sort of work). My point is simply that the line is being blurred between the empirical and the non-empirical, and this blurring has resulted in the loss of one particular aspect of a priority which has traditionally seemed important: that a priori knowledge can serve as a guide to experience in a way that is more stable than is ordinary empirical knowledge. Once we weaken the requirements on a priority, important aspects of a priority are muddied. Burge, of course, would applaud this muddying of the waters. But Kitcher, I think, has given us good reasons to be concerned that there are aspects of conception (S), the abandonment of which would leave us with a seriously impoverished philosophical distinction.

I have a second problem with Burge's example, that has to do with his specific formulation of the Acceptance Principle. Notice that this principle includes a clause which allows for exceptions: unless there are stronger reasons not to do so. This means that the Acceptance Principle itself is not, therefore, empirically defeasible. After all, how could a principle be defeated for empirical reasons if it includes, as part of its very formulation, the fact that other considerations, including empirical ones, might be stronger. It says in advance: don't always listen to me. Of particular relevance to this inquiry: don't listen to me if the empirical evidence indicates otherwise. So no empirical evidence could possibly prove the principle wrong.

This leaves one wondering what exactly was supposed to be the example of the empirically defeasible a priori warrant. Burge's intention, no doubt, is that the individual instances of the warrant are what are both a priori and empirically defeasible. But I have a suspicion about Burge's argument here, stemming from my way of conceiving the relationship between justification and defeasibility, as described by my analogy in the first part of this section. It seems to me that to the extent that some instance of the Acceptance Principle might be defeated by something experiential, that instance will be empirical. Though he doesn't discuss a specific example, Burge does defend his view by giving three reasons why being empirically defeasible does not make a particular warrant empirically based.[9] This suggests that Burge is sensitive to the type of problem that I'm bringing up. Unfortunately, none of the three worries that he addresses seems to match my own. Nevertheless, Burge does say something toward the end of his paper which is relevant: according to Burge, it is "the extended body of justification" that is relevant to a priori entitlement, where by this he means the justification that extends beyond individuals. He says, "If I am a priori entitled to accept an interlocutor's word, but the interlocutor provides with empirically justified information, it would be wrong to characterize my knowledge of the information as a priori. Similarly, if my source knows a proposition a priori, but I must rely on empirical knowledge to justify my acceptance of the source's word, it would be wrong to say that I know the proposition a priori."(Burge

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[9] Burge says, "Even if the Acceptance Principle is not an empirical principle, it may seem that particlar entitlements sanctioned by it, 'applications', must inevitably be empirical."(Burge [1993] p. 476) He then considers the following three versions of this worry. First, he discusses the objection that in order to know what one is being told, one must use perception (e.g., to hear or see the words).(pp. 476-481) Second, he considers that one might think that perception is necessary for us to understand that some bit of testimony is "presented as true."(pp. 481-4) And lastly, he considers the objection that perception is necessary to appreciate the context of the receipt of the testimony.(pp. 484-6)

[1993] pp. 486-7) In chapter 3, I will argue that there is a more modest form of empiricism, whereby an evidential system should count as empirical as long as it seeks to conserve evidential beliefs. The idea here is that as long as we judge our total belief system by its success at capturing the evidence, then that system should be regarded as modestly empirically justified. If I am right about this, then Burge's insistence on the extended body of justification would amount to a concession that empirical evidence is relevant to instances of the Acceptance Principle. After all, if all our beliefs are ultimately judged at least in part by how well they account for evidence, then the extended body of evidence for our beliefs is always (modestly) empirical. Since my arguments for this view have not yet been made, however, I will rest content with the following worry: that instances of the Acceptance Principle which are clearly empirically defeasible are only questionably a priori. This, in combination with the fact that what is clearly a priori, the Acceptance Principle itself, is not really empirically defeasible, suggests that something may be amiss about Burge's example of an empirically defeasible a priori warrant. [10]

So much for what's suspicious about a conceptualist example of (W). The second supposed example of an empirically defeasible a priori warrant is the relabilist one in Casullo [1988], and as we will see, the problem here is different. In this example, Phil is a logician who believes, based on a proof that he has carefully considered, that p entails

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[10] A third issue I have with Burge's example I'll only put in a footnote, since it's from an answer he gave to a question of mine at the NYU seminar. Burge seems to distinguish different strengths of a priority, though he admits that he is unsure exactly how to make the distinction. But he thinks that the a priori connection between belief and truth (that belief aims at truth) is not empirically defeasible. So he clearly thinks that, in addition to a priori warrants that are empirically defeasible there are other stronger a priori warrants that are empirically indefeasible. This is not a problem per se: I'm all for blurring the lines. But its does, I think, make clear that allowing empirically defeasible a priori warrants does not settle the question of the existence of the stronger, empirically indefeasible a priori warrants.

q. Phil has been undergoing a series of tests by his cognitive scientist friend Maria which have established (with, we are supposing, great accuracy) that whenever Phil makes an error in reasoning, there is some interference pattern discernible in his brain waves. While Phil was considering his proof that p entails q, Maria was monitoring him, and her tests showed the interference pattern which is characteristic of Phil's being in error. When Phil learns that the interference pattern was present, he no longer thinks that he was warranted in the belief that p entails q.[11] Casullo argues that this is an example of an a priori warrant that is empirically defeasible: Phil's warrant supposedly gets empirically defeated by the news that his brain was exhibiting a particular pattern of waves.

This reliabilist example does makes clear at least half of the point I emphasized against Burge: reliabilist weak a priori warrants can no longer serve as guides to our experiences which are somehow guaranteed to be correct. Casullo is quite clear that on his picture, some a priori warranted beliefs will be false.[12] However the problem with our reliabilist example is not that reliabilist weak a priority threatens an explosion of the a priori. Rather, what is bothersome about this example (and others like it[13]) is the nature of the empirical defeater, and the way it seems to reduce the a priori to questions of

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[11] Casullo offers another example which is supposed to show that logic is rationally revisable for more conceptual reasons. Since this is slightly off the current topic, I will not discuss it here. Goldman [1999] clearly endorses the idea of a priori warrants which are empirically defeasible. The example that I discuss in the text is one that Goldman appeals to in his defense of (W).

[12] See, for instance, Casullo [1988] p.199. Specifically, examples will either involve false a priori warranted beliefs, as the example under discussion does, or they will involve, as does the one mentioned later, misleading evidence. I am arguing in essence that either option is problematic.

[13] Others have suggested equally trivial defeaters. For instance, Kripke (Kripke [1980] p. 35) has suggested that such things as our reliance on computers in certain proofs, and Kitcher (Kitcher [1984] p. 40) that such things as the possibility of errors in very long proofs, might be sufficient to show that purported a priori warrants are empirically defeasible.

whether or not we are performing at the top of our abilities. That we could be mistaken that we have a proof of a logical theorem is not a new or startling thesis. Simply put, this example capitalizes on the fact that we are reasoners with limited abilities, and so in some cases, our abilities will lead us astray. This fact, surely, cannot be what centuries of philosophers have been worrying about when they discuss the a priori. Rather, what concerns philosophers, both proponents and opponents, is the idea that there might be some kind of knowledge which we have which doesn't depend in any way on our thinking about the world that we actually live in. The possibility that this sort of knowledge is defeasible is not the simple fact of our limited reasoning abilities. Its about having the best abilities one could hope for, and still finding out that the beliefs one has arrived at using those abilities do not seem to hold true.

If a priority is to retain any of its philosophical interest, what is needed is some way of ruling out trivial empirical defeaters like these. To this end, we can follow Field and distinguish two ways in which an a priori warrant could be undercut. Primary undermining evidence is "evidence that outweighs the non-empirical basis we had for making the claim." Secondary undermining evidence is evidence "showing that we did not after all have the non-empirical basis we thought we had for making the claim."(Field [1996] p. 362) Field proposes that we only allow primary undermining evidence to be seen as a legitimate empirical defeater. So, for instance, suppose we have performed a proof of a theorem, and come to believe said theorem on the basis of the proof. Then, suppose, we are informed by experts (be they machine or human) that our proof is in error. Here, it's clear that their testimony merely serves to inform us that our own evidence is not sufficient. So, Field's distinction seems to rule out the kind of trivial

defeaters that I am concerned to rule out. On the other hand, if we gathered enough evidence to decide to abandon classical logic for quantum logic, this would be an example of empirical evidence coming to outweigh some prior conceptual evidence.[14] In this case, the a priori warrant (to use the principles of classical logic) would be seen as empirically defeated.[15]

With this distinction in hand, we can see that the Casullo example never clearly shows that an a priori warrant can get defeated by primary undermining evidence. It is only ever defeated by secondary undermining evidence. Surely the ideal credibility of Phil's belief is its credibility when all systems are working properly. If Phil's belief comes about as a result of processes which made an error, then the belief in question is not really a priori. We would not want to say that the destroyed processor of a person with a head injury should still be thought as capable of issuing a priori warrants. Warrants for belief are a priori justified only under the assumption that the systems that produce them are working in proper order. Casullo does give a variant on the above

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[14] Well Field argues that even the decision to revise classical logic in favor of quantum logic could be seen as based purely on conceptual reasons.([1998] pp. 15-6) But I think that if any example is a good example (and I do think there are good examples), then quantum logic is—after all, the evidence that is supposed to motivate the revision here is experimental evidence in physics. I think most people share my intuition on this one, but I could be wrong. At any rate, I will more fully defend the empiricality of the quantum logic example in Chapter 3.

[15] There are other suggestions as to how to rule out these counter examples to apriority. Burge(1998) argues that the cases which Kripke and Kitcher consider are not convincing because they do not show that the source of the justification for a priori beliefs is essentially empirical. He argues that though computers are empirical things, the programs which actually run the proofs are justified, if at all, by the same facts which justify our own ability to do the proofs. Burge's concern is with the "source" for the a priori justification, not with the packaging that delivers forth the actual beliefs in question. If a belief is a priori, it is because it has a particular kind of source for its justification, and this source can exist for beliefs attained through myriad empirical means. I personally prefer Field's way of delineating the issue, simply because I sometimes find Burge's talk of "sources" a little mysterious.

example,[16] but it faces the identical difficulty. The variants are that Maria's machine is incorrect that the brain waves are showing interference patterns (Maria's machine is malfunctioning), and also that Phil doesn't ever learn the results of the test. The identical difficulty is this: the example relies on Phil's being potentially mistaken about the validity of his warrant, and this is a far cry from the ideal credibility of Phil's belief. I conclude that this example is suspect, in that it seems to assume that beliefs can be defeated for very trivial empirical reasons.[17] The example of (W) that the reliabilist gives is just as guilty of making a priority uninteresting as is its more rationalist cousins.

In this section, I have tried to make clear some general concerns with (W), and show how two prominent supposed examples of empirically defeasible a priori warrants legitimize those concerns. Burge's example nicely illustrates Kitcher's point, and shows how seemingly empirical warrants might be a priori in (W)'s sense. Casullo's example seems irrevocably wrongheaded in its reliance on what Field has called secondary undermining evidence, and so trivializes the a priori in a different way. Most important, perhaps, both examples demonstrate that (W) a priority can't do all the work that many have wanted the a priori to do. In addition, though it is obvious that logic is a priori according to (W), it is equally obvious, I think, that there is still something that empiricists about logic and their opponents disagree about. I want to call that something the a priority of logic. What Quine, perhaps the most famous advocate of empiricism

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[16] See his [1988] pp. 198-99

[17] Since I used points in Kitcher [2000] to support my claims against the conceptualist example of (W), let me be clear that my criticisms of reliabilist weak a priority are not in agreement with Kitcher. In fact, Kitcher disputes the possibility of our even distinguishing between primary and secondary undermining evidence. However, since his criticism is based on our taking a reliabilist conception of the a priori seriously, and since as Kitcher himself notes, reliabilist a priori could only ever be committed to a priority in the sense of (W), it is clear that the reliabilist examples here are suspect.

about logic, was really objecting to was the way that philosophers like Carnap thought that a priori warrants could serve as constitutive guides to experience. That is, it seems to me that what bothered Quine was the idea that certain warrants could be taken to be correct guides for experience, no matter what those experiences were. These were the warrants that were traditionally labeled 'a priori', and that is, in part, why I want to reserve the use of the term to refer to warrants which are empirically indefeasible.

b) General Questions Regarding the Weaker Conception of the A Priori

In introducing the distinction between the two conceptions of a priority, I suggested that there was a connection between justification and defeasibility that was not respected by (W): if some belief is said to be justified with out reference to experience then no empirical concepts should be relevant to whether or not we should have the belief in question. It is like the temperature in the room and the contributions of the heater and the window, in my analogy above: if empirical evidence can be seen as defeating certain justifications, then those justifications are, at least in part, empirically determined. That is, whether or not they are justified, and to what extent, is dependent on empirical information. To put it in a slogan: justification independent of experience ought to include immunity from falsification by experience. In this section, I want to again discuss this general claim, and defend it against an argument of Casullo's.

Before doing that, however, I want first to address a potential worry someone might have about my analogy. I have been comparing justification with the warmth of a room, but one might think there is a significant disanalogy between them. Whereas the

warmth of a room is something that noticably fluctuates in both directions, increasing or decreasing continually, justification seems to be more a matter of thresholds. Once we are justified in believing that 2+2=4, we do not further confirm our belief every time two things add to two things to make four things. Likewise, we do not think that every black raven we see is also a defeater for the belief that all ravens are green. Justification is more all or nothing than warmth. Once we are justified in an a priori belief, nothing further confirms the belief, but if the room reaches a certain level of warmth, say body temperature, more hot air will make the room noticably warmer.

But on closer examination, the apparent disanalogy is more a matter of what the thresholds are. Justifications exist only relative to knowers, or communities of knowers: beliefs are justified relative to what is known and how it is known. Whatever one's views on the metaphysics of justification, one would not say that someone is justified if they believe $E=MC^2$ because a pixie appeared to them in a dream and whispered it into their ear. Likewise, I want to say, there is a similar sense in which heat exists relative to perceptions of warmth. Again, I do not mean to preclude the view that temperature does have some objective measure, simply to point out that it also has some effects on us which are dependent on us and the kind of sensory beings we are. We do, in fact, sense it go noticably hotter when the temperature gets up over 100 degrees.

Now, the apparent disanalogy above can be readily explained by the fact that the thresholds for justification are more closely connected to us and our cognitive capacities than are the thresholds for temperature tied to us and our sensing capacities. While it is true that one feels it get noticably warmer when the temperature goes above 100 degrees, there actually is a point, much hotter than that even, beyond which further increases in

heat won't be felt by a human being. I don't know what it is exactly, but I imagine humans wouldn't really notice a difference between, say, 130 degrees and 140 degrees, Farenheit. With warmth, as with justification, there are thresholds beyond which further changes would not be felt. Its just that the thresholds in the case of warmth are wider apart, and do not really occur much for human beings to actually notice. So, there is no disanalogy between warmth and justification in this regard.

Some might think that my picture has a very unattractive consequence: if justification really is like warmth, then we should say that though 2+2=4 is, still we should see confirmations of this belief every time two things add to two things to make four things. This is not the place to fully discuss this issue, but let me just say that this may seem unattractive but it has some plausibility. The thought behind it is that when two things add to two things to make four things, this could be evidence for 2+2=4, it's just that we don't take it to be. Someone could, though, and this is why it is important for us always to recognize that this is evidence, even if the belief that it is for is one which doesn't, in fact, need evidence like this.[18] There is some sense behind the idea that the world confirms us in our most basic mathematical beliefs. I agree it is not how we usually talk about things, but I do not think that it really so counter-intuitive, after all. I realize much more could be said on this subject, but I think that dwelling on it any longer

[18] I think what I have in mind can be better seen with a different example inspired by a discussion in Adler [2002] (see Chapter 10--my discussion of the example and the conclusions to draw from it differ from Adler's in a number of important ways). Suppose Paul believes that Bridget is an environmentalist based on the fact that she seems to go out of her way, whenever she can, to do something positive for the environment (e.g., recycling, donating to environmental groups, etc.). Suppose that Paul, having known Bridget many years, is justified in this belief. Should we count Paul's seeing Bridget recycle her papers, yet again, as a further confirmation? Not if he is already really justified in this belief. Yet, we want to count it as a further confirmer because it might have counted as evidence, had he just met Bridget.

will get us involved in something too major. Suffice it to say that this consequence is one which I would defend in another context. Let's return, then, to my slogan.

Casullo [1988] argues, though not in so many words, that my slogan is incorrect. Casullo considers a thesis which is meant to express the symmetrical relation of my slogan. This symmetry thesis is as follows:

> (ST) "If evidence of kind A can defeat or override the warrant conferred on S's belief that p by evidence of kind B, then the belief that p is based on evidence of kind A."( Casullo [1988] pp. 196-7)

As I understand it, Casullo must have intended us to understand 'based on' as follows: B is based on empirical evidence in the sense that empirical evidence was indispensibly a part of its justification. Empirical evidence determines whether or not we should hold B to be true, that is, whether we should believe B or not. This, I believe, is also the sense in which the warmth of the room is dependent on both the heater and the window, and so is in keeping with the way that I've been conceiving the issue. So, with respect to the issue we are examining, (ST) says that if empirical evidence can defeat rational evidence for some belief, B, then B is based on empirical evidence (and vice versa). For example, suppose that B is the belief that we are warranted in forming beliefs according to the distributive rules. What (ST) says is that if empirical evidence (say from quantum mechanics) can override rational evidence for B, then B is based on empirical evidence.

Casullo rejects (ST), arguing that it would be counter-intuitive in the case of EEG arguments against our privileged access to our own pains and itches. According to Casullo, we might eventually get to the point where EEG readings are fine-grained enough to be able to see when a pain region or an itch region is firing, and they may

become accurate enough so that we might take them as evidence over introspective phenomenal reports. But if we adopt (ST) we have to conclude based on the possibility of this kind of EEG evidence, that our current introspective beliefs that we have pains and itches are really justified only by neurophysiological evidence. This, according to Casullo, is wildly implausible, and so, argues Casullo, (ST) must not be correct.[19]

But this is unconvincing. What is wildly implausible, if anything, is that our pains and itches might be justified only by neurophysiological evidence. That is, there may be reasons to think that this kind of scenario is not possible, given our private access to the veracity of the pains and itches. But whatever one's opinion about that, surely (ST) itself shouldn't be seen as the culprit here. (ST) is extremely plausible in many other cases. [20] Consider sensory modalities: it seems very reasonable to think that if, for instance, nasal evidence can override visual evidence for some belief (I see what I think looks like a lemon but on smelling it realize that it is a small, odd-shaped, yellowish orange), then that belief ('This is a lemon') is based on nasal evidence, in the sense of 'based on' which I emphasized above. That is, my reason for no longer believing 'This is a lemon' is determined by or dependent on nasal evidence. Or consider another example, from jurisprudence: if DNA evidence can override eye-witness testimony (the eye-

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[19] See, Casullo [1988] p. 197

[20] Actually, I think a slightly weaker thesis is in order. My argument is that if an a priori warrant can be empirically defeated, then the warrant is not really a priori. Is a non-a priori belief necessarily empirically justified? Why couldn't it just be not justified at all? This might be the case, after all, with our logical beliefs (though I will argue otherwise). At most, I would want to advocate a weaker symmetry thesis:

> (WST ) If evidence of kind A can defeat or override the warrant conferred on S's belief that p by evidence of kind B, then the belief that p is not based on evidence of kind B.

Or, in other words, if empirical evidence can defeat rational evidence for p, then p is not based on rational evidence (and vice versa). Since the difference between the two theses is unimportant in terms of my point in the paper, I will continue to speak of (ST) in the text, but the thesis that I really consider myself to be committed to is the slightly weaker (WST).

witness says he saw the defendant murder Smith, but the DNA evidence shows that the blood of the killer found at the scene does not match the defendants), then the beliefs of the jurors concerning the defendant's guilt (e.g., that the defendant is not guilty) are based on DNA evidence. I do not think it is a good idea to draw general conclusions about (ST) from examples which are as vexed as is one's private access to one's pains and itches, especially when it is so plausible in other clearer cases. So, I am not convinced by Casullo's argument.

Where does all of this leave us with respect to the conceptions (W) and (S) of the a priori? I have argued the problematic nature of (W) by showing how supposed examples of empirically defeasible a priori warrants weaken it so much as to call into question whether it can do the work that traditionally wants doing. I have shown how conceptualist weak a priority blurs the lines between the a priori and what is usually thought of as more empirical, and how reliabilist weak a priority completely trivializes the idea of an empirical defeater. Furthermore I have very briefly sketched a general suspicion I have: that any empirically defeasible a priori warrant is just too empirical to be a priori. This is due, I have suggested, to there being a dependence, analogous to that between the warmth of the room, the window and the heater, among justification, defeaters and confirmers. I have argued that if something is disconfirmed by empirical evidence then it is based, at least in part, on empirical evidence. I think the moral is this: (W) is a deeply dubious notion. We should, therefore, think of a priori warrants as empirically indefeasible.

Having said all that, however, let me also say that I hope that the arguments of this dissertation are of interest whatever one's views regarding the issues discussed

above. For those who remain convinced that (W) is a notion worth exploring, let me then just stipulate: the notion of the a priori that I will be concerned with is the stronger one, whereby a priori warrants are not empirically defeasible. Nothing that I say in what follows depends in any way on one's agreement with me that this notion of the a priori is the better one. For the reader who disagrees that (S) is the only notion of the a priori of interest, this dissertation should be seen as an exploration of the arguments that logic is empirically indefeasible. Surely, this question is of interest in its own right. Many do argue that logic is empirically indefeasible. For instance, Field does. As I noted above, the revisability of logic remains a vexed and intriguing issue, whatever one's views about the nature of the a priori.

## II) Logic

There is a certain ambiguity in the term 'logic'. Logicians and mathematicians work with various logics, exploring and evaluating their formal properties, but cognizers are also said to reason in accordance with some logic. In the former sense, logic is thought by many to describe a realm of necessary truths, relations which obtain in virtue of certain necessary conceptual structures. This is not the aspect of logic that I'm interested in in what follows. Rather, my concern is with the latter notion, what we might call applied logic: logic as it should be used in various belief forming and belief retaining behavior.

The reason for this focus should, I hope, be clear: I argued above that by 'a priori' I will mean empirically indefeasible. Unless the logic we are considering is

applied, there is no sense in which experience might be relevant to our acceptance of it. As George Rey has pointed out, empirical information isn't relevant to conceptual necessity:

> ...a defender of the a priori can quite plausibly argue that the revision of any claim about a pure...necessary truth could never really depend upon anyone's actual sensory experience, since, at best, all that such experience can provide of relevance to that truth is the presentation of the actual as simply on e among the many possible worlds that the pure necessary truth concerns. A pure necessary truth is not a claim about anything idiosyncratic to the specific world we experience; so how could its justification depend in any essential way upon the character that world happens to present to us?(Rey [1998] p. 29)

I think that Rey's point here is actually a little confused, since he takes the question about the a priority of logic to be a question about logic in its former sense (and he seems to confuse it with metaphysical necessity, to boot). But *if* there are necessary conceptual truths in the first sense of 'logic', it seems that the status of these is left untouched by anything that experience could deliver. Since my concern here is to explore whether logic is a priori, and if it is, how a priori it is, we would do well to focus on a notion where experience and evidence at least have a chance at being relevant.

Most philosophers seem to think of the question of the a priority of logic in terms of the a priority of formal rules and principles of deductive, and perhaps inductive, logic. In the next chapter, when we examine the various metaphysical views regarding justification, we will see that the focus is almost exclusively on deductive rules. Nevertheless, it will be important in what follows that we also pay attention to a different class of rules, what are usually called pragmatic rules. This will be important because my argument will depend at least in part on showing that we could revise deductive rules using pragmatic rules.

My discussion of all of these various rules centers, of course, on whether or not they are empirically defeasible. I will follow Field in pursuing this question in terms of what he calls "evidential systems". These are idealizations of an organisms belief-forming and belief-retaining behavior. Evidential systems include deductive, inductive, and pragmatic rules, and are supposed to represent how an individual goes about evaluating the available evidence. For instance, evidential systems whose underlying deductive rules are classical would have it that we can believe 'q' when we believe 'p or q' and 'not-p'. But evidential systems whose underlying logic is quantum would not license our inferring according to this rule.[21] The question of whether logic is a priori thus becomes a question of whether or not it is possible to revise any of the deductive, inductive, or pragmatic rules that underlie our best evidential systems.

Before concluding, let me say a word about my terminology. Primarily what is a priori is a warrant.[22] Sometimes, I will talk about the a priority of logic, or the a priority of our methods of belief assessment, but what I will mean by this is the a priority of the warrant for us to use the rules and principles of the system in question. Warrants, in general, are warrants for beliefs, so I will also sometimes speak of the a priority of beliefs. (Although, again, I take it to be warrants which are primarily a priori.) For instance, somone who believes both 'it is raining' and, 'if it is raining then the game will be called', would be warranted by the rule modus ponens to believe 'the game will be

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[21] This is supposing that these logics could serve as the underlying structure for some evidential system. This is certainly something that Field objects to about quantum logic. (see Field [1998] p. 15) Right now, I am merely explicating the notion of evidential systems, and not defending the coherence of our becoming quantum logicians. This being the case, I can pass over the difficulties that bother Field.

[22] Many contemporary writers on the subject agree that warrants are the primary objects of a priority, e.g., Burge [1993], Casullo [1988], Goldman [1999]

called.' Many writers focus on the a priority of propositions or knowledge.[23] My preferred way of talking is easily translated into one of the more traditional means of discussing the a priori: if we are warranted in using some rule or principle of logic, then one could say that we are warranted in believing the proposition which expresses or instantiates the rule; likewise, in this situation, then one could say that our knowledge of the rule has an a priori warrant. I do not think that anything of importance with respect to the epistemological issues will be missed by my chosen terminology. I defend my preference on the grounds that warrants for beliefs, rules and principles incur less potentially discomforting commitments outside the scope of the current discussion: the status of propositions is questionable, and talking about logic in terms of knowledge may imply that our logical beliefs are more firmly justified than some have argued them to be (as we will see in chapter 5).

## Conclusion

In this chapter, I explained what I will mean by 'a priori,' The only sense of 'a priori' that I am interested in disagreeing with is the one that requires a priori warrants to be empirically indefeasible. Though some have defended a weaker notion of a priority, I have tried to call this notion into question. I have argued, first, that the whole idea of empirically defeasible a priori warrants is wrongheaded at a very general level. Second, I tried to show that the so-called examples of empirically defeasible a priori warrants in the

[23] For instance, Kitcher [1984] focusses on knowledge (see p. 21), as does, Kripke [1980] pp. 34-5, and Bonjour [1998] on propositions, (see p. 26).

literature are questionable. And lastly, I have defended my preferred understanding of a priority from a criticism by Casullo. With respect to 'logic', I briefly discussed the various types of rules whose a priority I will want to be investigating. In addition, I explained Field's notion of an evidential system, as this will be a very helpful way of thinking of the question of the a priority of logic.

## Chapter 2: The Metaphysics of Justification

A priori warrants are supposed to offer a kind of justification for their beliefs. Whether it be by default, or by consequence of some very important concepts, beliefs which are a priori warranted are beliefs which somehow don't need experience for their justification. In Chapter 1, we have gotten a little clearer about what we mean by a priori. In this chapter, we will spell out various views on justification. Philosophers have approached the issue of justification in general from one of two directions. On the one hand, one can examine it more from an epistemological perspective, with an eye, perhaps, toward giving conditions (necessary and/or sufficient) for a belief's being justified. On the other hand, one could be concerned about more metaphysical issues. Here, the key problems will be describing what does or does not make a belief justified: in making clear that the conditions laid down in the epistemological part of the inquiry are satisfiable in some way. My interest will be with the more metaphysical issues.

I will begin my discussion in this chapter by briefly comparing the metaphysics of epistemic justification to the more familiar metaphysics of ethical justification. Then I will describe three different general positions one might take with respect to the metaphysics of justification, and give examples of each. The examples are intended to be

merely illustrative. Each of the various positions that I will examine below has many different adherents and variations, and I do not pretend to be presenting an exhaustive overview of the current philosophical views in these regards. The three views are: nonnaturalism (the example is Peacocke's and Boghossian's conceptualism), naturalistic reductionism (the example is Goldman's and Rey's reliabilism), and nonfactualism (the example is Field's evaluativism). I will explain each view, and give some brief account of its answer to the question of justifying logical concepts. In addition, I will briefly raise some questions I have regarding the first two options above. My worry concerning conceptualism is that it doesn't really do a very good job at ruling out nonclassical logics, and so doesn't seem prepared to answer my target question question: might we revise logic for empirical reasons? And with respect to the reliabilists, I will raise two concerns that I have regarding how a priori reliabilist a priori really is. First it seems to me, that judgments of reliability will always be evidential, and so experiential. And second, that any plausible account of how these a priori systems get into our cognitive architecture may make them empirically defeasible in the sense that they may not always be fitness enhancing strategies. Lastly, I will explain Field's evaluativist view, whereby there are no facts concerning epistemic justification, simply us and our evaluations.

## I) The Normativity of Logic

It is often said that logic is normative. It is clear that logic, particularly as I am conceiving it, is the study of inference, and that decisions about which logic it is correct to use are decisions about which inferences we ought to make in various circumstances.

So as I will conceive it, different logics recommend that we have different beliefs, or recommend that we perform different actions. For instance, classical logic recommends that we believe that every possible sentence is either true or false (and behave accordingly, whatever exactly that means), intuitionist logic does not recommend this. It is the connection between formal logic and the principles of revision we use in our methodologies, I think, which underlies the intuition that logic is normative.

As I am conceiving it, then, we can think of logic as recommending that we have particular beliefs and/or perform particular actions. This sort of normativity is similar to the normativity of moral claims. Let us briefly review the metaphysics of the more familiar normative area of philosophy, ethics. Exploring the metaphysical status of the moral ought has long been of concern to philosophers. There is an extremely rich tradition of exploring the metaphysical underpinnings of our ethical or moral statements, in particular, of examining their factual status. This tradition has been responsible for bringing the non-factualist view to the highly sophisticated one it is today.[1] What follows will be an extremely superficial overview, intended simply to provide a framework in which to understand the ensuing discussion about justification more clearly.

Of primary importance to me regarding the metaphysical underpinnings of ethics is the distinction between factualism and non-factualism. The factualist says that rightness and wrongness are objective features of the world, facts which, though not quite as transparent as more ordinary sorts of facts, either hold or do not hold of the world that

[1] See Blackburn, [1988, 1993, among others] and Gibbins [1980]

we live in. The non-factualist, on the other hand, denies the existence of these sorts of facts. Of course, non-factualists need to give some account of what does justify us in holding our moral theories, and will in their first order ethics.[2] But the point will be that the first order commands of ethics won't get any normative force in virtue of corresponding to facts in the world. Much debate in meta-ethics surrounds the question of the extent to which non-factualists are really able to justify whatever theory they take to be best. For now, I wish to put those issues aside, and assume, for the moment, that non-factualism about ethics can give as full-blooded explanations of our ethical activities as can factualism. This is a topic I will address in chapter 5, though there, my concern will be with logic, not ethics. I would hope that some of what I say about logic would do equally well as a response to this issue for non-factualists about ethics, but I will not address this question directly. My plan is to assume in ethics, right now, as a way of making clear what the lay of the land is, something which I will defend later on in the case of logic: that non factualism does not collapse into some sort of hopeless relativism, and lose all normative clout.

Factualism and non-factualism are therefore, to be contrasted with relativism and skepticism. Both of these latter views deny that we have any reason to believe that our moral pronouncements have any transcendent normative force. Relativists believe that justification is a factual affair, but that the only real objective moral facts which are ascertainable are those which concern the opinions of one's current culture (or some such sociological fact). For the relativist (at least of the cultural variety), things are right or wrong according to how well they accord with what society currently thinks is right or

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[2] See Blackburn, [1998] Chapter 9. See also Blackburn, 1999

wrong. The skeptic, I think, agrees with the relativist that these facts are all that is available, but thinking that these sorts of facts are not really justificatory at all, concludes that we have no reason to prefer one moral theory over another. The details of these views will not be important, and I bring them up only as a contrast. For, whereas both relativists and skeptics will deny that we can say, for instance, that murder was wrong in Macedonia in 132 BCE, both the factualist and the non-factualist agree that, if we are correct that murder is wrong, then murder was also wrong in 132 BCE in Macedonia. For the factualist it was wrong because there is some fact which makes it wrong, and for the non-factualist it is wrong because our best theories of ethics tell us that it was wrong, and that is enough for it to be really wrong.

In the next two sections, I will explore the analogs of the factualist and the non-factualist positions for the justification of our logical beliefs. Just as with ethics, one could either be a factualist or a non-factualist with respect to the metaphysical question of what justifies us in our use of one evidential system over another. As we will see, factualists argue that there could be objective reasons for thinking that, say, an evidential system that employs classical logic is correct. Non-factualists, on the other hand, argue that we can be fully justified in adopting one method over another in spite of there being no facts which correspond to the objective correctness of one evidential system over another. Likewise, there is an analog of the skeptic's position, here that, lacking sufficient things to serve as facts, there is no reason to prefer one evidential system over another. I'm not sure if there is an analog of the relativist position that anyone has ever actually held, but settling this matter is unimportant to the current inquiry.

My discussion will be on the metaphysics of justification, but there is a closely related issue which we should be careful to avoid confusing with justification: the metaphysics of logic itself. This is a topic I feel a little uncomfortable discussing, in part because I'm just not sure what it would mean for there to be a fact about logical truth or logical consequence. Field, though a nonfactualist about justification, is a factualist about logic, where this seems to mean that there is some objective relation of logical consequence which obtains for objects in the world. I find this view a little strange, especially given the close connection between logic and justification which Field assumes in his argument for the a priority of logic. Field view seems to be that though it may be a fact that,

If p then q, and p, then q,

nonetheless there isn't a fact about whether we are justified in believing this to be true. But given that, for Field, logic also somehow forms the normative basis for evaluative inference, this combination of views is very puzzling. There are facts about logical consequence, and logic underlies justification (or evaluation more generally), but there are no facts about justification. Field's reason for tolerating this odd bunch of beliefs seems to be that he is only a factualist about logic because he doesn't see how to be a nonfactualist about it. In particular, the best versions of nonfactualism to date, those defended by Blackburn and Gibbard, (see footnote 1 of this chapter) both rely on a notion of something's following from something else to explain how our behavior is circumscribed by our endorsing particular norms. I am more inclined to a non-factualist view of logic, and think there may be a way our of Field's worry, but going into this would take us too far afield. For now let me just be clear that I intend my discussion to

be of justification only, and both my discussion and my argument will avoid any discussion of the metaphysical status of logic, itself.

## II) Factualism About Logical Justification

In this section I will briefly canvas several different ways of being a factualist about logical justification. We know from above that a factualist about the justification of logic will hold that there are some facts which make it the case that a person ought to employ one evidential system and its attendant logic over another, but as it stands, this sounds quite mysterious. What would these justificational facts looks like? Clearly, they are not going to be facts like the fact that, right now, my water glass is to the left of my computer screen, but what will they be? We will discern at least two different possible factualist positions currently taken, though there are no doubt more possible and actual views than those canvassed below. One of these positions is rationalist in spirit: it says that the facts that justify us in employing one evidential system over another, and thereby committing to the correctness of one logic over another, are conceptual, having to do with entitlements accruing from applications of various concepts (via certain meaning relations). The second position tries to reduce facts about justification to certain naturalistic properties, like reliability.

a) Logical Facts as Non-Natural Properties: Conceptualism

There is a rich tradition in philosophy of explaining the justification of our logical and mathematical beliefs by appeal to non-natural properties, especially to the idea that the justification will be derivable in some way from the concepts contained in said beliefs. There are probably as many different kinds of rationalist positions about these facts as there are rationalist philosophers discussing the issue. What they all have in common is the view that the meaning of our terms, in this case, the logical constants, somehow manages to secure that certain inferences will be justified, regardless of the facts in the world in which the inference is taking place. An examination of even all the current rationalist positions, let alone their historical precedents, would be a dissertation unto itself. My aim here is only to present options. As such, I will focus on just one sort of rationalist view which is held by current thinkers. This view is found in the writings of Paul Boghossian and Christopher Peacocke, but I believe it more or less is representative of what is important to rationalist explanations regarding a priority, namely the emphasis on conceptual truths being somehow the source of the a priority.[3] I choose Boghossian and Peacocke because they are notable, in the literature, for being particularly concerned with a priority. Also, Boghossian has shown a great sensitivity to the sorts of issues which are the focus of this dissertation, and which require that a discussion of the a priority of logic be conducted on its own. This makes them a natural choice to be the rationalist representatives in my overview of positions regarding the a priority of logic

[3] See Peacocke [1992, 1993, 1996] for his early development of his theory. Peacocke slightly amended his view in [1998]. For Boghossian, see [1996, 2000].

Boghossian has claimed that our a priori entitlement to think ourselves justified in using a rule of logic "flows naturally from a conceptual role account of the meanings of our logical words."(Boghossian [2000] p. 250) The idea is that some subset of the possible uses that, say, 'and' participates in will be seen as meaning constituting, e.g., the uses that instantiate instances of the introduction and elimination rules for '•'. 'And' is just the unique concept, if any, "whose value makes the inferences in that subset truth-preserving"(Boghossian [2000] p. 249). In other words, if we think of logical constants as getting their meaning from facts which guarantee that they are truth-preserving, then the inferences which are prescribed by said constants can't help but be justified. For these reasons, we are a priori entitled to infer as the rules of the various constants allow.

Similarly, Peacocke has argued that the a priority of logic is guaranteed by a combination of our understanding of the meaning of the logical constants and the truth conditions that that understanding demands. According to this view, we have an implicit conception of the concept, say, conjunction, which has us feeling primitively compelled by inferences like the following:

$$\frac{A \; C \; B}{A} \qquad \frac{A \; C \; B}{B} \qquad \frac{A \quad B}{A \; C \; B}$$

This understanding of the connective is what guides us in our formulation of a determination theory, where this is simply the theory which makes the semantic assignments all come out correct: in the case of the logical connectives, this means that the determination theory makes the above inferences truth preserving. Peacocke gives what he calls a "metasemantic" account of the a priori: we are justified in our adherence

to the rules concerning conjunction by the fact that the determination theory guarantees us that use of those rules are valid.[4] The "judgmental practices" mentioned in the possession conditions (from "A and B" infer "A") are guaranteed to be correct because that's how the determination theory makes its semantic assignments. This guarantee is what assures us that a priori truths are possible. For both Boghossian and Peacocke, the logical facts are determined conceptually and the entitlement is explained as a result of the constraints on semantic assignments: logical concepts determine those inferences which are necessarily truth-preserving, giving rise to an a priori entitlement to use the concepts in question.[5]

My main goal in this chapter is simply to present the various possible positions one could hold regarding the justification of logic. Nevertheless, it is worth raising, if only briefly, a potential problem with this whole line of explanation, especially since it will be important again, in the last chapter. The issue that I am concerned to discuss is the revisability of logic, and my claim is going to be that certain pictures of the

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[4] See Peacocke [1993,1996] for his so-called meta-semantic account of the a priori. For more on implicit conceptions of concepts, see his [1998]

[5] Peacocke and Boghossian differ in ways but I don't think those ways are relevant for the current purposes. If I understand him correctly, Boghossian objects to the idea that to possess a concept one must have a conception, implicit or not, of the inference rules for the logical concept in question. He thinks it implausible that cognizers with the concept of conjunction have any explicit conception that 'A and B implies A.' Boghossian notes, on the other hand, that if all Peacocke intends is that thinkers infer according to the above rule to have the concept of conjunction (and so presumably have an implicit conception), then this will fall short of a full-blown justification of the concept in question. (See Boghossian [2000] p. 240, fn 10) I must say that Boghossian's last criticism of Peacocke is a little puzzling to me. As we will see in Chapter 5, Boghossian ultimately thinks our justification for our logical beliefs must presuppose the implication (and thus our use of it) that Peacocke thinks we have an implicit conception of. So it seems to me that he must concede, at some point, that our use of an implication rule must precede our entitlement to use it. At any rate, I won't quibble over the details here. For current purposes, I will treat the two views as largely equivalent. In this chapter, I have focused a bit more on Peacocke's theory as his is the more fully worked out of the two. In Chapter 5, I will focus more on Boghossian as he most explicitly addresses the problem that is the focus of that chapter.

justification of our methods are more amenable to this possibility than others. Now, the potential problem that I see is this: its not very clear to me exactly how the rationalist explanation of logical facts is supposed to help with the problem that I'm concerned about. What I will be concerned to make sense of is the idea that we can revise one evidential method and its attendant logic in favor of another. From the perspective of the question, Is logic revisable in light of empirical experience?, the rationalist story just seems like a non-sequitor. For, why should it matter to the possibility of empirical defeasibility that we can tell a story about our a priori entitlement which cashes things out in terms of constitutive rules for use (be they implicit in our concepts or not)? My question is, "Can we make sense of our revising one logic in favor of another for empirical reasons?" and their answer seems to be, "We know we are entitled to infer according to certain logical rules because those rules guarantee the truth of beliefs gained according to those inferences."

To see better what I have in mind here, consider the work that this sort of theory can do. Both Peacocke and Boghossian emphasize the important work that their various semantic constraints perform. In particular, both emphasize that their respective theories can make clear that one is in the presence of a spurious concept. Consider Arthur Prior's famous connective 'tonk', whose implicit conception contains the following inferential rules

$$\frac{p}{p \text{ tonk } q} \qquad \frac{p \text{ tonk } q}{q}$$

This concept can be seen to be spurious because there is no uniform assignment of semantic values which will make both rules truth preserving. In particular, consider what

happens when p is true and q is false. To make the first rule truth preserving, one has to conclude that 'p tonk q' is true. But since q is false, this would make the second rule go from the true 'p tonk q' to the false 'q', and so the second rule requires that 'p tonk q' be false.[6] Another example of a spurious concept which Peacocke discusses comes from John Skropuski [1995]. The concept, 'tonkinois', has the following inference principles:

| | |
|---|---|
| x is brown-eyed | x is tonkinois |
| x is tonkinois | x is less than 30 feet tall |

Trying to formulate a determination theory for this concept will lead one to a dilemma. Suppose that there are conditions in the world that must obtain for some object, x, to satisfy 'tonkinois', and call these conditions P(x). Can a person rationally think that a thing is brown eyed without thinking that it is also P(x)? If the answer to this question is yes, then the first inference principle is not really constitutive, for it would require that the truth of a thing's being brown eyed entailed that that thing was tonkinois. If the answer to this question is no, however, then the concept 'tonkinois' can not be a concept which specifies conditions beyond those required of being brown-eyed. But in this case, the second inference principle, which would require that things be brown eyed and 30 feet tall, could not really be constitutive of the concept 'tonkinois'. In this case, it is seen that it is rationally impossible to accept the constraints imposed by these inference principles because there can be no concept which satisfies these constraints.[7]

These examples, I think, make clearer how determination theories work. Sense constrains and determines reference, as Peacocke notes, but referential considerations

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[6] See Peacocke [1992] p. 21, and Boghossian [2000] p. 247

[7] See Peacocke [1996] I think that Peacocke (and Boghossian) must simply mean by 'truth preserving' that the inference in question cannot take us from true premises to a false conclusion.

play a regulatory role with respect to senses. But they still don't address my question, because they say nothing about making sense of choosing between two non-spurious concepts. For what would happen if one was trying to decide between inferring according to one of two evidential systems, each with its own logical rules? It seems likely that at least some, though perhaps not all, of the deviant logics that have been suggested employ genuine, non-spurious concepts. In arguing for the preferability of giving a realist account of the logical constants, Peacocke [1987] tries to give an account like the one above for conjunction, for the other connectives. The constants other than conjunction present problems for Peacocke's program, because as he notes, it is unlikely that most people have any conception, implicit or not, of the rule, for instance, of ~-introduction as part of their primitive understanding of 'not'. In his discussion of negation, he says something very relevant to the current point. In trying to describe what is primitive in our understanding of 'not' and sufficient for classical negation, Peacocke appeals to the idea of an "incompatibility relation".[8] He notes there that the intuitionist will surely think herself entitled to construct an intuitionist account of intuitionist negation along the lines of his realist account of realist negation (which will include appeal to some sort of intuitionist imcompatibility relation), but doesn't think of this as any sort of objection to his view. He says, "When the intuitionist agrees that it is primitively obvious that ~ A is incompatible with A, what he means by 'incompatible' is not what the classicist means. What the intutionist means by the incompatibility of A with B is that the suppositions that A and B are both verified leads to absurdity."(Peacocke [1987] p. 165) The important point for our purposes is that

[8] Peacocke, 1987, p. 164

Peacocke seems to be acknowledging the phenomenon which I'm calling problematic. When the concept in question is negation, it is not sufficient just to rule out the concepts which can not get consistent truth assignments, as there are alternative semantics and semantic assignments regarding 'not' which do an equally good job at preserving truth in inference.

One is no more allowed to infer a falsehood from true premises in intuitionist than in classical logic. As such, it's hard to see what the conceptualist picture, in and of itself, could tell us about choosing one set of logical rules and principles over another, if both were genuine alternatives in the sense of being truth preserving. Rather, it seems clear that, given that there are two possible logics, each with its own concepts which constitute its valid rules of inference, and each of whose inferences are truth preserving, some considerations outside the realm of mere conceptual analysis will have to come in to decide between them. This is, in fact, implied by Peacocke in the same paper, which opens with the admission, "This paper is not, and could not be, a general defence of realism," but rather a theory about the sense of the logical connectives, without which, "...any realist's views will be incomplete."(Peacocke [1987] pp. 153-4) The general defense, in other words, is a separate matter from the conceptual analysis.

I should hasten to make clear that this is not necessarily a criticism of the view in question. Peacocke and Boghossian are not really trying to answer the question that I am asking. Rather, to recall the discussion of the first chapter, they seem more to be trying to make sense of the a priori by explaining a possible reason we might have for thinking that there are certain things we are a priori entitled to. To the question "Why should we consider ourselves justified in inferring according to the principles of classical logic?" the

conceptualist response is a perfectly reasonable answer: "Because, those principles are such that they are guaranteed to take us from true premises to true conclusions, and what else could we want from our logical principles?" There is, as I pointed out in Chapter 1, a big disagreement in the literature over exactly which version of the a priori should be defended or defeated. I think that the rationalist story as told by Peacocke and Boghossian is not a good answer to my questions in part because of this disagreement. I will not, however, press this issue any further.

b) Logical Facts as Natural Properties—Reliabilism

Of course, one doesn't have to be a conceptualist to believe that our a priori entitlement to infer according to the rules of logic is constituted by some fact. There are other accounts stemming from a more naturalistic tradition, like the ones to be found in Goldman [1999] and Rey [1998], which find the source for a priori knowledge not in rational intuitions but in the ability for some process to be a reliable producer of truth. These accounts emphasize, in general, that methodological rules that reliably lead one from true beliefs to other true beliefs are rules that we are justified in using. This being so, there is no reason to think that some of the rules which get so justified, such as logical and mathematical rules, might be such that they don't seem to make any reference to experience. If there were rules like these, and if they were in fact, sufficiently non-experiential, then we should think of ourselves as being a priori entitled to infer according to them. Notice that this version of factualism does seem to address the question which is the focus of my inquiries: assuming that it is not indeterminate (an

important assumption which I will argue in Chapter 4 is questionable), there will be a fact as to whether one evidential system employing one logic is more reliable than another system employing a different logic.

Let us focus on Goldman's account for a moment. Goldman subscribes to what he calls two-stage reliabilism.[9] The first stage is called the standard-selection stage: a community comes to find that certain belief forming processes and methods are reliable, and others are not. The second stage is called the standard-deployment stage: the community begins to judge the warrant for any given belief based on whether or not it was arrived at using the approved processes and methods. A priori warrants, according to Goldman, are just as likely to be candidates for these justification conferring processes as are empirical warrants. On this account, the reasoning and calculational processes which underlie our inferential practices, where these processes are understood as being something which is a part of a person's fundamental cognitive architecture, could be just the sort of processes that get justified in the way described by the two-stages above.[10] If this was the case, according to Goldman, we would have every reason to see this justification as a priori. How do we know that a given process is justified, as opposed to just being seen as justified by us? The reliabilism is here: the justified processes are the reliable ones, which in the inferential case, presumably, means that they are the ones which take us from true premises to true conclusions.[11]

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[9] See Goldman [1992]. Goldman's account seems to be the one that is endorsed by Casullo [1988].

[10] According to Goldman, only processes that are part of our fundamental architecture should be a priori warranters. So, in particluar, he leans toward the idea that learned methods should not be seen as a priori warranters (though Goldman is prepared to be convinced to the contrary, should a case be made).(Goldman [1999] p. 14)

[11] See Goldman [1999] especially pp. 10-12

Rey also gives a reliabilist account of the a priori, though his differs in its details from Goldman's. He asks us to imagine a person equipped with a little sub-system in their brain which, as a matter of contingent fact, can grind out theorems of first order logic. If this person is caused to believe "Nothing bites all and only those things that don't bite themselves," there is no reason not to classify their belief as a priori knowledge. Rey argues that it would be a priori, because the sub-system that he describes is a non-axiomatic system of natural deduction, relying on the standard rules like modus ponens, universal generalization and conditionalization (Rey [1998] p. 33), and as he notes, "any (empirical) assumptions that might have been made in the course of the proof were made only 'for the sake of argument', and were 'discharged' in the usual way, e.g., conditionalization..."(Rey [1998] p. 34) And it would be knowledge because each of the rules is justified by the fact that, as first order soundness proofs show, they are absolutely reliable: "it is impossible for them to produce a falsehood as a theorem."(Rey, ibid.) Just as empirical knowledge is the result of processes which are, in fact, reliable in the actual world, so too a priori knowledge can be the result of processes which are reliable at producing true beliefs from true beliefs, only this time, it would be in all possible worlds.

As I noted at the outset of this section, I think that the reliabilist account of the facts which constitute our justification for inferring according to certain principles of logic provide a much more satisfying answer to the kind of question that I am concerned to examine. However, the account above cannot be a justification that can claim to be a priori. Most simply, as Kitcher has argued, the account requires a priori justified beliefs

to be empirically defeasible.[12] If this is true, then the notion of the a priori that the reliabilist can defend is too weak by the standards of a priority laid down in the first chapter (where, not coincidentally, the best examples of reliabilist empirically defeasible a priori beliefs were found to be inadequate). But even the specifics of how we define a priority aside, there are more general reasons to think that a reliabilist account of the a priority of logic is far too naturalistically determined to really deserve the title a priori. This can be seen in two different ways.

In the first place, there is the reliance on reliability. How, exactly, are we supposed to determine if something is reliable except by reference to experience? For instance, Goldman essentially divides the task of conferring a priori warrants into two distinct parts: the warrant part, as given by judgments of reliability, and the a priori part, as given by the fact that the process in question is one which is non-experiential. Now the judgment that some particular process is reliable is purely experiential, as no one would deny. What I'm pointing out is that this reliance on experience in the ascertaining of the warrant seems to taint the warrant given in a way which precludes its being seen as non-experiential. But, it will be objected, the reliabilist need only say that the inference in question is reliable, not that we know that it is reliable. This being the case, we could be warranted, without our knowing it because it is in fact true that our inference rules are reliable. Thus, the experience can't be an essential part of the warrant after all, contrary to my above complaint. If that's the objection, then it seems to concede that experience does somehow taint the warrant in question. In this case, there is a serious problem. For now, we get the strange consequence that the warrant is legitimately non-experiential

[12] See Kitcher [2000] pp. 73-4

only until we have a demonstration of its actual reliability, at which point it becomes tainted by our empirical understanding. If the concession is not intended, then at the very least, I think the reliabilist has to say a lot more about why the experience does not taint the warrant in a way which makes it a questionable example of the a priori.

The second way that the reliabilist account might be thought too empirical is made clear by considering both Goldman's and Rey's contention that the warrant producing processes in question be part of a persons fundamental cognitive architecture. It is reasonable to ask, of these accounts, just how we are to imagine these processes and subsystems getting into our brains? A natural explanation of this phenomena would surely proceed along some sort of evolutionary lines, with the system being one which somehow enhances an individual's fitness for survival (surely getting true beliefs by inference from other true beliefs would be something which would enhance fitness). This sort of explanation is all well and good, but its now not clear that this process really is a priori, in the sense of being empirically indefeasible. Surely we can imagine that our world might have been some other possible world, where reasoning according to the methods of first order classical logic would not be fitness enhancing (say, totally chaotic worlds or evil demon worlds). If this is possible, then it seems likely that in a world where these processes and subsystems are not fitness enhancing, creatures with those processes and subsystems would not live long to breed and pass on their genes. In these worlds, creatures would be far better off developing different processes and subsystems.

The important point for our purposes, though, is this: In these non-classical worlds, it might be the case that the processes and subsystems in question are not reliable. If this is the case, they can no longer be considered as having an a priori warrant. So, it

seems hard not to conclude that the warrant in question is empirically defeasible, and so not a priori. This criticism, of course, depends on taking a particular conception of the a priori, and as I pointed out in Chapter 1, both Goldman and Rey are explicit that they think that a priori warrants should be empirically defeasible. Goldman and Rey would disagree that being empirically defeasible makes the warrant not a priori, and so they would not accept the last conclusion drawn above, that the warrant is empirically defeasible and so not a priori. For them, a warrant can be both empirically defeasible and a priori. I tried to argue against this conception in the first chapter, and I need not repeat my complaints.

## III) Non-Factualism About Logical Justification

As we have just seen, factualists argue that there is some natural or non-natural property which makes it the case that we are justified when we are. Let us now look at an alternative view, which doesn't hold justification to be a factual property at all. There are several different ways of being a non-factualist about a particular area of discourse. Since my sympathies lie with non-factualism, and since, it seems to me, it is generally less understood than factualist alternatives, I will spend some time developing it. In the first part of this section, I will examine Field's arguments to the effect that justification is non-factual. Non-factualists about logical justification believe that there is no property that adheres to some deductive methods but not others, in virtue of which we are right to use the former but not the latter.

Let's begin by laying out Field's views regarding the a priority of logic. Field is what he calls an "undogmatic a priorist": though he thinks that his reasons are very good reasons for not believing that logic could be a posteriori, he allows for the possibility of future developments which lead to the opposite conclusion. The sense of possibility here is what Field calls epistemic possibility. This is like the possibility in the idea that set theory might be inconsistent: we have very good reasons today for thinking that set theory is consistent, but must admit that future set theoretic developments could undermine those reasons. This sense of possibility is to be contrasted with genuine (i.e. mathematical or logical) possibility. Though it may be epistemically possible that set theory is inconsistent, it is not mathematically or logically possible that it is, since no one has any idea how to actually prove the inconsistency. In the same way, Field argues that he has very good reasons, today, for thinking that logic is a priori: according to Field, it is not coherent to be anything other than a priorist about logic. This is why it's not genuinely possible to revise logic for empirical reasons. But that does not preclude some future logical developments from opening up new possibilities, which is why its not epistemically impossible, either. Thus, though Field does fully believe that we ought to treat logic as though it is a priori, he also believes that he might possibly think otherwise someday.[13]

Field gives two main arguments for being a non-factualist about justification. One argument is that the alternatives are completely unpalatable. Field finds rationalism, he calls it "non-naturalism", utterly mysterious, and though he obviously harbors sympathies with the spirit of reliabilism, he calls it naturalistic reductionism, he

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[13] See Field [1998] pp. 4-6

argues that their program gets hopelessly bogged down when one starts to consider details. The other argument for his version of non-factualism, he calls it "evaluativism," is that it recasts the key issues in discussions of the a priority of logic in a way which makes them more tractable. In what follows, I will explore each of these points.

Let us first look at some of his arguments against the alternative views that we've spelled out above. In the first place there is what Field calls non-naturalism. This option gets pretty short attention from Field. As he sees it, this method essentially says, with respect to the justifiedness of logic, that certain deductive methods have the non-natural property of being reasonable, This property is, "...not explainable in terms of anything non-normative (hence, presumably undetectable by ordinary perceptual processes)."(Field [2000] p. 127) He calls it a "strange" sort of property, in that even if we assumed that some methods did have it, why would we think that the methods we use do, and what reasons could we possibly have for thinking either way? In another paper, he likens this sort of non-naturalistic property to a "mysterious justificatory fluid", and argues that the picture it presupposes is "totally perverse". Speaking of the a priority of inductive methods, Field says, "Advocates of different inductive methods are portrayed as holding different positions about which inductive premises squirt justificatory fluid to which conclusions...," and he concludes, "...why should we care about justificatory fluid? The only thing to care about is whether and how to reason inductively."(Field [1998] p. 7)[14]

[14] It does seem here that Field may be being a bit dismissive, as there may be less mysterious ways of cashing out rationalism's attractions than you would think with all the talk of justificatory fluid. George Bealer [1999], for instance, has argued that we should justify appeal to intuitions naturalistically by revealing how they are reliable producers of true beliefs. However, as I have already noted, I am not

What would make evidential systems that use classical logic more justified for the naturalistic reductionist like the reliabilists that we examined would be something like it's being a more reliable producer of true beliefs than, say, a system that uses intuitionist logic. I have already given one suggestion as to why this reliabilist move might be a bad idea in the case of a priori justification, namely that its not clear how a priori this more naturalized a priori really is. Field offers several different arguments to the effect that reliabilism as a story of justification is unsatisfying.[15] Here, I will point out only Field's very general difficulties with the motivation for reliabilism. He argues that reliability alone is not what we value, or should value. Some very reliable methods which he notes would be a bad idea to adopt: believe only logical truths, or, believe nothing.[16] So, at the very least, reliability will need to be balanced against other goals that we have, such as wanting a theory that is strong, or fruitful, or one compatible with our real-world limitations. Indeed, reliability itself is not so straightforward: do we want reliability in the short or the long run, and what do we mean by long or short? It seems pretty unlikely that the setting and the balancing of these goals will be achievable without bringing in some sort of evaluations on our parts. For instance, we'll have to evaluate when we weight our goals, and also when we assess how well a particular theory actually achieves a given goal. Considerations like these make it seem highly unlikely that questions of justification are going to be settled easily by appeal to natural facts.[17]

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particularly interested in factualism of this sort because it doesn't seem to provide a very good answer to the sort of question that I mean to be asking.

[15] See Field [2000] pp. 124-7, and pp. 136-7; see also [1998] pp. 7-11

[16] See Field [2000] p. 125

[17] See Field [1998] pp. 9-10

Though it is clear that we do value having true beliefs over false ones, Field argues that there is no reason to think this interest is what constitutes the reasonableness of the beliefs in question.[18] We might well have an interest in knowing that certain methods are very reliable producers of truth, but only to the extent that we think it good to have reliable beliefs. There is no further need to see our interest in the reliability of our beliefs as what somehow makes the beliefs in question reasonable ones to have. The upshot of Field's discussion is this: though there is clearly something right about the idea that we want reliable beliefs, it is something much more mundane than what reliabilists think it to be. We want reliable beliefs, no doubt, because we are much more successful creatures when the majority of our beliefs are reliable guides to the world around us. But this does not mean that we can reduce the question of what it is reasonable to believe down to a question of reliability.

We have just seen why Field thinks that factualism about justification is a bad idea. Still, why should we think that non-factualism about justification is a good idea? Field argues that adopting a non-factualist perspective about justification offers new perspectives on two particular questions: that of skepticism, and that of a priori justification. To see what these new perspectives are we should say a little more about what Field's theory is. According to Field's theory, evaluativism, while it is nonsense to try to reduce questions of justification to questions of fact, it is equally nonsense to try to explain them by anything rational or generally "non-natural". Rather, questions of justification are evaluative: they have to do with the desirability of our using one particular method over another particular method. For Field, evaluative issues come in at

[18] Field [2000] p. 25

many levels: at the level of choosing goals, at the level of assessing those goals, at the level of prioritizing those goals, to name just a few.[19] Justification, when seen in evaluativist lights, will consist a lot more in saying why one's preferred theory is better than the alternatives. The emphasis will become far more "practical"[Field 1998, p. 12], more an issue of whether or not something is "good policy"[Field, 1996, p. 377] The question according to Field goes from, what makes modus ponens justified?, to why should I use modus ponens?

With respect to the a priority of logic, this new emphasis has very clear results. When the issue of the a priority of logic is framed in factualist terms it seems like the central problem is one of specifying how we can be justified using some rule, say modus ponens, given that we don't have any evidence for it (see the end of my discussion of non-naturalism, above). But when looked at non-factually, it is clear that what we want to know is whether or not it is a good idea for us to treat certain rules as though they were empirically indefeasible.[20] The factualist offers a rather "mysterious" way of conceiving the problem, which makes us look for some positive thing that we could definitively say attaches to one evidential system but not another. The non-factualist perspective on the problem invites clearer answers by making us focus on which evidential system seems best for us to adopt. This is an important shift because, while it is true that there are considerable problems with justifying logic (as we will see in Chapter 5), the answer to

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[19] At the level of idealizing those goals. Field's evidential system picture is about ideal evidential systems. In order to assess what evidential system a person is using, it is likely that the assessor will have to idealize in a number or ways. For instance, people make mistakes in reasoning, so we want to know how they reason when they aren't mistaken. These idealizations, too, are places where indeterminacy can sneak in: it can be genuinely indeterminate which evidential system a person is using. (Field [1996] p. 363)

[20] See Field [2000] p. 128; See also [1996] p. 378-9

why we should treat modus ponens as empirically indefeasible is fairly clear cut, according to Field. As we have seen (and will examine very shortly in more detail), Field's main argument is what makes this latter question more clear cut: we should treat modus ponens as empirically indefeasible because treating it as empirically defeasible makes no sense. The shift to a non-factualist perspective illuminates Field's strategy regarding the a priority of logic very nicely, and easily explains why the best reason for being a priorist about logic is the lack of any reasonably good alternatives.

Another issue that non-factualism seems to illuminate is that of skepticism. Field argues in several places that when the issue of justification is framed in factualist terms, skepticism seems a very reasonable alternative. The skeptic, we are supposing, advocates abstention from belief because nothing is justifiable. When what we are looking for is some mysterious property which some evidential methods have and others lack, then the view that there is no such property, and that, therefore, nothing is justified, becomes quite attractive. But if the discussion concerns whether or not we ought to treat certain rules and principles as though they were justified, skepticism starts to look a lot less appealing. It seems pretty silly to argue that it is better to abstain from having logical beliefs at all. Clearly, if we abstained from having any logical beliefs, and thereby did not take ourselves to be allowed to infer according to particular rules and principles, we would be much worse off than if we were allowed to use said rules and principles even lacking any justification. Reasoning according to some rules, even unjustified ones, has got to be better than just not reasoning.[21]

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[21] See Field [2000] p. 127, and 1996, p. 379. According to Field, what we want to explain is the reasonableness of following some particular rule or method, and the factualist offers four options: 1) Naturalistic Reductionism, which defines justification as a matter of how good a rule is at producing truth

There is a further issue that non-factualism helps bring to light, and this one will be of great importance for me in the Chapter 4. One thing that follows if evaluativism is correct, as Field notes, is that there may be a considerable amount of indeterminacy regarding questions of which evidential system is best for us to use. It seems quite likely, given all the ways in which evaluations are crucial to the assessment of whether or not some system or method is best for us to use, that there will be cases where its just not determinate whether one system or method is better than another. To take a very simple example, suppose that we can agree exactly on our goals, call them A, B, C and D, and their relative weights of importance. Even given this highly unlikely scenario, we still might disagree greatly about which method is best at achieving them. You might prefer method X, which does a fantastic job at achieving A and C, but not so good at achieving B and D, whereas I might prefer method Y, which is great at achieving D, but not good at B, and perfectly adequate for A and C. Under circumstances like these, it will just be indeterminate which method we really ought to adopt. "...(I)f at a given time there are two available methods, neither definitely better than the other at meeting the goal or tradeoff of goals (that is, the methods are either incomparable or tied), then there is no

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and avoiding falsehood (Goldman and Rey); 2) Non-Naturalism, which defines reasonableness as a primitive property, not explainable in non-normative terms; 3) Egocentrism, which defines reasonableness by proximity to our rules, our rules being, by stipulation, the best ones; and 4) Relativism, or skepticism, which says that there is no rational reason to prefer one set of rules over another. Since he thinks that option (2) is just too mysterious a property to be of much use, and since (3) seems to sort of evade the question at hand, providing definition instead of explanation, the best candidate would seem to be (1). However, as we have seen, Field finds reliabilism unattractive for several reasons. This leaves (4) as the most reasonable option, unless one is a non-factualist. The skeptic above is skeptical of our ability to meet the demand that justification be constituted by some natural or non-natural property. But by denying the need for that demand, Field reframes the problem of justifying some rule of inference as that of being clear on why we should follow or value this rule. The skeptical response to this—we shouldn't follow or value this rule—is a lot less concerning, because it would have us not following or valuing any rules, and this is just absurd.

factual question as to whether the beliefs licensed by one but not the other are justifiable."(Field [1998] p. 9)

In this section, I have tried to explain Field's non-factualism regarding logical justification. I have shown both why Field thinks that non-factualism is more satisfactory than the alternatives, and how Field thinks that a non-factualist perspective helps to clarify certain issues in epistemology, specifically, the a priority of logic and skepticism. Field is also clear that non-factualism entails that some of our judgments regarding justification will be indeterminate, a point that I emphasized will be important in the next chapter.

## Conclusion

In this chapter, I have surveyed three different metaphysical positions regarding epistemic justification. I have explored examples of all three, and have raised questions about two of the views: conceptualism and reliabilism. My worry with respect to the former, is that it is ill-equipped to address the question of revising logic, and with respect to the latter, it is that it is not a good example of a priority. The third view, nonfactualism, is my preferred view. I have explained several arguments for this view as given by Field. In Chapter 4, we will see another good reason for empiricists to be nonfactualists.

## Chapter 3: Empirical Reasons

In Chapter 1, we saw that for logic to be a priori, it must be impossible for the rules and principles of logic to be revised for empirical reasons. The question, of course, that immediately wants asking is, what does it mean to be revisable for empirical reasons? As it turns out, answering this question is not so straightforward, and my argument for why logic is not justified a priori will turn on a particular conception of what it means for something to be revisable for empirical reasons. More specifically, as we will see, Field argues very persuasively that logic is a priori as long as we restrict our understanding of 'revisable for empirical reasons' to something like, revisable according to some one of our best idealized systems of evidence assessment. My argument to the contrary will be that Field's notion of 'empirical reasons' here is unnecessarily restrictive, and that a broader notion of what does and does not count as an empirical reason is more in keeping with at least some of our inferential practices in science. When empirical reasons are conceived in this latter way, I will show that logic, too, should be seen as revisable for empirical reasons, and so, not a priori.

In the first section of this chapter, I examine Field's argument for the a priority of logic. In section 2, I return to my discussion of what we should mean by 'logic' in the

first chapter. I will clearly describe one pragmatic rule and show how it might help us to revise a principle of deductive logic, in one particular example. In the third section, I will explain exactly why the revision described in the second section should count as empirical. I will argue that there are two different strengths of empiricism. A belief or warrant is strongly empirical if it is revisable for the sort of strictly evidential reasons required for Field's argument, and it is what I will call modestly empirical if it is revisable for more purely pragmatic reasons. I argue that though Field is right that logic is not strongly empirical, it is nonetheless modestly empirical, thus showing that logic is not wholly a priori. In addition, I will defend my view against several kinds of objections.

## I) The A Priority of Logic

I will begin by looking at Field's argument and getting a better grip on what Field means by 'revisable for empirical reasons.' Field has challenged the claims of radical empiricists by arguing that both deductive and inductive logic are a priori, though they are a priori for very different sorts of reasons. Let us start by looking at what Field has to say about the problem with revising our most basic inductive methods. Field argues that we must have certain most basic evidential rules and methods, for example, (R): "If after t you have observed $s_t$ ravens, and $r_t$ of them have been black, you should believe to degree $(r_t + b_t)/(s_t + c_t)$ of any raven not yet observed that it is black, where $b_t$ and $c_t$ are parameters representing the current bias, which changes over time."(Field [2000] pp. 144-4) One could think of the sequence of these rules, over time, as being

governed by something like the meta-rule (R*): "Act in accordance with ($R_t$), where the parameters $b_t$ and $c_t$ are obtained from earlier parameters by such and such updating process."(ibid.) The problem with revising these for empirical reasons is that they seem to account for their own revisions in advance, since part of the very rule says to take evidence into consideration: factoring in new evidence is part of the very rule itself. Given this, it's hard to see how evidence could ever cause us to overturn the rule, and so it is hard to see how we could ever come to revise our most basic inductive rules for empirical reasons.

According to Field, this does not preclude our coming to revise these rules at all, and we might even revise them as a result of certain observations that we make. For instance, we might, from a series of bad experiences, start giving more weight to the goal of avoiding falsehoods and less to the goal of achieving truth. In this case, according to Field, we have a shift in goals, not a case of there being evidence against some rule. Another "more important" example: observations lead us to think up new rules which seem to have more intrinsic appeal. Again, Field says, "Here, too, it is transparent that the shift of rules is not due primarily to evidence against the old rules."(Field [2000] p. 146) What seems ruled out, rather, is that we would change one of our most basic rules because we had what we regarded as empirical evidence against it. As Field goes on, "Of course, on the basis of the new rules we might find that there is evidence against the old. But if the old rules didn't agree that it was evidence against them...then the decision to count the alleged evidence as evidence depends on an independent shift in the rule."(ibid.)

In addition to basic inductive rules, such as the ones that Field specifies, I noted above that evidential systems will also have most basic deductive rules, and that Field defends the a priority of these as well. Can we make sense of revising our most basic deductive rules for empirical reasons? Field claims we can't, but notice that it must be for very different sorts of reasons than those that came to play in the case of our most basic inductive rules. The quality, as it were, that makes difficulty for revising our basic inductive rules for empirical reasons seems clearly to be its self-correcting nature. In particular, it is because the best candidates for our most basic inductive rules are almost certainly going to be rules which can take empirical evidence into account (who would want to use an inductive rule that didn't?) that these rules seem like they must be a priori. Basic inductive rules tell us what we should believe no matter what the evidence, so they cannot be overturned by evidence, on pain of giving inconsistent advice. Deductive rules, both our most basic and our non-basic ones, are not self-correcting like this. They do not have built in capabilities for assessing evidence, be it empirical or not.

This raises the question of why we should think these rules should be regarded as a priori. The argument for this is to be found in a different paper:

> ...the case in favor of standards that treat logic as a priori depends almost entirely on the fact that it seems impossible to come up with any minimally decent standards that treat it otherwise....Every serious attempt to formalize our inductive methods that I am aware of builds in a substantial logic (typically classical logic) in a way that seems to guarantee treating that logic as a priori. For instance, consider inductive methods of a basically hypothetico-deductivist sort: they say that how well a theory is confirmed depends on its deductive consequences in some definite logic, and on the deductive consequences in that logic of its main competitors...It is hard to see how the logic employed could itself be subject to empirical confirmation on this approach: ...it seems pretty clear that the logic employed to derive the consequences of theories is bound to

> favor theories based on that logic over theories based on competing logics. (Field [1998] p. 12)[1]

The key to Field's argument lies in the idea that the logic we employ is "bound to favor" itself. Field's elaborates this claim in what immediately follows, when he considers probabilistic evidential methods "of a roughly Bayesian sort". He says, "...it is built into standard Bayesian methods that all classical logical truths have probability 1, and that if a set of premises classically entail a conclusion then the probability of the conclusion is at least as great as the probability of some conjunction of the premises. This precludes use of standard Bayesian methods to revise classical logic."(ibid.) What makes basic deductive methods unrevisable for empirical reasons, then, is the fact that our best evidential systems, like Bayesianism, build in an assumption of the unrevisability of those deductive methods. In the Bayesian case, the assumption is found in the fact that logical truths all get probability 1, and that conclusions must have probabilities at least as high as the premises that imply them. These assumptions assure that no Bayesian method could conclude that classical logic have a probability of less than 1. A Bayesian argument to revise logic for empirical reasons would be incoherent because, in essence, it would advise us to reason in a way which is inconsistent with being a Bayesian.

What about hypothetico-deductive evidential systems? These are systems that are supposed to establish hypotheses by deducing observable consequences from them, and then testing for those consequences. Simplifying greatly, according to the hypothetico-deductive method, we can test a hypothesis H by combining it with a number of other auxiliary assumptions to yield an empirical prediction. These auxiliary

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[1] See also Field [1996] for related arguments.

assumptions are numerous and multifarous, ranging from assumptions concerning the correct rules and principles of logic and assumptions about the actual mechanics of the test, to assumptions, both highly theoretical and otherwise, from other scientific theories. When we perform the test, if the predicted result is forthcoming, then we consider the hypothesis at least tentatively confirmed, and if the predicted result is not forthcoming, then we consider the hypothesis tentatively disconfirmed.

Hypothetico-deductive evidential systems don't carry the explicit requirement that logical truths get assigned a probability of 1, so why should we think that this method makes logic a priori? The reason, in general, is that some deductive rules are necessary in order to make the predictions that ultimately assess the hypotheses in question. Whatever these rules are, these must be regarded as necessary, at least relative to the hypothesis being tested. To see this, consider Quine's famous appeal to the metaphor of Neurath's boat in his defense of his claim that any belief, even a belief about logic, is revisable. The thought here was supposed to be that one could test, as a hypothesis, an actual rule or principle of logic, by deriving some prediction from the rule, holding other auxiliary assumptions fixed (including other rules and principles of logic), and see if the hypothesized prediction was forthcoming. For example, to borrow from Putnam's argument for quantum logic (which I will consider in more detail later), we could test the distributive rules by the two-hole experiment, and find that one direction of distributivity seems to fail for quantum phenomena. Each rule of logic, the thought went, would, in principle, be testable in this way (though of course it might be a lot more difficult to come up with a good prediction which could test some of the other rules and

principles of logic), and so no rule of logic would be beyond revision for empirical reasons.[2]

There are a number of difficulties with this whole picture, but for our purposes, we can focus on just two problems. The first is that it doesn't actually show that logic is not a priori in Field's minimal sense of a priority.[3] For though we might subject one logical principle to direct test, we are certainly going to need to hold other rules and principles fixed to actually get the desired prediction, and these rules and principles would themselves be a priori by Field's standard. They would, after all, be necessary for the actual deriving of consequences which allow us to evaluate the hypothesis in question either negatively or positively. So, this suggestion does not go far enough: it may show how an individual rule of logic is revisable at some point for empirical reasons, but it will not show that logic is not a priori in Field's minimal sense of being necessary for the evaluation. The second problem is related: even if the distributive rules are revisable for empirical reasons in this way, the empiricist needs to show that every rule is revisable for empirical reasons. After all, if the rules which derive the consequences from the distributive rule plus quantum theory are not themselves revisable in this way, then some rules and principles must be a priori after all.

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[2] This defense was first most clearly defended in Adjukiewicz [1949].

[3] Field himself notes that his notion of a priority is not one that many have defended. It is, in fact, somewhat akin to what Friedman [1999] has called the relativized a priori of Reichenbach and Carnap: beliefs which are warranted a priori are simply those beliefs that function in their epistemological contexts as necessary for thought regarding some particular area or other. I will more fully consider the question of how the conclusions of the current work reflect on the historical differences between Quine and Carnap in the conclusion.

Bayesian and hypothetico-deductive theories of confirmation and evidence assessment are, of course, deeply troubled and many have questioned their plausibility.[4] But they have yet to be replaced by anything that seems remotely more plausible, and Field's bet seems to be that any replacement for them will similarly treat the rules and principles of logic as sacrosanct. After all, in order to be a system of evidence assessment, there must be some logical rules or principles which provide the rules of assessability. It's hard to see how we could even have an evidential system which didn't supply some deductive and/or inductive rules for evaluating evidence. Field's claim is that logic is necessary for evaluating evidence, and so it functions, in evidence evaluating contexts, in such a way that it cannot be defeated by any evidence. The general idea here is, I think, this: deductive rules and principles form the basis for any (decent) system of evaluating evidence. As such, they are shielded from empirical risk. They cannot be refuted by empirical evidence because one needs logic for there to be any refuting going on at all. This being the case, Field concludes that logic cannot be revised for empirical reasons, and is, therefore, a priori.

## II) Pragmatic Rules and Quantum Logic

Field's argument is that logic is a priori in the sense that it is not revisable according to our best evidential methods. In this, I think Field is correct, and will not dispute his conclusions. Nevertheless, I also agree with Quine when he says, "...no

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[4] See, for instance, Glymour [1980] chapter 2 for a good overview of problems and attempted responses to the problems.

statement is immune from 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] p. 43) That is, I still think logic is empirical in some sense of the term. Making clear exactly how it might still be empirical, in spite of Field's findings, is the burden of the remainder of this chapter.

Two aspects of Quine's point are important in my defense of the empiricality of logic. The first is what I will call Quine's plea for parity. Here, what I have in mind is the fact that empiricism about logic need only show that logic is just like other aspects of what usually gets deemed scientific innovation. Granted that Field has shown that there is a sense in which logic is different than other scientific pursuits, as long as the empiricist can show that there is still a sense in which science is continuous with logic, then logic might still be empirical in that sense. In the next section, I will make clear exactly what this plea amount to, and explain and defend the sense of 'empirical' which must be at work in this claim.

The other important aspect of my defense will make great use of Quine's appeal, above, to simplicity. That is, Quine appeals to pragmatics in his assertion of the possibility of revising logic. According to Quine, we use a number of informal principles in trying to decide how to revise our total belief set when a tension in our theories becomes apparent, such as when a prediction fails to be realized. These principles include simplicity, familiarity of principle (conservatism), scope (generality), fecundity,

and ability to account for testable consequences (refutability).[5] Overall, the idea is supposed to be, we should prefer belief sets which embody theories which are simpler, have greater explanatory power, and cohere with well-established empirical evidence, in addition to being relatively conservative revisions or extensions of our former belief sets.

In chapter 1, I noted that Field seems amenable to the idea that certain pragmatic rules might also be necessary for the proper functioning of our evidential systems. For example, simplicity is thought to be desirable inductively, as when we prefer regular color predicates to Goodmanian temporally indexed predicates like 'grue.' [6] In addition, pragmatic rules are also supposed to play a big role when we reason in inference to the best explanation: it is simpler, overall, to suppose that when 2 students hand in identical papers, they both used the same source, rather than to suppose that they both somehow managed to write identical papers on their own.

But in addition to this role in our evidential systems, pragmatic rules have a (perhaps unique) role to play in helping us to revise and adjust our total belief sets when the evidence doesn't clearly indicate one way or the other. So, consider that favored example of empiricists, quantum logic. The problem here is supposed to be that one can derive a prediction concerning the two-hole experiment from quantum physics using classical logic which does not pan out. Quine's point is that, facing a contradiction like

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[5] See Quine, [1955],. See also Quine and Ullian [1978] (where he adds Modesty, or logical weakness); Quine [1960(b)], pp. 17-21; and Quine [1963] (242-245).

[6] For instance, Field's example of an inductive rule is one which concerns ravens, but why should we think that we are concerned about the percentage of black ravens as opposed to canavens, where canavens are black ravens until the year 2004, and yellow canaries thereafter. In other words, inductive rules like the one Field considers are subject to Goodmanian type grue considerations, and many have thought that pragmatics are relevant to our ruling out grue-like predicates.

this, we can, in principle, revise any one of our beliefs, and we should be guided by the pragmatic factors that I've indicated above. In this section, I will develop this example and show exactly how it might have been simpler to revise logic in light of quantum mechanical anomalies.

To do this, I will need to be very clear about the criterion of simplicity that I'm appealing to. So much has been written on this principle that it would be impossible for me, in the current context, to do anything like an overview.[7] My focus will be on one of particular rule, put forward by Gilbert Harman. Harman endorses what he calls the simple measure of change in view: "Take the sum of the number of (explicit) new beliefs added plus the number of (explicit) old beliefs given up."(Harman [1986] p. 59) I choose Harman's rule because I have to choose one to make the point I want to make, and it seems particularly well suited in the case that I examine, but there are probably others that would do the job. At any rate, my current goal is not to explore which simplicity measure really is best. Rather, it is to show how it might be simpler for us to revise logic when there is a tension in our physical theories.

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[7] Anyone familiar with the literature on simplicity knows that it is vast and multifarious. In the first place, there are many ways that simplicity considerations have been thought to figure into our theorizing. Simplicity considerations guide many different sorts of our nondeductive inferences. There are general considerations of parsimony, such as those advised by Ockham's razor, which ask us to postulate as few entities as possible to explain the phenomena in question, or those that Hume unsuccessfully tried to justify, that nature is simple. As I have already noted, simplicity also plays a role in inference to the best explanation and other of our evidential endeavors. And there are particular rules of simplicity, designed to be all-purpose guides to belief revision and theory and hypothesis choice. To make my point, I must pick one of these, but there are a large number of suggested rules for how simplicity should play a role in belief formation and revision. I don't mean in any way to suggest that a lot more couldn't be said about the relative merits of each of these rules.( See Hesse [1967] and Harman [1994]) This, however, is not the place to say any of it.

Let me be very clear about what I will and will not be endorsing in Harman. In particular, let me explicitly note that I do not agree with his radical rejection of logic in reasoning.[8] I agree with Field that idealized evidential systems have a crucially important role to play in our thinking about the world. This seems, to me, to be eminently defensible; physicists, after all, idealize about the physical world, and it seems reasonable to think of formal deductive and inductive systems as idealizations of actual cognitive mechanisms.[9] In thinking this, I am disagreeing with Harman: there is some role to be played by deductive and inductive logic, characterized by formal rules and principles for the generation and revision of beliefs.

Nevertheless, it seems undeniable to me that at least some of what goes on in science does not proceed along such neat lines. That is, I agree with Harman that at least some of our thinking about our world can not be so neatly encapsulated by idealized evidential systems. I will argue that pragmatic rules have an important role to play in our theoretical reasoning about the world when that reasoning goes beyond the evidence, and I will use Harman's rule as a way of codifying and making clearer the sort of rules and principles involved in our revising according to the pragmatic method. My aim is to show that this role of pragmatics is legitimate and defensible as part of a broadly empiricist methodology.

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[8] Harman [1986] is famous for arguing that logical rules and principles are not relevant to belief revision, what he calls "change in view." Harman argues that deductive reasoning does not and cannot represent reasoned change in view. In addition, Harman [1973] also holds that inductive reasoning is just another species of inference to the best explanation. He argues that there is no such thing as inductive logic, though he acknowledges that there is probably inductive reasoning (i.e., reasoned change in view). Harman's position regarding the role of logic in reasoning is controversial, to say the least.

[9] This line of thought has been argued, for instance, in Ellis [1979]

With our simplicity criterion in hand, let's return to the quantum logic example, and consider it in more detail. This example has the advantage of having actual adherents, living and historical, which makes it plausible as a story about how a logical theory ever might be overturned by empirical evidence. Nevertheless, it also has its disadvantages, as quantum logic, itself, seems very problematic (and unnecessarily complicated, for the purposes to which I really want to put it). Let me be clear, then, that I am not advocating that we adopt quantum logic.[10] Rather, I am simply appealing to it as my illustration of why revising logic for pragmatic reasons should be seen as revising it for modestly empirical reasons.

For simplicity's sake, I will make a few assumptions. First that it is clear that we do use classical logic now (and clear what that means). Second, that the only other viable logic we might use is quantum logic (where by 'quantum logic' I intend to be singling out logics in which the distributive rules are restricted). The first, I think, is a generally accepted by many, though there are those who challenge it.[11] The second assumption, though is especially dubious. Just limiting our scope to quantum mechanical evidence, it has been suggested that we should be intuitionists, that we should give up bivalence, and that we need some sort of non-classical account of conditional probability.[12] So clearly

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[10] For one, it is doubtful that quantum logic really could do all the work it was supposed to have done. For instance, Gibbard argues that it doesn't even solve the two-hole experiment, properly conceived, and Stachel has argued that it completely fails to explain the phenomena in question, which is what Putnam claimed it could do.

[11] See Priest, Graham MS

[12] See Newton-Smith [1982] for the suggestion that we become intuitionists, Reichenbach [1948] that we should abandon bivalence, and Gibbins, [1987] pp. 150-1 for the suggestion that it is the classical understanding of conditional probability that must be jettisoned. In the Quine quote which opened this section, he seems to be referring to Reichenbach's suggestion, rather than the one that I focus on.

the idea that the choice we face is between classical logic and some form of quantum logic is very simplistic.

I will focus, in what immediately follows, on the writings of Putnam.[13] Though many have argued that quantum mechanics shows the need to revise certain of our logical rules, Putnam is the one who most heartily endorsed the conclusion that logic is just another empirical theory.[14] Putnam is quite clear that there is a way things really are, logically speaking. He claims, "...physical laws have to be compatible with logic—that is to say, they have to be compatible with the *true* logic, which is quantum logic," (Putnam, [1974] p. 61). He argues that the case of logic is "exactly parallel" to the case of geometry: just as geometry was thought to be a priori until non-standard geometries and finally Einstein showed it to be otherwise, so too logic will come to be seen in the proper (e.g., non a priori) light once the challenge posed by non-standard logics, and their applicability in science, becomes clearer.[15]

Simplifying again, the evidence that we should revise logic is supposed to come (mainly) from the famous two-hole experiments in physics. Borrowing heavily from Fine's [1972] exposition of the problem: in these experiments, there is a filament which

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[13] Putnam, Hilary [1974, 1968]

[14] Putnam has been influenced, I should say, by the writings of David Finkelstein, who is equally vociferous in his advocacy of the empirical nature of logic. (See Finkelstein 1964) According to these philosophers, we can give an operationist account of the logical connectives. (See Putnam 1968, section 8) This could be seen as another instance of what I called naturalistic reductionism in the last chapter.

[15] See Putnam [1974]. Both the comparison and what it is alleged to show have been challenged. The comparison itself is challenged by Field, as we will see in Chapter 5. But what the comparison is supposed to show has been objected to by, among others, Rey [1998] and Katz [1998], who both counter that geometry has not been shown to not be a priori. Essentially, these philosophers argue that though geometry itself, pure geometry, is still a priori, which geometry best describes the actual world has always been an empirical question. Both of these philosophers views, I think, depend on a more factualist conception of justification.

spews electrons towards a plate. There are two holes on the plate, A and B, equidistant from the electron source, and behind the plate is a screen which is sensitized to record the places where the electrons hit when they get through the plate via hole A or B. When A and B are both open, there will be an interference pattern on the screen: there are peaks right behind each of the open holes, with troughs to either side of these large peaks, and then somewhat smaller peaks to either side of each trough, which troughs again, and so on. This is what we'd expect to find if particles behaved more like a wave. If we close the B hole, the pattern on the detector screen is what Fine calls an A-pattern. When we close hole A, the screen displays a B pattern. Superimpose the A-pattern on the B-pattern, and the pattern will be additive: there are a large number of hits right behind each of A and B, and the hits sharply taper off to either side of this peak. This is just what we'd expect to find if particles behaved more like atoms. The problem, at its heart, is that the two patterns are very different from each other. Each pattern is simply a representation of a series of electrons going through either an A hole or a B hole and registering on a sensitized plate. It's hard to see why the fact that in one series, both holes remained open the whole time, while in the other, one was always closed, should make a difference to the way they travel through the holes in the plate.[16]

As Putnam formulates it, the problem is that when both holes are open, the probability of an electron hitting a particular region, r, is not what we would expect given the classical understanding of probability, which Putnam seems content to leave

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[16] See Fine [1972] p. 4

unrevised.[17] In particular, we think it should be one half of the sum of the probability of its hitting the region r when it goes through A, and its hitting r when it goes through B. By the definition of conditional probability, one gets that

1) $P(r \mid h_1 v h_2) = P[r \& (h_1 v h_2)] / P(h_1 v h_2)$.

The distributive law allows us to expand the right hand side to

2) $P[r \& (h_1 v h_2)] = P[(h_1 \& r) v (h_2 \& r)] / P(h_1 v h_2)$

$= P(h_1 \& r) / P(h_1 v h_2) + P(h_2 \& r) / P(h_1 v h_2)$.

Since there is no reason for the particle to favor either slit over the other, we have that

3) $P(h_1 v h_2) = 2 P(h_1) = 2 P(h_2)$.

Therefore, the expanded right hand side above becomes

4) $P(h_1 \& r) / P(h_1 v h_2) = P(h_1 \& r) / 2 P(h_1) = P(r \mid h_1) / 2$

And

5) $P(h_2 \& r) / P(h_1 v h_2) = P(h_2 \& r) / 2 P(h_2) = P(r \mid h_2) / 2$

This, finally, allows us to infer

6) $P(r \mid h_1 v h_2) = 1/2 [P(r \mid h_1) + P(r \mid h_2)]$.[18]

This, as I noted above, is not what we find actually occurring in the two-hole experiment.

The situation, greatly simplified, is that there seems to be an tension in our physical theories: if we reason classically, then we are led to predict that the probability

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17 As I noted above (see footnote 12), this is one of the many ways that people have suggested accounting for quantum mechanic's anomolous results without revising our most fundamental logic. In particular, Gibbins argues that the real solution is to see that one can't formulate the problems of quantum mechanics in terms of probability. For any complementary properties, p and q, the conditional probability of (p&q) will be 0, since the probability of (p&q) is a quantum mechanical contradiction.(see Gibbins [1987] p. 151)

18 Putnam [1968] pp. 180-1

of an electron arriving at some region on the detector screen in the two-hole experiment should be one half of what it is in the one hole experiment, and that is not what we find. The solution to this problem is for us to consider the various possible revisions which make sense of the evidence, and pick the one which best explains it in a way which gets rid of the tensions (where by 'best explains' I mean something like, is simplest, most fruitful, and the like[19]). What are the various possible revisions? In what follows, I want to focus on the possible options that Putnam seems to have in mind.

Before I examine these options, it is important to note that the two hole experiment is but one manifestation of a more general puzzling feature of quantum mechanics: so-called particle-wave duality. Elementary particles seem to behave much like particles in many ways. For instance, in the two-hole experiment with only one hole open the pattern on the detector screen is additive, which best fits a particle view of the particles. Nevertheless, they also behave sometimes like waves, as in the two hole experiment with two holes open. Indeed, the quantum mechanical treatment of particles in a system depends entirely on the fact that the state of a system is represented by wave functions. Not, of course, waves in physical space, but mathematical waves, which represents, for instance, the statistical probabilities of a particles having some particular value at some particular time. Exactly how we should understand these waves and their relation to elementary particles is, perhaps, the single biggest interpretive question in quantum mechanics. If we treat these waves merely as probability waves, we run into the

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[19] I do not mean here to be precluding that explanation might consist in something other than simplicity and fruitfulness. Explanatory goodness is usually itself considered a pragmatic criteria of theory evaluation, and an incredibly difficult one to understand at that. Because of this, I have chosen to focus on simplicity. But it may well be that explanation stands alone, and is not easily reducible to the other pragmatic criteria mentioned in the last section.

problem that the particles do seem to behave like waves, as in the two-hole experiment. However, treating them as something like real waves has huge difficulties of its own, as when we actually make a measurement, our measurement is of a particle at a place and a time (the so-called reduction of the wave packet). For Putnam, a satisfactory explanation of the two-hole experiment will have to explain not only what goes wrong in the two-hole experiment as he conceives it, but will also have to adequately interpret quantum mechanics so as to make the above difficulties less puzzling.

The two interpretations of quantum mechanics that Putnam explicitly considers as rivals to his quantum logical solution are those that appeal to hidden variables to explain anomalous phenomena like the two-hole phenomena, and the Copenhagen Interpretation of quantum mechanics. According to the former, the paradoxes that arise are a result of our ignorance of the full nature of the phenomena we're dealing with. That is, it is well known that in quantum mechanics, one cannot simultaneously measure both the position and the momentum of a particle. According to hidden variable theorists, elementary particles do have both position and momentum at every instant, its just that we don't know both at the same time. Hidden variable theorists treat wave functions as probability waves, but they don't offer a very satisfying explanation of the two-hole experiment. They argue that our measurements disturb the particles they are measuring, and cling stubbornly to the idea that when we take some measurement, the measurement we get represents something very close to the state of the particle right before it was measured. With respect to the two-hole experiment, the hidden variable theorist has to say, "...one of the two measurements—the measurement that takes place when a speck appears on the emulsion, showing that a particle has hit, or the measurement that takes place when a

gadget is introduced at the slits to determine which particles go through which slit—must disturb the particle."(Putnam [1965], pp. 139-40)

The second interpretation is the so-called Copenhagen Interpretation. This interpretation seems to be the one that is most accepted by physicists themselves. According to this interpretation, observables like position and momentum only exist when a measurement is being made. Thus, in the two hole experiment, the particle does in some sense go through both holes when they are both open (but there is no detector at either hole) because it is in a state that is a superposition of many statistically probable states. This would eliminate the offending prediction and ensuing contradiction by making the probabilities for the individual events, going through an A hole or going through a B hole, undefined in the two hole experiment. In terms of Putnam's formulation of the problem above, it would leave the probability of the electron's going through $h_1$ and hitting r undefined, and so too for $h_2$, and so would leave the equations in lines (2), (4), and (5), undefined. It is only when it gets measured (i.e., hits the screen), that it collapses into one state and makes a definite measurement. But because the particle when both holes are open is actually a wave of statistically probable positions, repeated experiments give rise to the wave (i.e., interference) pattern.

Putnam is not satisfied with either of these solutions. Putnam argues that hidden variable theorists are, "...required to postulate strange laws whereby each measurement somehow disturbs the very thing it is measuring—e.g. letting a particle collide with a plate produces a speck on the plate, but at a place where the particle would not have been but for the presence of the plate."(Putnam [1965] p. 139)) According to Putnam, the hidden variable theorist does not really offer a solution to the two-hole experiment at all.

This is because of his commitment to the idea that though our measurements may disturb the particles in some way, they are still measurements which could accurately give a picture of the state of quantum mechanical systems when they are unobserved. Even in the best of these hidden variable theories, that due to David Bohm, Putnam argues that, "...an unknown physical force (the 'quantum potential') obeying strange laws is introduced to account for the disturbance by the measurement."(Putnam [1965] p. 140) Clearly, Putnam doesn't think this is a very desirable option.

The Copenhagen Interpretation does not fare much better in Putnam's opinion. Even though most physicists seem to accept this interpretation, Putnam is unhappy with it because he thinks it is hopelessly ad hoc, and unexplanatory. In the first place, he notes, "...superposition of states is not explained at all, but simply assumed as primitive."(Putnam [1965] p. 146) That is, rather than explaining how a particle could actually be a wave of statistically probable states, the Copenhagen Interpretation just asserts that it is. But this is precisely what is so mysterious about quantum mechanics, and so assuming it as a given isn't very satisfactory. The unsatisfactoriness of taking superpositions as givens is made abundantly clear by Putnam's second objection, namely that there are big problems with explaining why superpositions hold only in the micro, and not the macro world. This problem is most intuitively exemplified by Schrodinger's famous cat, who seems to be both alive and dead until we look in the box. Putnam's objection to the Copenhagen Interpretation is, in essence: it is bad enough to simply take the superposition of states as a given when the states are those of elementary particles, but it is defies the imagination when even macro-level objects can be in superposed states as well.

The important point for my purposes is that, according to Putnam, both of these other interpretations do not sit well with our theories of the world, conceived as a whole. If the laws necessary to make sense of the hidden variable theory are really very strange, and, as Putnam says in the quote above in the text, in violation of all the causal rules, then it would be better not to have to postulate them. In particular, we would have to add many beliefs to our theories, both in the form of the laws themselves, and in the form of the explanation of those laws, to make sense of them. Likewise, if the Copenhagen Interpretation can't make sense of how micro states give rise to macro states, or explain any principled reason for drawing the line at micro states, then it will appear to be ad hoc in character. Again, we will have to add many beliefs to our theories about the world which explain why there is this difference between the macro and micro world.[20] Either that, or we'll have to add beliefs which explain why it is that the macro-world actually is like the micro-world. But since this will involve our explaining how a cat can be both alive and dead when we're not observing it, this will need to be a lot of beliefs (not to mention, the disbelieving of a lot of beliefs currently held).

Putnam's solution is to limit the validity of the distributive laws so as to disallow the expansion of the right hand side of the statement of the conditional probability of a particle's hitting the region given that its gone through one hole or another.[21] That is,

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[20] As Putnam puts it "...the special character of macro-observables" will need to be "derived from some plausible definition of macro-observable together with a suitable formulation of the laws of quantum mechanics." (Putnam [1965] p. 150).

[21] Actually, one need only give up half of the distributive rules: from $r \& (h_1 \vee h_2)$ to $(r \& h_1) \vee (r \& h_2)$.(See Gibbins [1987] pp. 131-140) I will speak as though one has to give up the distributive rules, but what I mean will be that one has to give up this one side of the rule. The more complicated truth doesn't gain any advantage as far as the topic that I want to focus on, and it is much wordier.

Putnam's solution is to make the move from (1) to (2) illegitimate. This effectively just doesn't allow the offending prediction to arise, and so no conflict with experimental evidence can arise, either.[22] How does his solution address the problems that beset the other interpretations? According to Putnam, his solution makes logically impossible the idea of a complete description in quantum mechanics. Putnam claims that his quantum logical solution can retain determinacy, so that he can be a realist about the properties of particles.[23] Particles have an actual position and an actual momentum at every time, but the inability for us to predict what these are is not due to hidden variables. For every statistically possible position of a particle at a given time, there is a statement which asserts that the particle is at that position, such that the disjunction of all those possible position statements is true. Nevertheless, as a matter of logic, we will always be ignorant of which statement is the correct one if we have made a measurement for momentum.[24] There are no hidden variables, nor "strange laws" needed to explain them. The inability for us to predict is due to our own ignorance, but the ignorance is a logical consequence of the fact that quantum mechanics doesn't allow us to answer all physically meaningful

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[22] Of course, commentators have taken Putnam to task for his claim that quantum logic helps explain what's going wrong in the two-hole experiment. Stachel [1986] for instance, argues that Putnam's solution is not as explanatory as he thinks. (p. 319) See also Gibbins [1987] Chapter 10, especially pp. 147-9.

[23] Many have challenged exactly what this realism amounts to. For instance, Gibbins argues that his realism is in name only, since it simply allows that any given particle does definitely have a particular position and likewise does definitely have a particular momentum, but it cannot, by Putnam's solution, have a joint position and momentum (see fn. 24).(Gibbins [1987] pp. 151-2)

[24] Suppose that all n of the possible positions of a particle could be represented as a disjunction of statements, each of which asserts that the particle is at some place at some time. Then, it will be true that $(p_1 \vee p_2 \vee p_3 \vee \ldots p_n)$. Likewise, it will be true that $(m_1 \vee m_2 \vee m_3 \vee \ldots m_n)$, for each statement of possible momentum of the particle. This means that the conjunction of both of these long disjuncts will be true. That's the realism. Now suppose that we have measured the momentum, and we know that $m_3$ is the true statement. So we will have it that $m_3 \& (p_1 \vee p_2 \vee p_3 \vee \ldots p_n)$. Nevertheless, since the distributive law is restricted, we are logically prohibited from inferring $(m_3 \& p_1) \vee (m_3 \& p_2) \vee (m_3 \& p_3) \vee \ldots (m_3 \& p_n)$. That's what is supposed to expalin the ignorance.

questions. Putnam says,"'Indeterminacy' comes in not because the laws are indeterministic, but because the states themselves, although logically strongest factual statements, do not contain the answers to all physically meaningful questions."(Putnam [1968] p. 185) This, according to Putnam, is "not due to mere ignorance", however, since we could not have known both position and momentum without knowing a logical contradictions.[25] A consequence of this is that there is no need to distinguish micro- from macro-states, nor a need to explain superpositions. The cat is either alive or dead, just as the particle is either here or there, and either has this momentum or that momentum, its just that we are logically prevented from knowing which by the restriction of the distributive rules. He sums his position up, "The only laws of classical logic that are given up in quantum logic are the distributive laws...and every single anomaly vanishes once we give these up."(Putnam [1968], p. 184)[26]

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[25] See Putnam [1968] pp. 184-7.

[26] Here I think it is important to point out that we could decide to revise the principle, not globally, but merely locally, in dealing with quantum phenomena. Susan Haack has argued that this should not really count as a revision of logic, and that only global revisions should be seen as challenges to the primacy of classical logic.( Haack [1974] pp. 42-4) Haack's argument, in essence, is that if two logics are said to apply to different domains, then we should be able to distinguish the domains that they apply to. This will either be done according to the form of the sentences or their content, and either way she thinks there are problems. As I read Putnam, he seems to be suggesting that we use quantum logic only when the sentences we are concerned with are sentences in our theory of quantum mechanics. This would be a good example of someone arguing that we should adopt a deviant logic regarding some local domain based on what that domain is about. However, someone might distinguish domains by some formal property. For example, a proponent of tense logics might suggest that we use classical logic for non-tensed sentence, and tense logics for tensed one. According to Haack, the problem with the former criteria for distinguishing domains is that we would lose the topic neutrality of logic, and the problem with the latter is that it seems to do away with any sense of rivalry.
Both of Haack's arguments here are questionable. With respect to the first objection, Haack is surely correct that many philosophers think that logic ought to be topic neutral. However, this view of logic may be one that needs revising if empiricists about logic are correct. After all, advocates of a view like Putnam's would want to say that different logics do apply to different domains, and that therefore, logic is not topic neutral. At the very least, then, Haack's objection here seems to assume as correct a thesis which may lie at the heart of the debate. Haack's second argument is even more dubious: the argument that tense logic and classical logic are concerned with different domains and therefore are not rivals misses the point that many people think that we should use just classical logic, for every domain.( See, for instance, Resnik

My claim, then, is that revising logic might be simpler by Harman's simple measure. After all, the choices according to Putnam are 1) we add some very strange laws (which, since they are so strange, would require the addition of many beliefs to explain their inclusion in the theory); 2) we simply accept superposition of states at face value (and so must explain how it is that we can make sense of dead and alive cats, which will certainly require the adding and taking away of a number of beliefs); or 3) we restrict one logical law and so disallow the offending prediction. (3) is simpler because the number of beliefs that we have to take away (we wouldn't infer premises (2-6) above) would be less than the number of beliefs that we would have to add on either of the other two options.

Whether or not he particularly has simplicity in mind, Putnam clearly thinks that keeping classical logic is not preferable to other options. Putnam says,

> ...the situation in quantum mechanics may be expressed thus: we could keep classical logic, but at a very high price. Just as we have to postulate mysterious 'universal forces' if we are to keep Euclidean geometry 'come what may', so we have to postulate equally mysterious and really very similar agencies—e.g. in their indetectability, their violation of all natural and causal rules and their ad hoc character—if we are to reconcile quantum mechanics with classical logic *via* either 'quantum potentials' of the hidden variable theorists, or the metaphysics of Bohr.(Putnam [1968] p. 191)

In this section, I have tried to make sense of Putnam's claim, but showing exactly how it might be preferable for us to give up classical logic.

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[1996]) For them, any use of a deviant system anywhere is not justified, and so the suggestion that we use tense logics even only for tense sentences suggests that there is still a genuine rivalry between tense and classical logics. Most important to the current point, this second point misses the crucial fact that if we once thought that the logic of quantum mechanics was classical, and if we revised this belief for pragmatic reasons which included evidential considerations, then we should be counted as revising logic for empirical reasons (well, modestly empirical reasons).

## III) Two Kinds of Empiricism

### a) A plea for parity

In the Quine quote which opened the last section, Quine clearly suggests that empiricism about logic could be the view simply that logic is continuous with other more generally recognized scientific endeavors. It is time, now to explain this plea for parity, and make clear exactly what sort of empiricism is at work in my example in the last section. Empiricism, at its core, is simply the idea that our beliefs are justified by experience, not reason. But there is an ambiguity in what it means to be justified by experience, as opposed to reason. Beliefs are justified by experience in one sense when they are inferred in accordance with certain rules and principles. They are justified in a different sense when they are the beliefs which best fit the evidence. In the context of the question that concerns us, we have seen that empiricism about logic would amounts to logic's being revisable for empirical reasons. Likewise, there is an ambiguity in what it means to be revisable for empirical reasons. In one sense, a belief is revisable for empirical reasons if it is revisable by the rules and principles which confirm our beliefs. In the other, a belief is revisable for empirical reasons if it is revised so as to preserve as much of the evidence as possible. This being the case, there must be some sense of a belief's being empirical which is not as strong as the one that Field appeals to. In this section, I will develop this more modest conception.

Let us call the requirements for being revisable for empirical reasons that are operative in Field's argument, strong revisability requirements, and the corresponding notion of empiricism, strong empiricism.

> *Strong empirical defeasibility*: a belief, B, is strongly empirically defeasible if and only if it is possible to revise that belief by using (some one of) our best evidential methods (e.g., Bayesianism, hypothetico-deductivism).
>
> *Strong Empiricism*: A belief B is strongly empirical if and only if it is strongly empirically defeasible.

Beliefs which are strongly empirically defeasible, and thus strongly empirical, are beliefs like "All ravens are black", and "There are genes that predispose their owners to breast cancer." What Field has shown is that our most basic logical beliefs are not like these other beliefs: because we need deductive and inductive logical principles to be doing anything that deserves the name of evidence evaluation at all, the rules and principles of deductive and inductive logic are not strongly empirically defeasible.

But beliefs are also revisable for empirical reasons in a different sense, which is slightly more modest, and which correspondingly makes slightly less stringent requirements on what it is to be revisable for empirical reasons. According to this weaker notion, a belief could be revisable purely for pragmatic reasons and still be seen as empirical, though modestly so. This weak sense of empirical defeasibility simply requires beliefs to be defeasible by some method which seeks to conserve as many as of our evidential beliefs as possible.

*Modest empirical defeasibility*: a belief B is modestly empirically defeasible if and only if we could revise B by a method which is conservative over the evidence.

*Modest Empiricism*: a belief B is modestly empirical if and only if it is modestly empirically defeasible.

The general idea is that modestly empirical beliefs are those that can be revised in order to preserve as many evidence claims as is possible, which, of course, is every belief for the Quinean. My claim is that though Field is correct that logic is not strongly empirically defeasible, logic is nonetheless modestly empirically defeasible, and so modestly empirical. To combat Field's minimal notion of a priority, the empiricist should adopt a more minimal notion of empiricality. [27]

In the example in the last section, the reason why we use pragmatic criteria like a simplicity rule in deciding among these possible revisions is that the evidence does not determinately point one way or another. When the evidence doesn't determine how we should revise our total belief set, then our decisions are based purely on extra-evidential, pragmatic criteria. In going beyond the evidence, the empiricist generally judges which revision is best in terms of how well the proposed competing revisions explain the evidence in question. Many books have been written on explanation. For our purposes, we can just say that a theory is preferable when it lives up to various of our pragmatic

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[27] I will focus here on giving a more modest sense to 'empirical', but in part what I am also doing is questioning Field's contention that we should conceive of our evidential methods in such narrow terms. I could just as well argue that there is only one sense of empirical, but that the notion of evidential system should be broadened so as to include inference by purely pragmatic rules. This is completely in keeping with my arguments for a more modest sense of empiricality. However, I choose to argue for two sense of empirical because I think that Field is in some sense correct about logic being less directly empirical. Mostly, though, I consider this a matter of terminology.

standards. Or, in other words, that it is in living up to these standards that a theory is said to explain the evidence. I think a lot of different ways of conceiving of explanation are probably compatible with an empiricist conception of our methods. What is crucial in making them empiricist, however, is that their primary goal is to conserve the evidence.

The quantum mechanics example shows that decisions about which logic we use are continuous with decisions in science that are usually thought of as empirically motivated. All of the proposed revisions that Putnam considers should count as modestly empirical, because they all are made in a way that seeks to conserve the evidence of the two-hole experiment. Simply put, revising logic as a response to the two-hole experiment (and other anomalies of quantum mechanics) is just as empirical as is the Copenhagen Interpretation of quantum mechanics. Logic is like any other part of our scientific theories that go beyond the evidence, which is to say that it is revisable for modestly empirical reasons. This makes logic sensitive to evidence: different evidence might well lead to different sorts of logical theories.

Of course, Field's arguments make clear a sense in which logic is beyond empirical considerations, namely that it is beyond strongly empirical considerations. Nevertheless, there is no need to see logic as completely distinct from the empirical. Since pragmatic rules are what decide among competing hypotheses at this very theoretical level of science, the hypotheses should be regarded as equally, if only modestly, empirical. But, it will be objected, logic is still different than physical theories in that it is the consequences of adopting theories in physics which are directly empirical, whereas there are no consequences to adopting one logic over another: logic supplies the very mechanism of determining consequences in the first place. But, notice that there are

consequences to our adopting one logic over another: depending on which logic we adopt, we will find ourselves with very different belief sets. Deciding among different logics is a matter of comparing the various possible belief sets that adopting each logic would give us. Most importantly, comparing our different possible belief sets when we change our logic is not really epistemically dissimilar to comparing our various possible belief sets when we hold our logic fixed in order to derive the consequences of adopting different physical theories (or different versions of the same physical theory).

What this shows is that there are two facets to the modestly empirical: one of them has to do with our broad understanding of the content of our theories, the other has to do with how we think about our theories. Decisions regarding both are subject to the same considerations. That is, we want both to lead to good theories about the world as measured by their more direct strong empirical consequences. In principle, if there is a conflict among our strongly and modestly empirical beliefs, we may choose to revise any part of our overall belief set: something having to do with the strongly empirical (as when we might decide that our instruments weren't functioning, or that we've made an error in our calculations) or something having to do with the modestly empirical. And since this latter category is itself composed of two elements, the revision could in principle take place either in the physical theory or the logical theory.

b) A general concern for modest empiricism?

Pragmatic reasons are thought to come into effect when the evidence doesn't clearly indicate one way or the other; they are, as another name for them suggests, extra-

evidential. This fact about pragmatics is clear from my discussion of why it might be simpler to revise logic in light of the two hole experiment. Indeed, my explanation of modest empiricism makes great use of this fact about pragmatic rules: they are our preferred method when the evidence does not clearly indicate either way. Quine himself made very clear that our appeal to a principle of simplicity does not have anything to do with the nature of the world, but is instead, purely methodological. He says of simplicity:

> This strategy recommends itself on much the same grounds as the strategies of conservatism and modesty. The longer the leap, we reflected, the more and wilder ways of going wrong. But likewise, the more complex the hypothesis, the more and wilder ways of going wrong; for how can we tell which complexities to adopt? Simplicity, like conservatism and modesty, limits liability. Conservatism can be good strategy even though one's present theory be ever so far from the truth, and simplicity can be a good strategy even though the world be ever so complicated. Our steps toward the complicated truth can usually be laid out most dependably if the simplest hypothesis that is still tenable is chosen at each step.(Quine and Ullian [1978] p. 72)[28]

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[28] It is worth noting here that this stance, vis-à-vis the principle of simplicity was not always so popular. According to Elliot Sober, philosophers like Hume did not accept this explanation of our use of a simplicity rule: Hume thought that if one wanted to justify induction, one would have to somehow show that nature, itself was simpler. This task proving quite impossible, the answer preferred by contemporary philosophers like Quine is that simplicity considerations are purely methodological, in the sense that they have more to do with us than with the nature of the phenomena being investigated.(See Sober [1988] Chapter 2)

Sober, in fact, is critical of the idea that there is such a thing as a purely methodological rule of inference. Sober has argued that simplicity considerations can only be evaluated given background assumptions that include empirical information, thus casting doubt on the idea that there is such a thing as a purely methodological reason for revision. Sober argues that confirmation is a three place relation, obtaining between evidence and hypotheses only in virtue of background theory. To demonstrate this generally, Sober considers an argument in Good [1967]. Hempel famously argued that the reason why white tennis shoes don't appear to confirm the hypothesis that all ravens are black (though in fact they do), is that ravens are more rare than nonblack nonravens, and so white tennis shoes don't appear to offer the same degree of confirmation. But what Good points out is that whether white tennis shoes confirm a hypothesis or otherwise depends on what else one believes. Suppose one were to believe that either there are lots of ravens, and 99% of them are black, or that there are very few ravens, and 100% of them are black. Under these sorts of background assumptions, the more black ravens we see, the less confident we should be that all ravens are black. Good took this to show that confirmation is a three place relation, and that empirical assumptions are relevant to our assessing how confirmatory any given bit of evidence is of our hypotheses. What Sober takes this to show is that simplicity, too, can only operate in the context of background assumptions, including empirical ones. After all, if evidence E is said to confirm hypothesis H over H', it can only do so given our other background theories. When seen in this light, as Sober notes, simplicity considerations serve as the manifestation of background assumptions: to say that H is simpler than H' is just to say that, given our background assumptions, we should prefer H.(See Sober [1988] pp. 63-4) Sober gives a specific example from the field of evolutionary biology to show how simplicity considerations depend on empirical assumptions, but explaining it would take us too far afield. (see Sober,

Because pragmatic principles are seen in this purely methodological light, some philosophers have been skeptical of beliefs inferred as a result of purely pragmatic reasoning, and have claimed that these beliefs fall short of our epistemic standards of truth.

Harman's simple measure admittedly does invite this sort of worry. For instance, the simple measure is sensitive to the order of inference. Harman's example of this concerns the following five beliefs:

*P*

*Q*

*if P then R*

*if Q then R*

*not R.*

The minimal change here is to stop believing *not R*. But if one does not at first notice the inconsistency, one might instead first infer not *P* from *if P then R* and *not R*, and similarly, infer not *Q* from *if Q then R* and *not R*. In this case, the simplest thing to do is to stop believing *not P* and *not Q*.[29] But, it might be argued, either it is true that *not R*, or it is true that *not P* and *not Q*. So Harman's simple measure fails to yield knowledge of the way the world is.

Given considerations like these, it might seem doubtful that decisions made on the

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1988, Chapter 3). Anyone who holds, as empiricists like Quine certainly did, the coherentist theory of justification must recognize that no evaluation is free from background assumptions, including empirical considerations. Sober's argument demonstrates that methodological rules don't stand alone, and that their applicability in any given case may sometimes depend on strongly empirical considerations.

[29] See Harman [1986] pp. 58-9

basis of pragmatic principles would have any claim to getting at nature at its joints (to borrow a phrase from David Lewis). The most famous advocate of this sort of worry is Van Frassen. According to Van Frassen, pragmatic considerations are an essential component of scientific theorizing. But, Van Frassen argues, "They are specifically human concerns, a function of our interests and pleasures, which make some theories more valuable or appealing to us than others."(Van Frassen [1980] p. 87) For this reason, Van Frassen holds that we should be anti-realists about certain aspects of our scientific theories.[30]

However, I do not see that anything that I've claimed needs to be threatened by this line of argument. The whole debate between realists and anti-realists is, strictly speaking, irrelevant to the point I am emphasizing in this chapter. Though Van Frassen's constructive empiricism is itself one version of modest empiricism,[31] I want to be clear that I see no general reasons for modest empiricists to be anti-realists. In the first place, realists like Boyd, also acknowledge the role of pragmatic criteria in our reasoning (and

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[30] Van Frassen claimed that if a thing wasn't observable then it should count as theoretical, and our philosophical commitment to it should be instrumentalist, rather than realist. Van Frassen defined observable to mean, visible to the human eye, were the eye to be there to see it.(See Van Frassen [1980] pp. 13-9) Many took issue with his drawing the line where he did, and accused him of having a selective skepticism.(See for instance, Churchland [1985]. For an informative overview of the debate regarding not only observability, but realism and anti-realism more generally since Van Frassen's book, see Kukla [1998])

[31] He argues that theories must be empirically adequate, and this is a version of what I've been calling modest empiricism. A theory, for Van Frassen, specifies a family of models, and the empirical substructures of those models are the parts of the models that are supposed to represent observable phenomena. According to him, a theory is empirically adequate if it has a model where all appearances are isomorphic to the empirical substructures of that model.( See Van Frassen [1980] Ch. 3, especially pp. 63-9) Thus, though Van Frassen denies that our pragmatic methods provide epistemic reasons for belief, he is nevertheless an empiricist: scientific theories seek to save the phenomena, and in this are justified by their success, not their truth. On Van Frassen's view, we accept a theory, and commit to being guided by it as a research program and to using it as our conceptual structure for the world. This commitment is justified minimally in that it saves the phenomena.

even explain realism in terms of the general success of our method in doing so).[32] This shows that one's position on the realism/anti-realism debate is not predetermined by one's position on the role of pragmatic principles in our methods. More importantly, my argument for the empiricality of logic is based mainly on what I have called Quine's plea for parity. My position, therefore, is that whatever one's view about the realism/anti-realism debate, one should judge logic and theoretical physics to be continuous. Again, my only claim is that if the Copenhagen Interpretation gets to count as empirical, so should revising logic. Van Frassen may be an anti-realist, but he is also an empiricist, and for him, both would count as empirical. Realism and anti-realism are not what's under discussion.

c) Field on pragmatic reasons for revision

My argument against Field's conclusion depends on the idea that logic might be revisable for pragmatic reasons. We have already noted that Field can acknowledge that pragmatics have some role to play in our evidential systems. However, Field, is critical of the idea that logic could be revisable for pragmatic reasons. He gives many reasons for his skepticism. In the remainder of this chapter I will try to address his worries.

i) A problem with my example

The first one I want to address is Field's concern that Putnam's way of describing the paradox and its possible resolutions is too simplistic, and that, presumably, a more

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[32] See Boyd, [1984, 1985]

realistic construal of the options available to scientists and philosophers would show that quantum logic was neither simpler, nor more preferable.[33] My response is that I don't see why this should matter. Putnam's description of both the problems that arise for the other interpretations and of the work that can be done by his solution may be deeply problematic. But I'm using this example not because I think quantum logic is correct, or even because I think that Putnam's take on the paradox is the best on offer. Rather, I'm using it as an example of how it might be simpler to revise logic for pragmatic reasons. From this perspective, I could have just made my example up entirely (as I will do in the next chapter, in one case borrowing a fictional example from Priest, and in one case, with my own bit of fiction). What is important to make my point is not that the arguments for revising logic actually are good, only that they might be. This being the case, Putnam's argument, even if flawed, can provide a good example of how it might be simpler to revise logic. Put another way, even if it is in fact true that quantum logic is not simpler or more explanatory, and even if it is in fact true that no physical theory has yet offered a serious challenge to the supremacy of classical logic, this might be merely accidental. The challenge that I wish to address is that no decent evidence evaluating system that we currently know of *could* show how the rules of logic were wrong for empirical reasons, not that no evidence evaluating system *actually has* done so.

ii) A problem with Harman's simple measure

A second problem concerns my appeal to Harman's simplicity principle. Field is quite clear that there may be unattractive simplicity measures that would make logic empirical. One example he gives, "...we can rule it simpler to abandon modus ponens

[33] This problem was relayed to me in personal correspondence with Field.

than to keep the combination of modus ponens and that snow is black, so that modus ponens would be revisable on the basis of the color of snow. But," he goes on, "the issue is whether there is a measure of simplicity etc. that would make logic empirical in a way that anyone would regard as minimally attractive." (Field [1998] p. 14) According to Field, Harman's simple measure is one of these unattractive simplicity rules. The reason is this: logical rules don't have to be specifically formulated in our inferences (they are rules that guide inference, and need not make an explicit appearance in the premises), so, removing a rule from our bag of approved rules of inference won't involve any explicit change in belief. Harman's simple measure would always make it simpler to revise logic.[34]

My response to this objection is that the rule in question may have many applications in our overall beliefs, even limiting it to the domain where we wish to restrict its applications. If this is so, then we have to consider all of the conclusions that we will be prohibited from making as a result of restricting the rule in the domain in question. If there are enough of them, and if, in particular, many of the conclusions that we would have to disbelieve as a result of revising our logic are conclusions that we would like to hold on to, then we will have to add a lot of beliefs in order to either get them back another way, or to explain away our displeasure at no longer getting to believe them. In this way, Putnam's example is particularly apt: he claims (probably incorrectly) that restricting the distributive rules would have the effect of disallowing all of the

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[34] Again, this is from personal correspondence. Of course, even if we don't have to explicitly include a rule as a premise in our inferences, the rules we employ might well be explicitly endorsed somewhere in our overall web of belief, and so we would at least have to unendorse them. Nevertheless, even if this is true, unendorsing one or two rules of inference will likely usually be simpler than adding explanatory beliefs, so the spirit of the objection still is in order.

undesirable conclusions of quantum mechanics. In other words, the other beliefs that we'll have to drop as a result of switching to quantum logic would be the one's that were also causing troubles. Since this may or may not be the case with every possible suggestion to revise our logic, I don't think Field's objection just automatically shows that Harman's simple measure makes revising logic too easy. Of course, there may be other objections to the simple measure, but the one that he has voiced to me does not, I think, preclude it as attractive.

iii) How do pragmatic rules really help the empiricist?

I have already called attention to the fact that the strictly evidential systems of strong empiricism actually do make use of at least some pragmatic rules. For example, even our strictly evidential methods need to use simplicity in ruling out grue-like predicates, a fact which I mentioned at the beginning of my discussion of the role of pragmatics in the last section. Attention to this fact raises two possible worries for my argument. The first is the most serious: pragmatic rules seem to be just as necessary for the evaluation of evidence as are deductive and inductive rules. This being the case, pragmatic rules would seem to be as a priori (in Field's minimal sense of the term) as deductive and inductive rules.[35] Addressing this issue is the subject of the next chapter. There, I argue that pragmatic rules are themselves revisable for modestly empirical reasons, and I show why Field's arguments concerning the a priority of deductive and inductive rules do not apply to pragmatic rules in their modestly empirical role.

The second worry is quite different. Having acknowledged that pragmatic

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[35] Again, personal correspondence with Field.

criteria do have an essential role to play in evidence evaluation (their modestly empirical role, as I have called it), why should Field think that their role outside of evidence evaluation is one which deserves to be called empirical? Field is very dubious that there is a decent measure of simplicity which could make logical rules be revisable for empirical reasons. He says, of something like the suggested pragmatic method sketched in the last section,

> ...whether the method so described is a priorist about a given part of logic depends on how one measures simplicity and conservativeness (and to a lesser extent, on how one weights them against agreement with observations). In particular, it depends on whether the decrease of simplicity and conservativeness that results from modifying the logic could be compensated by increased simplicity and conservativeness in other parts of the theory (holding the observational predictions fixed). Without a measure of simplicity that gives an affirmative answer for some but not all choices of the possible observational predictions, one doesn't have an inductive method on which the logic in question is empirical. And it isn't in the least obvious that there is any possible measure of simplicity with these properties which is at all attractive.(Field [1998] p. 13-4)

If I've understood the issue here correctly, we can rephrase Field's difficulty in the following way: why should we regard the modestly empirical revisions that I'm advocating in this chapter as empirical? After all, if every possible revision in the quantum logic case has the same empirical content, then what's empirical about using simplicity to decide among them?

My answer to this should, I hope, already be clear. What I've argued is that we can revise our logical beliefs when the resources of our evidential systems have been exhausted. In addition, I've offered a plea for parity: if the other options are empirically motivated, then so is revising logic. I do, in fact, think that all of these revisions should count as empirical, if only modestly so. These revisions are empirical because, in effect, they seek to save the evidence. Just because the live options (to borrow a phrase from

William James) regarding revision in the quantum mechanics case all would be modestly empirical, doesn't mean that there aren't possible revisions which aren't modestly empirical. We could deny the evidence, for instance. Of course, that would be crazy, and no one does that. But that's because we're all modest empiricists. We all think that, whatever our theories about the world, they should be successful, where we measure success by a theory's ability to do many things: control our environment, respond to more strictly evidential tests (these are the tests that get done when we're using our evidential systems), predict novel phenomena (again, by using our evidential systems). But, that said, it is undeniable that sometimes we must go beyond the evidence in a way that merely seeks to preserve and explain it. So, my point could again be put in this way: if the Copenhagen Interpretation is an empirical theory, then so is quantum logic. Both are a response to the same puzzling evidence. Perhaps neither are strongly empirical, but they are at least, both modestly empirical.

iv) What if logical notions are required for our use of pragmatic principles?

The problems that get raised here all revolve around the fact that we will still need some logic when we assess the preferability of one belief set over another, even restricting ourselves to belief revision in the extra-evidential case. If this is so, then at least some deductive rules would still be a priori, after all. How does logic play a role in the pragmatic method? Field makes clear two ways that this might happen. He says,,

"...you need some background logic to determine what consequences will result from using the theory's own logic, and to assess how well these consequences agree with observations; a fact that might lead one to suspect that at least this background logic would turn out not to be empirically revisable."(Field [1998] p. 13)

The first problem that I will address is that we will want our revision to have observational consequences, so that we will need at least some logic for deriving observational consequences. This might be one of two different problems. In the first place, it might just be another way of making the point that that the revision should have evidential teeth. That is, this might be a slightly different way of complaining about the empirical merits of my arguments for when and how logic might get revised. Of course, we do generally want our theories to have observational consequences, but this may not always be possible. I am not aware of any observational consequences that would help to distinguish the various interpretations of quantum mechanics from each other. If this is true, then though observational consequences may be desirable, they are not always attainable. For this reason, any revisions that don't have observational consequences would not be strongly empirical. But that's fine. Again, mine is a plea for parity. My claim is that revising logic is just like other revisions that don't have empirical consequences: these revisions are not strongly empirical, they are modestly empirical.

The second way this first worry might be developed goes like this: I have been claiming that we should compare and contrast total belief sets. In the case that I developed, these total belief sets include all of quantum mechanics, observational predictions included. That is, these total belief sets include the evidence for the truth of the quantum mechanical view of the world. These observational predictions must be derived using some deductive and inductive rules, so our evalution of total belief sets can not really be independent of these rules. If this is the worry, then it seems to rest on a conflation of evidence gathering and belief revision. In belief revision, the observational beliefs are a part of the belief set to be revised; they are, as it were, already derived. With

this in mind, any revision that had us disbelieve observational consequences would likely not be simpler, since observational consequences make up such a large part of our body of beliefs. In the quantum logic case, we would have a huge number or observational beliefs we would have to give up if we didn't retain the observational consequences of our theories. We do not, in revising our beliefs, start as though from scratch every time. Rather, we just take the observational consequences as given (unless, of course, within the domain for which the restricted rule is being considered there are observational consequences which will be effected, but that is an issue which I have already addressed). Indeed, the observational consequences are precisely the evidence every revision will have to take account of if the revisions are to count as modestly empirical, on my view.

The second problem that Field raises concerns the role of consistency in our extra-evidential reasoning. It is clear that we will want to revise in the way that is simplest consistent with the evidence. Since this involves some notion of consistency, Field seems to be arguing that logical rules must be playing some role here. Of course, simplicity rules as I am conceiving them don't need to make explicit reference to being consistent with the evidence; Harman's simple measure surely doesn't. Nevertheless, we will only want to consider options for revision which are consistent with the evidence, so the spirit of this objection still needs answering.

My response to this second problem is two-fold. First, why should we think that the relevant notion of consistency here is logical? I see no reason why we could not single out a class of beliefs, the evidential ones, and just treat those in a way which doesn't make them susceptible to revision (hold them off to one side, as it were). If we did this, there would be no clear sense in which logical consistency was what was at

work, and so no conflict of the kind that Field is worried about. But, even if we do need some notion of logical consistency here, I have a second response: I can see no reason to think that the consistency involved will be one which restricts our ability to revise the rules of logic. The sense of consistency that will be at work in trying to pick out the simplest revision consistent with the evidence will not be one which needs to single out any one particular logic (or even any one particular rule, as far as I can tell). Field seems to think that the sense of consistency involved will at least rule out our revising the principle of bivalence,[36] but I'm not sure I see why. After all, to say that something should be consistent with the evidence is just to say that the evidence shouldn't say p, where p is some evidential belief, and the revision say not-p. But most non-classical logics can guarantee that. Remember that the question I'm focusing on right now is whether some rule or principle of logic might be revised for pragmatic reasons. As long as there are non-classical notions of consistency, there will be room for revising one logic over another.

One last possible difficulty along these lines is to be found in Boghossian [2000]. Boghossian argues that pragmatic rules might still need logical principles in order to be applied to our revisions. For example, the empiricist will surely need to reason at least as follows: The best set of observation sentences is the set with property F (say simplicity and conservativeness). Set O has property F. Therefore, O is best. (Boghossian [2000] p. 233, my paren.) Clearly, the reasoning here depends on some rules of logic. This being so, it seems that we haven't really avoided the minimal a priority of logic in going over to

[36] In personal correspondence.

pragmatics at all: logic is still minimally a priori, because we can't be evaluating pragmatically without appealing to some deductive rules of evaluation.

This objection, I think, cannot be avoided. As we will see in the next chapter, the principle of non-contradiction, though necessary for pragmatic evaluations is revisable for modestly empirical reasons,. However, as I will make clear, precisely what makes this principle revisable is that it is not being used to derive consequences in a way which precludes its own revision. If a rule is necessary for inference in our purely pragmatic evaluations, then I must concede that that rule will have to be a priori, and may not be modestly empirical, for exactly the reasons Field gives.

This brings up a point of Field's which I mentioned briefly in the first section of this chapter, but have largely ignored since then. For the radical empiricist to really get what she wants, it will have to be possible that no deductive rules are such that they can't be revised for empirical reasons, modest or strong. If Boghossian is right that modus ponens is unavoidable when we use the pragmatic method to revise other rules of logic, then it seems the radical empiricist will not get everything she wants. Nevertheless, I think we can say a few things here to lessen the blow to radical empiricism. In the first place, the radical empiricist seems to have gotten an awful lot: every rule except modus ponens is potentially revisable for empirical reasons. Contrary to what many have claimed (my specific concern here being Field), many, perhaps most, rules of logic are such that they can be revised for modestly empirical reasons. Secondly, of all the rules to have to admit might be purely a priori, modus ponens is probably the least damaging. After all, again putting this in the context of classical verses non-classical logics, every logic accepts some form of modus ponens. So, as with my point about consistency

above, even if we do need modus ponens, the vast majority of non-classical logics will have a modus ponens like rule. What this suggests is that we may still have empirical reasons for thinking that we shouldn't use classical logic. This being the case, the empiricist has gotten a great deal of what she wants.

v) Questions about our real reasons for revising logic in my example

I will address one final problem of Field's. Even allowing that it might be simpler to revise the distributive rules, Field argues that this revision would be attractive on totally independent grounds, and so would not be an empirical revision after all. He argues that even if we grant that Putnam's argument for quantum logic works in some sense, this needn't establish that logic is revisable for empirical reasons. He says,

> ...if...nondistributive logic became a possibility, then the case for switching to nondistributive logic would be strong on non-empirical grounds. One of the attractions of it would be simply that it is weaker and yet still allowed us to conduct our affairs; another is that it would allow for certain possibilities ('non-distributive superpositions') that there could conceivably turn out to be evidence for. If 'nondistributive superpositions' are possible, then their possibility (which needn't have anything to do with quantum mechanics) could in principle have been recognized prior to anyone noticing quantum phenomena, and the shift to a logic that allows for them would be attractive on conceptual grounds. They would be useful in quantum mechanics in the same way that certain free logics are useful for sentences containing 'Santa Claus'; quantum mechanics would be no more evidence against the distributive law than the non-existence of Santa Claus is evidence against the principles that distinguish classical logic from the appropriate free logic.(Field [1998] pp. 15-6)

The best way to begin thinking about this problem, is with a question: why does a revision becomes attractive to us? According to Field, among the general criteria we use for deciding among logics are the logic's strength and its ability to capture possible evidence. But if we have general non-empirical reasons for preferring weaker logics, and for allowing for possibilities that we might, conceivably have evidence for someday, then

why shouldn't we think it attractive to switch to paraconsistent logics? After all, these, too, are weaker, and yet still allow us to conduct our affairs. And they also make room for inconsistent phenomena, and we might someday have evidence of this.

The reason, of course, is that we don't have evidence for it, and, more particularly, nothing in our current scientific thinking about the world makes us think that there might be this kind of phenomena. Of course, we might have other non-empirical criteria that would rule out paraconsistent logics in particular, but my point is entirely general. In the abstract, there are many, many logics we might use. Given the many possible logics we might use, evidence might actually help us limn the options. I have not denied that there might be some non-empirical considerations which cause us to favor one logic over another. What I have denied is that these are the only sorts of reasons that cause us to favor one logic over another. Evidence may not be the only thing that helps us rule out other logics, but it might be a contributing factor. Modest empiricists can tell us why we shouldn't adopt paraconsistent logics for empirical reasons: we have no evidence to suggest that there is inconsistent empirical phenomena.[37]

Of course, revising our logic is not something we can do for strongly empirical reasons, and if one only considered strong empiricism, then between empirical and non-empirical reasons for revision, I'd have to favor non-empirical reasons as well. This is because I have agreed with Field that it does seem incoherent to revise logic for strictly

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[37] Even Priest, I think, doesn't think that we should use paraconsistent logic for empirical reasons in the sense that I'm using the term. That is, the evidence he usually cites regarding inconsistent phenomena have more to do with mathematical and logical and philosophical concepts, e.g., set theory, and the truth predicate. He does have one fanciful story where he imagines that someone has been given a box which is both empty and occupied.(See Priest [1997] If this story were true, then I would certainly agree that we might have evidence that we should become paraconsistent logicians (at least for the box domain).

evidential reasons, and those are the ones at work in strong empiricism. Nevertheless, the more modest form of empiricism that I have defended requires simply that, when all the evidence is in, we can revise and adjust our total belief set in any way which is conservative over that evidence. We do this by considering various pragmatic criteria and comparing the competing total belief sets we might adopt.

## Conclusion

We have seen in this chapter that the rules and principles that underlie our strictly evidential methods can not seem to undermine themselves, at least not by the lights of some of our best evidential methods. In addition, we have distinguished two senses of empirical: in the stronger sense that Field uses, beliefs are empirical if they are revisable for strictly evidential reasons, whereas my preferred more modest sense says that beliefs are empirical if they are revisable in a way which is simply conservative over the evidence. I have argued that logic is modestly empirical because it can be revised for pragmatic reasons and I have given an example to show how this might happen. I have defended my modest conception by offering what I have called a plea for parity: since theoretical physical beliefs get inferred as a result of purely pragmatic reasoning, logic is at least as empirical as these. Lastly, I have tried to address several possible and actual objections to the position that I have advocated in this chapter.

## Chapter 4: Revising Empirical Methods

In the last chapter, I argued that though logical rules and principles may be priori in Field's minimal sense of a priority, they are also empirical in my more modest sense of empirical. The strength of my argument rests on the plausibility of three claims in particular. The first is that some of our beliefs go beyond the evidence and are decided purely on the basis of pragmatic criteria. The second is that logic is among these beliefs. The third is that these sorts of beliefs, though not strongly empirical, are still modestly empirical, and so logic is at least modestly empirical. Since so many philosophers seem prepared to concede that at least some of our more highly theoretical beliefs do go beyond evidence, I focused on showing the second and third of these claims. I tried to show exactly how logic might be revised for purely pragmatic reasons, and I argued that pragmatic rules are part of a method that deserves to be called empirical because revisions in accordance with this method are conservative over the evidence: that is, revising should limit the amount of change in belief with respect to evidence, whatever we count as evidence.

Nevertheless, my defense of modest empiricism is importantly incomplete, for I have said nothing, so far, about the epistemic status of pragmatic rules. Clearly, if

pragmatic rules cannot be revised for modestly empirical reasons, then my defense of empiricism will have failed. After all, pragmatic rules seem to be essential to the proper functioning of our evidential systems. In addition, pragmatic rules are just as necessary for the extra-evidential evaluation of total belief sets as deductive and inductive rules are for the evaluation of particular evidential beliefs. Therefore, if these rules are not revisable for modestly empirical reasons, then we will have no choice but to regard them as wholly a priori, in Field's minimal sense of the term. In this chapter, I will complete my defense of empiricism and show how these pragmatic rules might, themselves, be revised for modestly empirical reasons.

To begin, I will examine Jerry Katz's argument that pragmatic rules are not revisable for what I have been calling modestly empirical reasons. According to Katz, there is a very general problem with Quine's thesis of universal revisability. In order that every belief of ours be revisable, our belief in the correctness of our most basic rules of belief revision would have to be revisable as well. Katz argues that, since any argument for the revision of these latter beliefs would have to use the rules in question, this would undermine these rules' authority to serve as reasons. In the first section, I will explain Katz's worry and show that it rests on inflated factualist assumptions about justification. In the second section, I will explore some fictional examples of how the basic rules of the pragmatic method might get revised for pragmatic reasons. I will show that Field's arguments for the a priority of logic do not carry over very well to the case for the a priority of pragmatic methods. In addition, I will try to assess the overall plausibility of the examples I give.

## I) Katz and the Revisability Paradox

Katz has raised what one might generally call a self-defeating problem with revising the rules of our pragmatic method. Katz is concerned that any argument against the rules of our most basic method would undermine itself. After all, as a rule of our most basic method, it must itself be used in the revision. But this would involve our using a rule which the conclusion declares to be unfit. Not surprisingly, similar claims have also been made against the idea that we might revise the basic rules and principles of deductive and inductive logic.[1] The problem here is completely general: we would use a rule in the derivation of a conclusion which declared the rule incorrect.

Katz's argument is supposed to demonstrate the "impossibility" of Quine's thesis of universal revisability. The claim, in particular, is that Quine's epistemology is subject to a paradox whereby basic principles of belief revision both are and aren't revisable. According to Katz, Quine's epistemological holism, is constituted by three principles: the when of revision, the where of revision, and the how of revision. The when is the principle of non-contradiction: we should revise our belief set when there is some sort of contradiction, usually between our theories and the experiential evidence which lies at the

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[1] Fine [1972] says, of his portrayal of the sort of argument that one would have to give to revise logic for empirical reasons: "...the very same grounds that would support (the inference) manage themselves to undermine (the conclusion)."(p. 12, my paren.) Heelan, [1970] worries that the arguments of the quantum logic advocates in particular run into a self-defeating problem. According to Heelan, Putnam's argument takes the form of an instance of disjunctive syllogism (Either classical logic is correct, or quantum logic is correct. Classical logic is not correct. So quantum logic is correct). But disjunctive syllogism isn't valid in non-distributive quantum logics.(see pp. 323-5) There is even a brief mention of what looks like this sort of problem in Mittelstaedt, [1981] p. 4, where he say, "...even if logic is based on experience, it must be clarified how quantum mechanics as a theory which makes us of classical logic can give rise to the discovery of a non-classical quantum logic."

periphery of our web of beliefs. The where is the thesis that everything is revisable: we can in principle revise wherever we see fit. The how is the principle of simplicity: we should revise in such a way as to make the least possible adjustments to our overall belief set. Katz says of these constitutive principles, "...it is impossible for an argument for belief revision to revise any of them because revising any one of them saws off the limbs on which the argument rests."(Katz [1998] p. 73) He then goes on to provide a specific example of the problem.

> Consider a special case of the paradox. Given universal revisability, the principle of noncontradiction is revisable in principle. If it is revisable in principle, there is a possible belief-revision argument for its reevaluation. But, as we have seen, since the principle of noncontradiction is a constitutive principle, it must appear as a premise of the argument. But if it is right to revise a belief in the system, that belief was wrong all along, and if it was wrong all along, it cannot be a part of a sound argument. The argument for revising the belief would be unsound and provide no grounds for the revision. Hence, there can be no sound argument for revising the principle of noncontradiction, and it is not open to revision. Nonetheless, since all beliefs are revisable, the principle of noncontradiction must be revisable, and hence it is both revisable and non revisable.(Katz [1998] pp. 73-4)

There are many ways that Katz's argument here might be questioned. In the first place, and probably least importantly, this is a somewhat simplistic reading of Quine's theory.[2] Nevertheless, making the picture more realistic won't, as far as I can see, either help or hinder Katz's essential point, but would simply add more complications, so I will not press this issue here. The supposed problem is that if these principles are genuinely constitutive of belief revision, then they cannot, themselves, be revised, and this is a general challenge which I think worth considering.

A more important source of discomfort with Katz's argument comes from his contention, clear in the quote above, that the pragmatic rules we use in belief-revision

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[2] As I noted in the last chapter, there are many other factors as well.

must appear as premises in the arguments in which those rules are used. This assumption is, in fact, quite dubious, and I know of no analogous requirement for the rules of our more strictly evidential methods. Nevertheless, I wish to let this problem slide, as even conceding his point, I think there is a major and important problem with his claim.[3]

So, let us grant that Katz is right that, if these principles are genuinely constitutive of belief revision, then they must be a part of any actual argument that they be revised. The question is whether he is correct that this thereby means that revising any of these fundamental principles is not possible. Certainly, there is an intuitive prima facie case to be made: it just doesn't seem possible that we could use some principle in an argument, say the principle of noncontradiction, to arrive at a belief that that very principle is not correct. Intuitive as it is, however, I will argue otherwise.

One question which will be important in what follows is what sense of possibility (and impossibility) Katz means to be evoking here.[4] As we saw in Chapter 2, Field tries to distinguish epistemic possibility from what he calls genuine possibility in his explication of his undogmatic a priorism. The idea is that though some things are

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[3] What I will concede is that arguments which utilize basic rules of belief-revision require premises that endorse those rules. That is, that an argument which used, for instance, Harman's simple measure rule, would have a premise which endorsed the correctness of the simple measure rule; something like, "It is correct to reason according to the simple measure rule," or, "We ought to reason in accordance with the simple measure rule." I am not, however, conceding that the simple measure rule need itself appear as a premise in an argument for its revision. The reasons for this will become clear when my solution to Katz's paradox is explained.

[4] At least since Kripke's famous "Naming and Necessity" lectures, philosophers have tried to distinguish merely epistemic possibility from other sorts of possibility. Kripke tries to distinguish it from a notion of metaphysical possibility, or being true in all possible worlds. Since my focus here is on epistemic issues (and since, as a nonfactualist, I am essentially denying a substantial metaphysics of epistemic notions), it would be best, I think, not to have to go into the idea of metaphysical possibility as well. For this reason, I will focus on the two senses of possibility that Field develops.

possible to imagine, in the abstract, we have no idea how to imagine them mathematically or logically coming about. Field's example, recall, is the inconsistency of set theory: it is possible in the abstract that set theory is inconsistent, but until we see the mathematical proof of the contradiction, this possibility remains purely epistemic, and is not genuine. So, though the inconsistency of set theory is not impossible, simpliciter, it is possible in a way that is so mathematically and logically unexplained that this possibility is not, at this point in time, a genuine one. The question, therefore, is whether Katz is claiming that it is genuinely, as opposed to merely epistemically, impossible that we could revise the principle of noncontradiction or of simplicity by its own lights. Correspondingly, we also need to know whether Katz thinks that the empiricist must show that it is genuinely or epistemically possible to revise pragmatic rules for pragmatic reasons.

Katz does not explicitly say either way. It is clear, however, that what Katz objects to is the possibility of a particular sort of argument structure, and his reasons for rejecting this possibility have to do with a problem of logical structure. That is, the reason for the impossibility of this sort of argument have to do with general problems concerning inferential support: a principle can not be part of an argument for its own inadequacy. Given this, it seems plausible that Katz is claiming that it is genuinely, i.e., logically, impossible to revise a basic principle of belief formation, B, for reasons as given by B. This would be genuinely impossible, according to Katz, because the logical situation is such that the revision would be unsound if true. This being the case, it would seem that to refute Katz's claim, the empiricist needs to show that it is genuinely (logically) possible to revise pragmatic rules for pragmatic reasons.

My argument against Katz's claim will depend on a particular view of justification. Specifically, it will depend on a nonfactualist view of justification like the one advocated by Field in Chapter 2. Recall that a non-factualist about justification takes questions of justification to be evaluative ones that don't ever get factual answers. As we saw, Field claims that nonfactualism helps cast certain issues in epistemology in a more tractable light. For instance, he argues that the switch to a nonfactualist perspective makes clear the strength of Field's own argument presented in the last chapter.[5] From a nonfactualist perspective, systems that treat logic as a priori seem best because there are no obvious good alternatives that treat it otherwise.

One important consequence of adopting a more nonfactualist approach to justification is that evaluations concerning what is and isn't justified are subject to indeterminacy in many ways. For the nonfactualist, being the best method is a matter of a method's being the best overall given our epistemic requirements and goals. But there may be no clear cut way to call one method better than another, if both do a minimally decent job of achieving our goals given our requirements. Suppose we are trying to choose between two theories, Theory A and Theory B, but both do a very good job of accounting for the phenomena we are concerned with. If we decide to believe in theory B because, say, it seems slightly better in achieving some goal that we particularly value, this doesn't necessarily show that Theory A is undermined. As Field says, "...(a) system could be an extremely good one, even if there were better ones."(Field [1996] p. 370) In this case, it would be indeterminate whether B is better in a way that rules out the

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[5] See Field [1996] p. 379, and [2000] p.127 for argument regarding nonfactualism and skepticism. For a summary of these arguments, see my discussion of Field's evaluativism in Chapter 2.

reasonableness of A. Let us say that when this happens there is an indeterminacy regarding correctness. This possibility arises because, as I noted above, a nonfactualist must admit that two theories might both do a good enough job for us, or even a very good one. We might still prefer one for being slightly better, but not better in a way that requires us to ever see the abandoned theory as not good enough.

Field's example of this sort of indeterminacy concerns philosophical theories about the a priority of logic, itself. As we have seen, as an undogmatic a priorist, Field cannot rule out the possibility that there might be an evidential system some day, unobvious to him now, which does treat logic as empirically defeasible (in his strong sense). According to Field, even if there were, this would not mean that treating logic as a priori, now, is somehow incorrect. As a non-factualist, Field can recognize that there may be many different evidential systems which each do a very good job of meeting our cognitive needs. Acknowledging that this situation is a result of his non-factualism regarding justification, Field then goes on to say, in essence, that if one were a factualist, one could hold fast to the idea that theory A really was the truly correct theory, thus undermining theory B.[6] Field, of course, doesn't take this to show that there are facts about evidence, only that some people think that there are.

Two questions, I think, raise themselves at this point. In the first place, why wouldn't our choice of one method that makes logic empirical simply amount to a

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[6] Field says, "Of course, if you think there are facts about evidence, and that an evidential system is 'correct' if it accords with those facts, then the existence of a 'correct' system that treats logic as empirical would show that systems that treat logic as a priori are incorrect."(Field [1996] p. 370) What Field is acknowledging here is that if it were true that there were such facts, then it might make some sense to say of our belief--that an evidential system which treats logic as empirically defeasible is a good thing--that it really was incorrect.

repudiation of any method that didn't? According to Field, it will take some sort of conceptual shift for his imagined scenario to take place. That is, it will be because there is some sort of conceptual development that he imagines we might be able to go from thinking logic is a priori to thinking it is not. And from what was said in the first part of the last chapter, we can imagine the sort of conceptual shift that Field has in mind: we will have to come up with an evidential system which somehow does not treat the truths of logic as Bayesian or hypothetico-deductive systems do. Notice that from the perspective of our former (Bayesian or hypothetico-deductive) methods, we were not incorrect, and still aren't: what Field has shown is that these methods do not allow that logic be revisable for empirical reasons. Given this, I propose that the answer to the first question is that we need not see our former theory, that logic is a priori, as incorrect because at the time we were not in a position to see the possibility that it might be otherwise. It just wasn't a part of our conceptual repertoires before the conceptual developments (whatever those are), and so we are not in any way at fault epistemically.

The second question is, what considerations might lead us to think that two methods, one which makes logic a priori and one which doesn't, are both good enough? How might we think that both methods are in some sense good enough? My answer to this question has two parts. First of all, we should ask whether indeterminacy of this sort is really so surprising. That it might be indeterminate which method we should use seems to be a fact for other theories that we've looked at, as well. For instance, take reliabilism. Neither Goldman nor Rey nor Kitcher specifically discusses the point, but it seems clear to me that they, too, should allow that there might be indeterminacy regarding the reasonableness of adopting various methods of belief revision. According

to reliabilists, methods (what they would call processes) of belief-revision would be acceptable to the extent that they are reliable producers of true beliefs. But there is no reason to think that one method will have a lock on reliability. Borrowing an example from Field: imagine two systems, one of which is stronger than the other in that it takes more risks, and thereby comes to produce a greater quantity of true beliefs than the weaker system.[7] If the stronger system sometimes fails when it takes risks, but succeeds big when the risks pan out, then we could imagine that it would be indeterminate which system is more reliable. The stronger system is more reliable in that it actually gives us knowledge that the weaker system wouldn't achieve; it is more powerful. But it is less reliable in that it produces, say, a greater proportion of false to true beliefs than the weaker system (though within the range which is still considered acceptable as far as being justified goes). Obviously, we could do a lot to tighten things up by being more specific about how we are measuring reliability, but I don't see it happening to the point of completely eliminating the possibility of disagreement over judgments concerning what is justified. I would think that a reasonable reliabilist will want to recognize that scenarios like this can arise, and that reliability judgments as to something's justificational status might be highly indeterminate at times. So, the first part of my answer to the second question, why might we think that two methods are both good enough, is: whatever the reason, other theories seem to face similar difficulties.

But the second part of my answer will be more straightforward: thinking that two methods might both be good enough is part of the pragmatic bent of nonfactualism. Turning again to Field's example, Field says,

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[7] See Field [2000] p. 141

> ...we can't reasonably expect that the inductive method we employ is the best one possible; only that it meets our epistemic goals fairly well, and that we aren't in a position to employ any other method that would do better. A method can be completely acceptable now, and yet be superceded later if a better method becomes available. If it turns out that it is possible to improve our methods to make the question of the distributive law empirical...then the conclusion wouldn't be that the law *really isn't* a priori: the notion of really being a priori simply makes no sense. Rather, the conclusion would simply be that though the law is a priori according to the very good inductive methods we use today, it isn't a priori according to the still better methods we may someday develop.(Field [1998] p. 16)

From a nonfactualist perspective, there is no transcendent sense in which a method *really* is or isn't a priori. What matters is that the method that we employ be the best of all the methods that we might otherwise employ. Field thinks that a method which makes logic a priori is better in this sense: that it is the best that Field can come up with given our current thinking on the subject of logic's role in belief formation and revision. In the event that there is some conceptual shift which makes clear how logic could not be a priori, this will do nothing to vitiate the sense in which logic is a priori on the current accounts. Granted that Field's current way of looking at things is the best we can do for now, and granted that whatever the new method will be in the future, it will be the best we can do then, both are good enough for the purposes that we put them, by fiat. Both of these ways of looking at logic's epistemic status might be good enough because both methods do a good enough job at what we want our methods to do

How does nonfactualism, and the possibility of indeterminacy of correctness in particular, help address Katz's paradox? What nonfactualism paves the way for is an argument, one of whose premises is warranted by a fundamental rule of belief revision, which concludes that we should not reason according to the rule in question. Nonfactualists about justification could make sense of this sort of revision in cases where

there was an indeterminacy regarding the correctness of the rule in question: as long as two methods might both do a minimally decent job for us, then we could use the rules of one method to decide that we should be using the rules of another method. Let us call our pre-revision methods M1, and our post-revision method M2. Nonfactualists accept that systems like M1 might be good, even though M2 is better. This makes it possible for us to coherently revise some rule in M1, thus endorsing a new method M2 which doesn't include the rule in question. Nonfactualism leaves open the possibility that it might be indeterminate whether we should think of our use of the revised rule as correct or incorrect.

As a factualist, Katz would have no truck with this idea: he would argue that as long as there is a fact of the matter, and as long as there is some non-question begging way of discerning which fact it is, then we need not accede to the claims of indeterminacy.[8] I'll concede, for the sake of argument, that if there is a fact as to what we should do here, and if we can specify our access to this fact in a way that's acceptable to all parties involved, then the above argument would be self-defeating. If it is a fact that we should abandon some basic rule of belief revision, then Katz is correct that we cannot argue that we should do so using the very rule itself. According to Katz, any argument that did this could not be sound. This would, indeed, involve our using rules whose correctness we then came to deny.

But lacking a fact, there is no need for us to come to doubt our use of the principle in the reasoning that leads up to its abandonment. If M1 and M2 might both be good

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[8] See chapter 5 for a fuller discussion of the relationship between factualism and indeterminacy in justificatory matters.

methods in their own right, then we can use M1 to conclude that we shouldn't use M1. Though it is true that this would amount to an endorsement of M2, nevertheless it does not require our seeing M1 as having been undermined. Or rather, though we think M1 is undermined to the extent that we think we ought to use M2, we do not think it is undermined to the extent that we just think it outright incorrect. Nonfactualism allows that we might think M1 and M2 are both decent, even if we do have a slight preference for M2.

If this is correct, then it is not genuinely impossible to revise the basic principles of belief formation, at least not for the very general sort of reason that Katz gives. Or, put more positively, we have seen no convincing general reasons to believe that it is not logically possible to revise our basic rules of belief maintenance. Since Katz's reasons for claiming that this sort of revision is impossible depend on an inflated assumption about justification that empiricists have many reasons to shun, the impossibility is only illusory. Nonfactualists about justification can show how it might be possible to go from one method to another without thereby destroying the justification for doing so. Since nonfactualists tolerate a certain amount of indeterminacy in their judgments, they can explain how they might prefer one method while still not regarding a second method as totally incorrect. This explains how a nonfactualist can refute Katz's contention: the argument need not be unsound if there has never been a need to deny the truth (or correctness) of the premise.[9]

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[9] Here is where the details of my earlier concession, elaborated in footnote 3, becomes important. Katz's contention is that any argument for revising a basic rule of belief-revision would be unsound. But if the premise in question is simply a statement of the rule (and not a statement of its correctness), then this would put the focus too much on the factual status of rules: the truth which we would be denying would be the truth of the simple measure rule (or the principle of non-contradiction, or whichever rule was in

Earlier, I noted that reliabilism seems to also accrue the sort of indeterminacy which I'm emphasizing in my nonfactualist solution to Katz's paradox. This suggests that reliabilists, too, could avail themselves of the nonfactualist's solution to Katz's revisability paradox. What I take this to show is that most methods of belief revision which are more attractive to empiricists on independent grounds are able to handle the sort of criticism of the Quinean program which Katz has put forth. I will continue, in the rest of this section, to speak of the solution as available to nonfactualists, partly because I'm not sure how friendly most reliabilists would actually be to my suggestion. But I feel it is important to note that there is no principled reason that I can see why either of the more empirically-minded pictures of justification that have been suggested could not use the indeterminacy which their views accrue to a Quinean's advantage.[10]

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question). Since the issue here is clearly one concerning our inferential support, and therefore our justification of believing these various rules, we should focus, as I have done, on the rule's correctness, rather than its truth. Given this, there is another way to see the solution I'm offering to Katz's paradox: I'm denying that the premises which state our justification in using the basic rules of belief revision are truth-evaluable.

[10] I think that most philosophers of the non-naturalist variety see their views as avoiding indeterminacy. We will see this tendency at work when Boghossian argues that the non-factualist is open to charges of relativism in Chapter 5. However, speaking as someone who isn't at all attracted to non-naturalism about justification in the first place, I don't personally see why the non-naturalist couldn't just admit that there is some amount of indeterminacy in our claims about what is justified and what isn't. That is, there seems to be no theoretical reason to think that it must be rationally determinate whether one evidential system was better than another. Katz would certainly claim, I think, that non-naturalists should want to deny the vast majority of claims of indeterminacy. But his views also leave open the possibility that if there is no non question-begging way of specifying the fact in question, then the existence of the fact would be doubtful, at least in a different context from the one we are examining. For instance, he makes clear, regarding his own response to Quinean claims of indeterminacy of translation, that it must be non-question-begging ([1998] p. 91), and says of his argument, "Since the issue is whether there are intensions of sentences and whether they can be objectively determined, no question is begged in assuming natural languages have intensional structure for the sake of determining whether or not the assumption is true."([1998] p. 92) Whether or not Katz is right about this, it is clear that Katz is concerned to play from a level field. I take this concern of his to be a recognition of the possibility, at least in principle, that factualists might have to concede that factualism does have some burden of proof, contrary, it seems, to Boghossian's contention.

Though Katz's view seems to allow for the possibility of indeterminacy, I don't think he really thinks that much of it actually exists. His recent discussions have revolved mainly around mathematics and semantics, and he does not explicitly discuss the factualist question with regard to justification. However, he is quite clear that he thinks that logic is a subject which is treatable along the lines of his theory of

As I noted in the second chapter, Field offers many reasons for thinking that we should be nonfactualists about justification. What the previous considerations suggest is that empiricists, in particular, should be nonfactualists about justification. Doing so can make sense of the idea, so central to the empiricist's epistemology, that everything is, in principle, revisable for empirical reasons. For what we see now is that any argument like Katz's regarding the impossibility of Quine's view depends on a view of justification that will not be at all attractive to the empiricist and must, therefore, beg the question against her. Perhaps there are non-question-begging ways of showing that non-natural facts about belief systems are determinate: I can only await the arguments and address each one individually. But philosophers cannot insist that any argument to revise our basic belief-revising methods undermine themselves unless there is a (non-natural) fact of the matter regarding justification, as this is certainly not an independent, non-question-begging way of showing how the empiricist can't be right about epistemology. If the coherence of the empiricist's picture depends on one view of justification, and the criticism of the that picture depends on a different view of justification, then the critic of the empiricist must be seen as conducting the discussion in unfair terms. As long as there is some reasonable picture that can be made to coherently work as a whole, then the empiricist's epistemology can be saved.

I should add, just to be clear, that the empiricist could not use this as an argument for nonfactualism about justification. That is, she cannot insist that justification isn't factual, simply because she needs nonfactualism for her own views. In addition to the

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realistic rationalism. And if I am right in my diagnosis of his revisability paradox, then he clearly thinks there is no problem with claiming that there are facts about justification.

sorts of considerations that Field adduces in favor of nonfactualism about justification, the current point simply adds to its appeal for the empiricist. But this is not a defeat for factualism, nor do I regard it as one. Neither side here, I think, can claim the advantage, as far as the correctness of factualism over nonfactualism goes. I mean only to show that Katz's objections to the Quinean thesis of universal revisability itself depends on a view of justification which I am recommending that the Quinean reject. Since these objections are objections to the very epistemic possibility of Quine's thesis of universal revisability, they must show that there is no possible understanding of evidence assessment whereby it could be empirical. What I have just argued is that they cannot do this. There is a plausible perspective regarding justification, attractive on other grounds, and very much in keeping with the naturalistic methodology espoused by Quineans, which can make their view perfectly coherent.

## II) Exploring the Genuine Possibility of Revising Pragmatic Rules for Pragmatic Reasons

In the last section, I interpreted Katz's argument as an attempt to show that it is genuinely, or logically, impossible for us to revise pragmatic rules for pragmatic reasons. Nevertheless, Katz's concerns were very general. A full defense of empiricism should defend the genuine possibility of this sort of revision by at least attempting to show exactly how these sorts of revisions might go. That is, I have shown why Katz's very general worries about revising pragmatic rules for pragmatic reasons should not concern us, depending as they do on inflated assumptions about justification. But in order to

show that it is really genuinely possible to revise pragmatic rules for pragmatic reasons, the empiricist should say a bit more about how this might logically come about. The empiricist, one might say, needs to show how these revisions might come about, in their details. In this section, I will undertake this task.

To fully explore whether or not it is genuinely possible to revise pragmatic rules for pragmatic reason, then, we will have to see how we might actually come to revise these rules, in the details. That is, we want to be able to tell a good enough story about how this could come about that the possibility becomes logically tenable. In the first part of this section, I will explore some fictional examples of our revising pragmatic rules for pragmatic reasons. I will also show how these examples fare with respect to the two questions regarding the indeterminacy regarding correctness that I examined in the last section. There, recall, I showed how Field's example of indeterminacy of correctness could address the two worries I brought up, but lacking specific examples for my own case, I was unable to address these concerns for the issue at hand. In the second part of this section, I will explore the equivalent of Field's arguments for the a priority of logic for the a priority of pragmatic rules. That is, we saw in the last chapter that Field argues that inductive and deductive logic must be a priori. What I will do is explore whether or not the rules and principles of our basic pragmatic method must be a priori in a similar way. Finally, I will try to say something about the overall plausibility of my examples. I will argue that empiricists have reasons to be a little suspicious about the demand that they show, in great detail, how some revision is genuinely possible, at least when this demand is made in the context of how we might revise rules for empirical reasons.

a) Examples

The pragmatic method as we are conceiving it consists of the principle of noncontradiction in conjunction with several extra-evidential criteria, our focus being simplicity. Beginning with noncontradiction, then, our question is whether or not we can revise the principle of noncontradiction for pragmatic reasons. Consider the following bit of science fiction, from Graham Priest:

Revising Non-Contradiction, part 1:

Suppose sometime in the future we predict the mass of the collision of a large star with mass $x_1$ and a large planet with mass $y_1$ to be $x_1 + y_1$, but our prediction is greater than the actual amount, z, by an amount that is larger than we would expect as a result merely of experimental error. Some time later, we are similarly off in a different prediction involving very large objects with masses $x_2$ and $y_2$, and that the actual mass is again z. We cannot find a fault in either our instruments (otherwise massively trustworthy, we will suppose), or our calculations. We try to revise our theories about our measuring devices, but don't find any satisfactory way to do so. We hypothesize that some fundamental particle involved in our calculation of the masses, baryons, goes missing, or somehow is reduced in the collision, but our efforts to make sense of this are frustrated. Some bright young scientist named Einquine notices that the amount, z, is the same in both cases and hypothesizes that our arithmetic for counting baryons is wrong. She suggests that we use an inconsistent arithmetic for which z is the least inconsistent number. As new information comes in, she amends her hypothesis. She discovers that measurements of the two new fused objects taken from other parts of the galaxy are also both the same, though each location differs in its z, so she hypothesizes, consistently with the data, that values of z (including hers) vary according to the observers distance from the collisions. As this hypothesis is born out, she becomes more confident of her data, and begins to look into the mechanisms behind the effect on z related to distance.[11]

We might reconstruct Einquine's arguments something like this: Using a standard arithmetic whose logic is classical, we predict that the mass that results from a collision of large objects will be one number, and this prediction does not happen. Since we are predicting p, and not-p is the case, our belief set is contradictory. So we know that we must revise our beliefs. For various reasons, the most attractive candidate for revision is the principle of noncontradiction itself. So we should revise that principle, and begin using some non-standard arithmetic.

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[11] See Priest MS. According to Priest, one can model nonstandard arithmetics by simply dropping the consistency requirement. In these models, there is a tail T, (of numbers 0, 1,...n-1) which behaves consistently, but for n and every successor of n, the numbers form a cycle with a period, p, so that for any j $\geq$ n, j+ p = j. The models are characterized and individuated by T and p. (see pp. 7-10, sections 5 and 6)

As it stands, this story would only seem to show that we can revise noncontradiction for pragmatic reasons in the domain of baryons. Indeed, Priest makes clear that, in this case, revising our logic to get a nonstandard arithmetic would be a local revision.[12] This might be enough to show that the principle, as used in counting baryons, is revisable for empirical reasons, but it is not enough to show that the principle, as it is used in our most basic methods of belief revision, is revisable. Since the question we are concerned with now is whether it is genuinely possible for us to use the principle of noncontradiction in our methods so as to conclude that we should no longer use noncontradiction in our methods, we need an argument for why we might revise our pragmatic methods for the reasons adduced in those very methods.

Revising Non-Contradiction, part 2:

Suppose, then, that as we began to explore our universe, more and more evidence comes in that, with respect to particular domains, lead us to think that we should revise noncontradiction. If enough of it came in, and if the domains that called for this revision were fundamental enough, we could decide that we shouldn't use noncontradiction for the domain of our own inferential practices. What would an argument like this look like? Well, it might just be as simple as supposing that in one instance, we finally gather all our evidence together to decide to revise the principle itself. Suppose, then, that something like Priest's story does happen a lot, and that we find ourselves in a situation where it seems to be happening again. Let us call our evidence in this situation $E_{100}$, for the $100^{th}$ evidence we have that seems to suggest the revision of the principle of noncontradiction. We could say that our prediction, that some measurement would appear given classical reasoning, is refuted by evidence $E_{100}$. So we would have a contradiction. We could, as we have done previously, revise locally, and simply define the special assumptions that will be in operation for this new revised set of objects, as we have done for all the previous revised sets of objects. But, given that this is $E_{100}$, and we have these other 99 E's, we decide instead to revise the principle of noncontradiction in our own reasoning practices: that is, we decide, given the contradiction occasioned by $E_{100}$, to stop using the principle of noncontradiction at all in our reasoning. [13]

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[12] See Priest MS, p. 11

[13] Notice that I still construe this as a local revision, where the domain of discourse is our own reasoning practices. I suppose some people might want to say that since our reasoning practices are most basic, any revision of them should count as a global revision. After all, if I decide to revise the principle of non-

Is there a way this could really be simpler? In Chapter 3, we saw that the revision would need to simplify the physical theory. How could the above revision do that? Well, consider that this move would eliminate the need to define and implement 100 special assumptions which signal that noncontradiction is not true for some domain. This could indeed be simpler (by Harman's measure) if, i) we have some physical theory that explains exactly why each special assumption holds when it does, ii) we have some some physical theory which explains why non-contradiction doesn't work well in our reasoning about these new worlds, and iii) which also explains why noncontradiction appears to be a viable rule for our usual domain of objects. That is, it could be simpler if we had a theory about the worlds that made it simpler (I will return to this idea in the final part of this section).

So much for examples concerning noncontradiction. Let us see what we can say in favor of revising the principle of simplicity for pragmatic reasons.

Revising Simplicity:

Suppose that after some future scientific revolution, analogous to the conceptual revolution from classical to quantum physics, a future philosopher of science notices that at many junctures in history, many scientists chose not to believe some hypothesis for reasons of simplicity which turned out to be crucial hypotheses for the new theory. That is, that simplicity considerations tended away from what the philosopher of science currently regarded as the better theory. Our philosopher could reason as follows: Historically, simplicity dictated that we not believe $p_{t1}$ at time t1, $p_{t2}$ at time t2, and so on through $p_{t100}$ at time t100, but at time t101, we think we should believe each of these formerly disbelieved $p_{tn}$. For reasoners at t101, let us suppose there is a contradiction to be reckoned with (just as there was for those in the above embellishment of Priest. Further, suppose that for each tn, we can explain how we might have come to believe what we now regard as correct, but in each case, it would have involved our overriding some simplicity consideration. At each tn, then, we chose not to override simplicity

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contradiction because it doesn't work in the domain of my reasoning practices, then I am deciding to discontinue using simplicity considerations in my reasoning practices. So, the result of this local revision would be global. I consider this issue to be largely a matter of terminology, though it does lend itself to the idea that the whole distinction between global and local revisions might be problematic.

considerations. But now, at t101, facing a similar contradiction in our beliefs, we have a choice of what to do. We decide, based on the previous 100 times that simplicity considerations have gone astray, that we will not simply go with simplicity, as we have always done in the past. Rather, we will revise our belief that we should revise so as to minimize change in view. How might this be simpler? Looking over these 100 times, we can, for each one, give some explanation which shows how we might have come to believe what we now believe, but in each case, the explanation will be specific to the instance at hand. On the other hand, we could simply remove the simplicity consideration in our future inquiries, since that could show, in each case, how we might have arrived at the relevant belief had we not been guided by a simplicity rule.

Now that I've actually presented a few examples of revisions, my first challenge is to show how they support my claim in the first section of this chapter: that arguments to revise our basic principles of belief formation are not problematic for the sort of reasons that Katz gives. In all of the stories above, some rule or principle gets revised by a method which that rule or principle is a part of. Katz's very general problem with this is that any argument with this form would have to be unsound, since it would use a principle in the derivation of a conclusion whose truth undermined the truth of one of the premises.

My argument against Katz's claim was that since nonfactualism allows for what I called indeterminacy regarding correctness, two methods might both be, in some sense, good enough. So, focusing on my embellishment of Priest's argument, Revising Non-Contradiction, part 2, here is how it works: this argument does not undermine itself because our method when it included the principle of noncontradiction, M1, and our method after we have revised, M2, which doesn't include the principle, might both be good enough in many ways. When we acknowledge that evaluations concerning the correctness of methods don't have objective (or subjective, or whatever) facts to emulate,

then it becomes clear that it might be possible that two methods might both be good enough, in the way described above with respect to Field's example.

Earlier, I raised two questions about the cogency of the nonfactualist's claim regarding indeterminacy of correctness, and answered them on behalf of Field's example (which was, of course, the only example we had before us at the time). Let us ask both of these questions of the stories above. The first was, Why doesn't our choice of one method without a rule simply amount to a repudiation of a method that included the rule? Here, it seems there might be a problem for my stories. Recall that in Field's example, the very possibility of logic not being (strongly) a priori is somehow not available, whereas for anyone who reasons as I have recommended, there are no analogous barriers. The problem for my argument, then, might be that there is a relevant disanalogy between the two cases at hand. Field can explain why, in the case that he imagines, we need not negatively evaluate our former views: we need not because we do not think we are blameworthy for not thinking that logic is empirically indefeasible. In the cases that I am imagining, however, no such conceptual barriers exist: surely we know now that it is at least possible that simplicity considerations should not be given any credence. Likewise, the possibility of denying the principle of noncontradiction is certainly known, and even endorsed by some very smart people. More importantly, if what I am arguing in this chapter is correct, our pre-revision methods *are* able to revise their own rules, whereas this is precisely what is lacking in Field's example. It seems that in seeing that we should revise our beliefs, we need some reason not to see our former selves as blameworthy in some way. Field makes clear why that might happen in his example of indeterminacy

regarding correctness, but his reasons don't seem to apply to the examples that I have suggested.

My response is to concede that there is this disanalogy between Field's and my examples. That is to say, I concede that in the cases that I have imagined, a person who thinks that her former methods were in no way incorrect is not able to do this coherently simply because she didn't have the conceptual or theoretical resources to recognize her new methods as a possibility. But this doesn't preclude me from arguing that she might have other, perfectly good reasons, for not denigrating her former methods. In the cases that I have imagined, a reasoner might not think her former self blameworthy in using a system of belief revision which appealed to the principles of simplicity or noncontradiction because she didn't have the evidence against either before she switched methods. That is, let us assume that denying either of these principles has been a possibility all along. Just to be clear, this is to concede the disanalogy between Field's and my examples. Why would anyone finally decide to accept the conclusion? Well, I have argued that we would do this because we would finally have the evidence to convince us that simplicity considerations or the principle of non-contradiction are not successful (in the domains of our new theory).

In both Field's and my examples of what I have called indeterminacy regarding correctness, the imagined reasoner has a certain generosity towards his or her former self. Field seems to defend and make sense of this generosity by pointing out that in his example, the reasoner's former self couldn't have possibly been expected to recognize how he might have thought otherwise. I'm defending and making sense of my imagined reasoner's generosity in a slightly different, though no less defensible way: in the

examples that I explore, reasoners don't negatively evaluate their former selves because they didn't have the relevant empirical or theoretical data. Nor, of course, would having this data be a matter merely of thinking certain concepts through.

The second question asked earlier was, What considerations might lead us to think that two methods, one which includes a rule and one which doesn't, are both good enough? This question is asking why, having revised our methods, we might think that our pre-revision methods were, in some sense, still good enough. Notice that these two questions are related in having the same root question: why would someone bother endorsing the conclusion of any of the arguments suggested above if she thought that simplicity and noncontradiction were good enough? With simplicity this can be easily explained. As we have seen, Quine thought that simplicity was methodological, because we need some way of limning all the possible hypotheses we might appeal to, or all the possible beliefs we might revise. This is important in this context because it shows why we might still feel equivocal about simplicity. Consider Revising Simplicity. We can see the usefulness of rules like Harman's in contributing to the evolution of our beliefs, and so might still think that a system of belief revision that appeals to Harman's simple measure could be a very good one. Nevertheless, if we buy the story in Revising Simplicity, we will think a slightly better one should no longer take simplicity into consideration.

But what sort of reason might we have for thinking, once we have abandoned the principle of noncontradiction, that its use in a method of belief assessment was not really so terrible? Assuming that Katz is right that the principle of noncontradiction is constitutive of Quinean epistemology, the answer to this question is clear. In giving up

noncontradiction, we are giving up a way of conceiving of our most general epistemic methods. Whatever method we replace the old one with, it would, no doubt, be a radical conceptual departure from our old method, and would, in some sense, constitute a whole new method, rather than a slightly revised version of the old. In this way, revising the basic deductive principle of belief revision, that is the principle of noncontradiction, would be different than revising pragmatic principles like simplicity and conservativeness. But surely we can admit the reasonableness of different epistemological programs from our own, even if we prefer our own for various reasons. Though I think that the Quinean radical empiricism that I am defending is a preferable way of conceiving of our methods, I do not deny the reasonableness of the reliabilist program. So likewise, in the imagined case at hand, abandoning noncontradiction in favor of some methods that don't appeal to noncontradiction might well be preferable, but not in such a way as to rule out the reasonableness of other programs. If the question of which method we should use is not a fully factual matter, then we should not be quick to dismiss alternatives to our methodological programs when those alternatives have been and might still be successful.

It is worth noting, at this point, that the sort of considerations just adduced to explain the reasoning against noncontradiction could equally explain why we might abandon other basic Quinean principles. For instance, it could explain our abandoning modest empiricism by revising the principle which requires conservative revisions over the evidence, or Katz's where principle, by our revising the principle that everything is in principle revisable. So, focusing on the principle that we should take evidence into account, suppose that a seer came along whose abilities to just see truths about the world

were extremely keen. Suppose that this seer has expounded several entirely novel theories, which prove to be fruitful, and which lead to vast new scientific enterprises. My argument above makes room for the idea that we might choose to abandon our entire scientific method, and just follow this seer in whatever she says, for at least as long as our seer is alive (methods might not be merely topically local, but temporally local as well). Of course, we wouldn't have to see our former methods as unreasonable: in fact they always have been very good and will likely continue to be the best we have to offer after our seer dies. What this shows, I think, is that the empiricism being advocated here is what we might call, borrowing from Field, undogmatic empiricism. Future evidence might suggest that we shouldn't be empiricists anymore, but that doesn't make being a radical empiricist at all unreasonable, either now or in the future.

b) Field's Argument Revisited

Field's arguments in the last chapter were very particular: none of the best methods that we currently know of seem to allow for the revision of their most fundamental inductive and deductive rules and principles, at least not for empirical reasons. But there is also a more general point lurking in the background: that any decent method will face similar difficulties. This can be seen very clearly by considering a very general puzzle that Field develops for empiricism about basic evidential rules; indeed, concerning any empirical method. The puzzle, in two parts, explicitly concerns our inductive and perceptual methods. Part one is this:

> ...it would seem that we want our most basic inductive method to be reliable, and can investigate empirically whether it has been, and we will stop using it if we find that it has not. But in this case, the investigation of the most basic method can't be by another method, for by hypothesis none is more basic. Rather, the investigation of our most basic method uses that very method. So in the case where we empirically discover that method unreliable, the decision not to use the method would be based on that very method.

It goes on,

> Part Two of the puzzle says that the conclusion of Part One is incoherent. How can our method, in combination with evidence E (in this case, evidence of its own unreliability), tell us not to follow that very method? Our method presumably already tells us something about what is legitimate to believe and what is illegitimate to believe when our evidence includes E (say, when it consists of E & F).... Now if the method tells me that E undermines the method, it must tell me not to always do what the method tells me to do; in other words, it must tell me to do something different, on some evidence E & F, from what it tells me to do on E & F. (Field [ 2000] pp. 130-1)

Field's conclusion: "It would seem that only an inconsistent empirical method can allow itself to be undermined by empirical evidence of its own past unreliability."(ibid.) For a basic evidential rule to undermine itself, it would have to instruct reasoners to follow its rules in a way that goes against its rules. Though he only explicitly presents this challenge to inductive and perceptual methods, it is a potent challenge to anyone claiming that some method of evidence assessment can revise its own rules. Since that is precisely my claim, we need to see whether Field's argument for the a priority of deductive and inductive rules generalizes to the pragmatic rules that form the basis of the method that I am advocating.

We will begin by returning to Field's argument with respect to deductive rules. Field's argument, recall, was that, in point of fact, our best evidence assessing methods seem to require us to treat the deductive part of their logic as somehow fixed. So though we could be said to revise certain rules, we can't do it for empirical reasons as given by

those evidential systems. Whatever reasons we have for revising logic, they won't be because our evidential system leads us to that conclusion. The key to this problem lies in the fact that systems of evidence assessment have to treat certain deductive rules as sacrosanct. In the hypothetico-deductive case, these are the rules that derive the consequences that are the targets of the testing. The question before us, then, is whether or not pragmatic methods have to construe any of their rules as sacrosanct in a similar way.

How does Revising Non-Contradiction, parts 1 and 2, fare against the equivalent of Field's problem with deduction? At least as it gets used in part 1, noncontradiction is not deriving consequences from theories in a way that should interfere with its ability to undermine itself. As used in pragmatic arguments, as Katz himself points out, it is simply playing the role of signaling that we have a contradictory belief set, and some belief of ours must be reevaluated. (Indeed, it couldn't be playing its more formal role, as anything follows from a contradiction and the simplest activity, like going for cereal in the morning and not finding it, would allow us to infer anything we want.) But exactly which belief is the one that must give is determined non-deductively. So if the belief that must give is the belief which endorses the principle of noncontradiction itself, this is not because we can deductively determine that to be the belief that must give. As with the part 1 above, part 2 seems not to run afoul of any difficulty of the sort that worried Field about deductive arguments in the strictly evidential systems that he considered. For, again, noncontradiction is used in the above pragmatic argument as a signal for revision. It does not indicate one way or the other what should be concluded, and so it is used, you might say, in a non-deductive manner. I conclude that there is no problem similar to

Field's problem with revising deductive principles for strictly evidential reasons for revising the principle of noncontradiction for pragmatic reasons.

But what about our example of the extra-evidential criteria, simplicity? I will have a discussion in a moment of just how plausible Revising Simplicity is, but we can say this about it: there aren't any problems of the sort that Field imagines for revising deductive rules by our more strictly evidential systems. Simplicity rules are not used to derive consequences, they are used to order options according to a certain standard, and so if the simplest option is the one that denies the principle of simplicity, then simplicity considerations can "see" this. There is no reason to think, then, that the pragmatic method must treat its rules and principles in a way which precludes their own revision for pragmatic reasons. The pragmatic method is unlike the sort of methods that Field explicitly considers in this regard: for both evidential systems of a Bayesian and a hypothetico-deductivist sort, logical rules can not be revised because the evidence goes against them.

Let us next turn to Field's argument with regard to inductive rules. Field's puzzle for the empiricist, recall, is that our most basic rules are supposed to be the ones that guide us in our evidential systems and tell us what to believe, on the evidence. Given this, a basic inductive rule cannot conclude against itself. To do that, it would have to advise, that on certain evidence, the rule not be taken seriously. This puzzle is true of our most basic inductive rules because those rules take evidence into account: they say, no matter which way the evidence goes, how the rule should be adjusted accordingly. So in order to see evidence as undermining the rule, we would actually have to disregard the advice that the rule gives. The rule can't tell us to do this, on pain of being inconsistent.

Does anything like this problem arise for the principles of the pragmatic method that we have been considering?

Clearly, the principle of noncontradiction, being a deductive principle, is not self-correcting, so we can focus our attention on the question of whether simplicity considerations are.[14] Notice, that what Field is pointing out about inductive rules is something very specific: that they tell us specifically how to account for the evidence. The reason why evidence can't be adduced against basic inductive rules is that those rules tell us how to incorporate the evidence into the rule itself, thus yielding a rule which varies in its values. Rules of simplicity don't self correct in the way that inductive rules do. They don't take evidence into account. Of course, Revising Simplicity does suggest that a principle of simplicity might be self-correcting in some sense. But the sense in which it would be self-correcting is simply that it can determine itself to be the simplest principle to drop. The content of the rule does not adjust with the evidence, rather, simplicity criteria can show themselves to be the best candidate for revision. In the discussion of inductive rules above, I quote Field as saying that the rules need to agree that the evidence is evidence against them. Field rightly argues that inductive rules can't do this. But pragmatic rules, such as simplicity can agree that the evidence is stacked against them because they do not incorporate evidence. So I do not think that any problem analogous to Field's puzzle for inductive rules will arise for a simplicity principle.

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[14] Of course, one might think that using the principle of noncontradiction to conclude that we shouldn't use noncontradiction is inconsistent for the sorts of reasons Katz gives, but I have already addressed that worry. My only point now is that there would be no inconsistency arising from any self-correcting sort of reasons.

I conclude that the outlook for my view so far is promising: the pragmatic method can allow for its own principles to be revised for pragmatic reasons, at least, there are no problems of the sort that we have seen for logical principles and evidential methods. Neither Field's argument about the a priority of deductive rules, nor his argument about the a priority of inductive rules, seems to be applicable to the justificational status of pragmatic principles.

c) Genuine Possibility, in the details

In this section, I have given three examples of how we might revise pragmatic rules for pragmatic reasons. We have seen that these examples are genuinely, i.e., logically, possible at least in being able to address the sort of general concerns that would be at issue in addressing Katz's arguments. We have also seen that the examples do not have any problems of the sort that Field has raised for the possibility of their counterpart arguments regarding logical rules. One last issue, with respect to how genuinely possible these stories are, is whether they are good in enough, as I have been calling it, in their details.

What, then, does it mean to say that my examples are genuinely possible? Does Priest's story, what I called Revising Non-Contradiction, part 1, count as genuine or not? I would say yes. Priest describes the relevant scenario in enough detail to show that if we were faced with evidence like that, and if our other options for belief revision were unattractive enough, we might indeed consider revising our logic so as to arrive at some sort of inconsistent arithmetic. At the very least, I would argue that the burden of proof

here would be on those who didn't think that it represented a genuine possibility. What more could they want? Again, returning to Field's point about set theory, Priest has at least filled in his story enough to show us how the relevant contradiction arises, and why we might find ourselves unable to explain the contradiction clearly in other terms.

My embellishment in part 2, however, is a little fishier. In one sense, all I have done is to ask us to imagine many stories like the one Priest told, and to then imagine their cumulative effect on our thinking about our own methodologies. From this, one might think that if Priest's story is good enough in its details, then so is mine. But in another sense, my story is not as good in its details: I have said nothing to suggest why it might be simpler to abandon noncontradiction as a rule of our extra-evidential methods. Likewise, Revising Simplicity is, admittedly, pretty foggy on the details. I hope that I have given enough of a sketch to show that there is no logical reason to think that either Revising Non-Contradiction Part 2 or Revising Simplicity are not possible. But clearly, the more details that can be given, the better (which is why I gave so much in the way of detail in Chapter 3, when my intent was to show how revising the distributive rules might be simpler than the other options).

I will say this in my defense. The empiricist, I think, has reason to be suspicious of the demand to make every possible revision entirely clear, in its details. Revising Non-Contradiction part 2 and Revising Simplicity require that we, in some sense, will be able to understand what a method that doesn't use noncontradiction or simplicity would look like, but I really have no idea of the specifics. Perhaps one might think that I have shown that no reason yet given against their logical possibility is a good one, but that nevertheless, I haven't really shown how their possibility might come about. But one of

the claims of the empiricists, and particularly those who advocate the more modest methods that I do, is that sometimes, what makes an inference good or bad depends on empirical advances. If this is the case, then the details of the genuinely possible sometimes only come about as a result of an unimaginable possibility becoming actual. For example, showing how revising the distributive rule might be simpler, in the last chapter, was a matter of having specifics. Few philosophers, if any, would have been able to fill in the conceptual structure with enough details to explain the possibility of nondistributive superpositions before the evidence for their strange existence (and all the accompanying information that that evidence brings) came in.

This being the case, the whole idea that the empiricist must show how something might be genuinely possible looks a bit fishy from the empiricist's perspective, at least as it is used in the current context.[15] More to the point, the demand that I make the details of each case perfectly clear may be a demand that the empiricist has very good reasons to reject. The empiricist is committed to the idea that evidence could cause us to overturn cherished rules and principles, even very basic ones. But since, as we have seen, empiricism requires that theories fit the evidence in particular ways, it is quite clear that the empiricist is only claiming that we would do this when it was explanatory, i.e., when it gave specifics and made the evidence cohere into a plausible overall theory. Since, according to empiricists, it is empirical information which is the target of the fit, our not being able to say in advance what a method without noncontradiction was like would not

[15] It might still be acceptable as used in Field's example of the consistency of set theory since, for all that has been argued here, the consistency of set theory might still be a purely conceptual matter. Of course, many Quineans would disagree with this (see, for instance, Devitt [1998] and Resnik [1997]). If these defenders of Quine's program are correct, then the distinction would have to be fishy across the board.

really be much against the view in question. After all, this is what you would expect given the kind of evidence that would have to come in for us to revise noncontradiction.or a simplicity rule. It seems of the sort to be beyond imaginability.

So perhaps Revising Non-Contradiction Part 2 and Revising Simplicity are not terribly realistic, in their details. As I noted in the introduction, the current work is largely defensive. I am trying to show that there is a viable empiricist program for logic and reasoning more generally. Even if its true that I haven't shown that these stories are genuinely possible, in their details, I have shown this: that none of the specific charges that I can think to raise against these examples stick, and so we have no reason right now (other than our limited imaginations) to think that they are not possible in some way. That is, I have tried to show that Katz's reason for their impossibility are not very good, and that the equivalent of Field's reasons for their impossibility do not cause problems. I may not have shown them to be genuinely possible, but then again, no one has given any convincing reasons to think they are genuinely impossible, either.

## Conclusion

In this chapter, I have argued that there are no good reasons currently for thinking that we cannot revise pragmatic rules for pragmatic reasons. Katz's argument against this possibility was found to rest on an inflated assumption about justification; the equivalent of Field's arguments in Chapter 3 against deductive and inductive logic raised no problems for my claim; and at least one of the stories that I told of the possibility of this sort of revision seemed to be detailed enough to be called genuine. In addition, I tried to

argue of the stories that were not detailed enough that empiricists shouldn't feel obligated to be able to tell a detailed story about every possibility, since part of their claim is that empirical discoveries will provide the evidence on which explanations for genuineness depend. If my claims in this chapter are correct, then not only is logic revisable for modestly empirical reasons, but the rules which characterize these modestly empirical reasons are themselves revisable for modestly empirical reasons. This is important because, as we know, any method that wants to call itself empirical had better be able to be revisable by its own lights. Showing this of the pragmatic methods that I advocated in the last chapter has, I hope, made clear that modest empiricism about logic and our most basic methods of belief revision and formation is not left crippled by arguments like Field's and Katz's.

## Chapter 5: The Justification of Logic

One of the main points I made in chapter 3 is that the empiricist simply needs a notion of 'revisable' which makes clear that logic is no different than theoretical science. That is, logic gets to count as modestly empirical because logic is revisable for pragmatic reasons, and since no one denies that pragmatic reasons play a significant role in scientific inference, pragmatic reasons must, at least, be modestly empirical reasons. A related consideration which supports the idea that logic should be regarded as weakly empirical comes from considering the flip side of revisability: justification. To the extent that we think that we are justified in accepting the extra-evidential aspects of the physical theories that we accept, we are equally justified in accepting the evidential systems that we accept. And, most importantly, the reasons for feeling so justified will be the same: because the total theory and the evidence assessing system do the best job, in either case, of fitting our experience. Though there is certainly some mystery to the fact that theoretical physicists can do some math and come up with a theory which seems to work for the world, no one doubts that what justifies us in thinking that the theory might be true (or at the very least, might save the phenomena) is its ability to account for the evidence. The very same thing should apply to logic: though we can, in the abstract,

come up with various logics, they should be justified for us to the extent that they help us to organize the empirical evidence in a way which best helps us to reach our goals.

In this final chapter, I want to get a little clearer about what it means to say that a belief is justified for empirical reasons. In particular, having claimed that logic and pragmatic reasons are revisable for modestly empirical reasons, I want to reconsider the classic conundrum concerning logic's justifiability. It has long been thought that there is some problem with our justifying logic. Basically, the problem is that any justification will have to use logical principles, and so one would have to already be justified in using the logic in question in order to justify it. But I will argue that the claims of the last chapters should cause us to rethink this assessment.

To begin, I will explain in a little more detail exactly what the problem with justifying logic is supposed to be. After exploring the problem a bit, I will turn my attention to a recent article on the subject of justifying logic by Boghossian. In this paper, Boghossian examines the question of justifying logic in terms of the metaphysical positions regarding justification that we outlined in the second chapter. He makes several critical claims about both empiricism and nonfactualism about logic. In addition, he argues that his own position, which we have seen is a form of non-naturalism, can offer a plausible fix for the problem of justifying logic. I will examine each of these claims. What I will show is that his criticisms of nonfactualism are unconvincing. I will also compare what Boghossian's theory can do for the problem of justifying logic, with what a properly understood nonfactualism can do for the problem of justifying logic. I will argue that the nonfactualist can do at least as well as, if not slightly better than, the non-naturalist at justifying logic.

## I) The Justification of Deduction

The justification of deduction, like that of induction, is notoriously problematic. Hume famously argued that our use of induction is unjustified, propelled by mere custom or habit. He pointed out that if the only ways to justify something are deductively or inductively, then neither method is sufficient to justify induction: deductive justifications of induction give results that are too strong, making it the case that whenever the premises of an inductive argument are true, so is the conclusion, and inductive justifications of induction are circular. Haack [1976] has argued that the very same problems that hold for justifying induction also hold for justifying deduction. Like Hume, Haack argues that any inductive attempt to justify deduction would be too weak, making it the case that probably, when the premises were true, the conclusion would be true, and any deductive justification of deduction is circular. Haack considers several ways that one might attempt to defend the thesis that deductive methods can justify deduction, but argues that none of them are sufficient. Each of them falls prey to the charge of circularity, not, however, because they explicitly assume the conclusion in the premises. Rather, they are shown to be problematic because each of them would justify a non-valid rule of inference as well as they would a valid one (thus, it is thought, though not "straightforwardly question-begging", objectionable in a suspect way).[1]

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[1] See Haack [1976] pp. 79-80

Haack's example of this is the suggestion that we justify our use of modus ponens by demonstrating its validity by appeal to the truth table. The argument would go like this: Suppose that 'A' is true and that 'A ⊃ B' is true. But by the truth table for '⊃', if 'A' is true, and 'A ⊃ B' is true, then 'B' is true, too. So, 'B' must be true too. The argument is, therefore, valid and so it is further argued, justified. Though not explicitly circular ('B' does not appear as a premise), it does use the rule modus ponens to justify modus ponens, so it is rule-circular. For it says, in essence, If J ('A' is true and 'A ⊃ B' is true), and if J then K ('B' is true), then K, (and so 'B' is true). We can see that something must be amiss about it as a story about justification by seeing that an exactly analogous argument would show that the rule Modus Morons (MM), from 'B' and 'A ⊃ B' infer A, is justified. Here it is: Suppose that if 'A ⊃ B' is true, then 'B' is true. By the truth table for '⊃', if 'A' is true then, if 'A ⊃ B' is true, then 'B' is true. Therefore, 'A' is true. This argument uses MM to justify itself. For it says in essence, that if M (if 'A ⊃ B' is true, then 'B' is true), and if L ('A' is true) then M, then L. And protesting that MP is valid, whereas that MM is not, will not help here, as it is the justification conferring status of the validity of the argument that is precisely what we are trying to figure out.[2]

In a more recent article, Boghossian agrees with Haack that there is a problem, as he calls it, of "bad company". That is, rule-circular arguments can justify modus ponens, but they can also justify modus morons and Boghossian's favorite example of the kind of

[2] See Haack [1976] pp. 80-3. Of course, one big difference between MM and MP is that one can derive a contradiction from MM. This, of course, would be a good reason not to use MM. I only use this example (as does Haack) as a way of highlighting at least one of the problems with rule-circular justifications.

rule of inference you don't want being justified, the rules associated with Prior's connective 'tonk,' which were presented in Chapter 2. Of course, these inference rules allow one to infer anything from a belief, B, and so intuitively, it seems we would not be justified in using a method that employed the rules associated with the 'tonk' connective. To solve the problem of bad company we need a way to justify rule-circular arguments with modus ponens that doesn't also justify rule-circular arguments with modus morons or the 'tonk' rules.[3]

The difficulties with justifying deduction have given rise to various attempts to show that circular justifications could still serve some purpose. Michael Dummett [1973], for instance, argues that when we ask to be given a justification for some process, we are not asking to be persuaded that our method is correct: we know it is, we just want to know why it is. This should be construed more as a search for an explanation, and Dummett argues that there is no need whatsoever for explanation to be non-circular. Thus, though the justification of some deductive method could never convince someone who didn't already believe the method correct, it can serve to buttress the believer's faith

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[3] Boghossian also highlights a different problem with justifying logic, that of begging the question. The question gets begged because the skeptic doubts that the rule that you are trying to justify is justified at all, so assuming the rule in your justification won't do anything to sway the skeptic. That is, the skeptic has no reason to take MP as justificatory (in an argument for MP), if he doesn't think that MP is justified. As to the problem of begging the question, Boghossian ultimately concludes that there is no real response to the skeptic, and that we must admit that any sort of principle of the universal accessibility of reasons (whereby reasons can be seen to be reasons from any epistemic standpoint), is false. With respect to logic, at least, we will never be able to quell the skeptic. Thus, Boghossian distinguishes suasive and non-suasive reasons, and argues that our reasons for accepting logic, at least, are non-suasive: they could not quell a skeptics doubt.(See Boghossian [2000] pp. 251-3)

in its correctness.[4] But some version of the bad company problem will arise here as well: an explanation of MM which used MM would surely be as available as an explanation of MP that used MP.

The other famous response to the problem of justifying deduction is that given by Goodman. According to him, logic is justified when the rules that we do use come into reflective equilibrium with the rules that we ought to use.[5] But again, anyone delusional enough to think that using MM or the 'tonk' rules are good rules to use will not have any scruples, presumably, about actually using those rules. This, of course, would require that the person in question not care about things like consistency, or logically distinguishing the sentences of a language from eachother, but it is, I suppose, possible that this might happen. And so the possibility arises that the method of reflective equilibrium could justify these intuitively bad rules. There just has to another way.

## II) Boghossian's Arguments

We have just seen that the main problem with the justification of deduction is the so-called problem of bad company. In addition to his diagnosis of the problems with justifying logic, Boghossian's discussion consists of a consideration of each of the metaphysical positions examined in Chapter 2, and their relation to the problem as he diagnoses it. In this section, I will examine what he says about empiricism about logic and nonfactualism about logic; in the next, I will look at his arguments for his own

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[4] See Dummett [1973] p. 296

[5] See Goodman [1979] pp. 63-4

position, what we called non-naturalism about justification (conceptualism in particular). Though his comments on empiricism about logic are right enough, I think he is way off the mark in his understanding of nonfactualism about justification, and I will try to provide a better way to understand the view.

In "Truth by Convention", Quine famously argued that any justification of logic which appeals to logical concepts would be useless, and should not be seen as in any way vindicating the a priori status of logic.[6] According to Boghossian, Quine is right that if we accept a ban on rule-circular justifications of logical rules, then logic cannot be justified a priori. But what Boghossian does not see is why this fact drives Quine[7] to endorse empiricism about logic. After all, empiricism does not escape the circularity problem with justifying logic. According to the radical empiricists, as we have seen in previous chapters, we can compare the various possible belief sets we might have holding our physical theories fixed but varying our logical rules. As Boghossian rightly points out, however, any empiricist justification of logic will appeal to some logical principles, in that "...a large number of the core principles of logic will have to be used to select the logic that...is maximally justified by experience."(Boghossian [2000] p. 233) We have already seen one of Boghossian's examples of this in our discussion of Field's arguments against the empirical nature of pragmatic principles: The best set of observation sentences is the set with property F (say simplicity and conservativeness). Set O has property F. Therefore, O is best. (Boghossian [2000] p. 233, my paren.) Justifying logic,

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[6] See Quine [1936]

[7] And others: Boghossian's specific target here is more Harman, than Quine. See Boghossian [2000] p. 232. Boghossian here is responding to an argument in Harman [1996].

just like revising it for empirical reasons, requires logic, and this creates problems for our ability to critically evaluate logic. With respect to justifying logic, it creates the circularity problem and its attendant problem of bad company, which are as much a difficulty for empiricist accounts as they for any other.

In addition to showing how empiricism does not avoid these difficulties, Boghossian offers arguments against what he calls the nonfactualist's response to the problem of justifying logic. With his former point I am in agreement but the latter arguments, I think, are not quite as convincing. I think that both what he calls the nonfactualist response to the problem of justifying deduction, and his criticism of nonfactualism, completely miss the point of nonfactualism. According to Boghossian, the nonfactualist's response to the problem of justifying deduction is to deny that there is a problem at all. I suppose that Boghossian's thought here is that since there is no fact as to one system really being more reasonable than another, there is nothing here to justify or not. But this does not seem to me to be the best response on behalf of the nonfactualist, at all. The comparable position in ethics would be that since there are no ethical facts, we are not more justified in using one ethical system over another. Yet nonfactualists about ethics clearly do not endorse that thesis. According to nonfactualists about ethics, though there are no facts, there are our attitudes, and these, being all that there is, are enough to get normative discourse going. Similarly, nonfactualists about justification need not claim that nothing is more justified than anything else. Even if all there is is us, and our attitudes, according to nonfactualism that is enough for there to be evaluations regarding justification.

In Chapter 2, I noted that many have thought that nonfactualism can not adequately account for the normativity of evaluative judgments. I can now return to defend what was merely stipulated before: nonfactualist justification has normative force. I will do this by considering Boghossian's criticisms. Realists (factualists), according to Boghossian, can reasonably demand that someone who they disagree with about fundamental methods should give up their methods: since they think there is a fact, and their view is really in accordance with that fact, they are entitled to ask others to agree with them. Nonfactualists, on the other hand, do not have a similar entitlement. The reason for this is two-fold, according to Boghossian. In the first place, nonfactualists cannot explain what it is to accept a norm in an unconditional way. And in the second place, nonfactualists have no right to ask someone else to give up their fundamental norms.

What should the nonfactualist say about these complaints? Let us consider each point in turn. First complaint first. When Boghossian complains that the nonfactualist cannot make sense of unconditional acceptance, what he means is that the nonfactualist cannot escape evaluating the truth of a normative statement in terms of certain norms. For instance, Gibbard argues that normative statements are true in world-norm pairs, thus showing how the traditional arguments against nonfactualism can be overcome.[8] On this

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[8] The traditional arguments against nonfactualism that Gibbard focuses on solving are the problem of imbedded contexts, how normative content gets communicated from one context to another, and the problem of normative naivete. Gibbard's analysis has it that normative statements hold or don't hold in possible world norm pairs: more particularly, a normative statement S represents the set of all factual normative worlds$\langle w, n \rangle$, where $w$ is a possible world and $n$ is a complete system of norms. The problem of embedded contexts was how normative statements, which the nonfactualist denies to be factual, and so truth evaluable, can be understood in the context of inferences or conditionals. Gibbard's analysis makes clear how to give truth values to normative statements so that there is no special problem there. It solves the problem of communicating normative content because, according to Gibbard, the normative content of a statement is just all the world norm pairs in which the statement holds. It solves the problem of

account, normative statements are true in worlds if they follow from certain specified norms. That is, the truth of normative statements is conditional on the norms which govern that world. Boghossian seems to think it is obvious that accepting norms in an unconditional way is required for there to be normative force in our judgments. Why does he think this? His answer comes in the form of a question: "Should not an appropriate sensitivity to the fact that there is nothing that makes my norms more correct than anyone else's result in my being hesitant about accepting norms that apply to others regardless of whether they are also inclined to accept them?" (Boghossian, 2000, p. 244)

Well, no. As a nonfactualist, I believe that there are no facts which make me more right about some things. For instance, in the case of ethics, there is no thing that makes me more right than Osama Bin Laden. Nevertheless, I have no problem explaining why my views on war and justice are better than his, given my goals. The factualist thinks that I can't do this unless I believe there is a fact; unless I have some second order belief about what makes my first order belief true. But this surely is specious reasoning. A divine command theorist might easily argue along the same lines that I can't think my views are better than Osama Bin Laden's unless I believe that my views are commanded by God. In all honesty, I'd be perfectly happy for there to be normative facts, just like I'd be perfectly happy to really believe there was a divine deity who was going to grant me an everlasting afterlife. But just because I'd be happy to have those things, doesn't mean that they exist. I've seen every reason to be suspicious about accounts of moral facts and I've never seen a good reason to think that normative

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normative naivete, which was to explain how people can agree with normative statements even when they haven't fully worked out their norms, by noting that what the naif believes is simply the normative content which gets communicated. See Gibbard [1990] pp. 92-7

discourse requires anything more than us and our attitudes. Though I've argued that simplicity considerations might themselves be revisable, by our current methods, we should go by the principle that if we can get by with assuming less, we should.

The second complaint is that the nonfactualist has no right to ask someone who he disagrees with to give up his fundamental norms. Since the norms are fundamental, there is no neutral ground on which to make appeals. According to Boghossian, nonfactualists lack the ability to explain the basis upon which they could appeal to the person who they disagree with. Factualists, on the other hand, can coherently ask someone else to change their norms in favor of his own. Notice that the factualist may not have suasive reasons in trying to convince someone with whom he disagrees about fundamental methods of the correctness of his methods. Boghossian says, "...the point here is not about suasive effectiveness. I do not mean that the realist about justification will have an easier time persuading anyone of anything...The issue is rather about having the resources with which to think certain thoughts coherently. By virtue of believing that there are objective facts about what justifies what, the realist can coherently think that a particular epistemic system is mistaken. The non-factualist, however, cannot."(Boghossian [2000] p. 244) Nevertheless, the factualist is able to coherently demand that someone else revise their beliefs because he has facts on his side.

Boghossian's argument against nonfactualism seems to be that the lack of neutral ground impedes the nonfactualist's ability to demand that others change their fundamental norms. But why should this be so when the lack of suasive reasons doesn't impede the factualist? After all, suasive reasons are simply reasons that are commonly understood to be acceptable. For nonfactualists, the common understanding here must be

the neutral ground. The answer Boghossian gives, of course, is that, lacking suasive reasons, there is something else on which one bases one's claim to superiority. But this begs the question against nonfactualism in assuming that there is (or must be) something besides suasive reasons which could serve as neutral ground. For nonfactualists, neutral grounds and suasive reasons amount to the same thing. Nonfactualists don't think they need facts on their side to coherently make demands on other people. They only think they need what counts as good reasons (by their own accounting). Boghossian assumes that, lacking facts, the nonfactualist has nothing to offer in the absence of suasive reasons. But notice that the nonfactualist does think she's offering suasive reasons, even if the person she's trying to convince doesn't take them as such. Boghossian seems to be assuming that if reasons do not actually persuade the individual who one is disagreeing with, then they can no longer be regarded as suasive reasons. But no reasons can be suasive in that strong sense, as we have seen Boghossian himself acknowledge in the end of the paper when he concedes that one cannot quell skeptical doubts concerning some logic's justifiedness (see footnote 3 of this chapter). Nonfactualists think that they can be right even when the person they're arguing with doesn't agree because a reason's being suasive is not dependent on everyone accepting reasons as reasons.

The bottom line with both of these criticisms is that they beg the question against the nonfactualist by assuming the existence of an explanandum that the nonfactualist doesn't think needs to be explained. In the first criticism, the nonfactualist is supposed to explain how a theory can be unconditionally correct. But a nonfactualist doesn't think there is a valid sense in which a theory is unconditionally correct. In the second, the nonfactualist is asked to explain what there is beyond persuasion (when persuasion

doesn't work), when one does not share fundamental norms with someone else. But the nonfactualist doesn't think there's anything much to be done in these sorts of situations, and the factualist (in spite of insisting that facts let him coherently say he's right but not the nonfactualist) doesn't either, as is evidenced by Boghossian's concessions about non-suasive reasons.

The summation of Boghossian's criticism of nonfactualism about justification is that it engenders an intolerable relativism. The upshot of the problem for nonfactualists, according to Boghossian, is that they can not say that a belief is unjustified, "...no matter how crazy it may be, so long as that belief is grounded in a set of fundamental epistemic norms that permit it, no matter how crazy they may be."(Boghossian [2000] p. 244) As long as a belief follows legitimately from some set of norms, no matter how crazy they may be, the belief must be justified, for the nonfactualist. Not surprisingly, I do not agree with this general assessment. Forgetting me and Osama Bin Laden, why must I agree that reading chicken innards is as good a method of prediction as my own, as long as the belief produced by the reading follows legitimately from whatever norms govern chicken innard reading? In chapter 3, I argued for a version of empiricism which is conservative over the evidence, and the method of chicken innard reading, I am assuming, is provably not that. According to Boghossian, as a nonfactualist, I can value methods that try to revise conservatively over the evidence, but I cannot say that a method that doesn't do so is not acceptable. But why can't I say this is unacceptable by my norms? Clearly I can. And, as Field notes, whose other norms should I care about their being acceptable to?[9] If

[9] Well, there is probably an answer to that question: I care about my norms being acceptable to the sort of people who I think share similar norms. To the point that if someone whose opinion I really value thinks

you think that beliefs should be revised in a way that is conservative over the evidence, then you probably should think that reading chicken innards is a bad idea. There is that conditional acceptance again, but if what I said above is correct, then Boghossian's point against conditional acceptance begged the question. The nonfactualist thinks this is good enough: at least, it better be since it is all there is.

Nonfactualism (evaluativism) does allow for a certain amount of moderate relativism. Regarding the scenario noted in connection with the question of whether reliabilism allows for indeterminacy, in Chapter 3, Field says about nonfactualism:

> So we recognize that a slight modification of our goals—an increase in the relative value of reliability over power—would lead to a preference for the other system, and we regard the alternative goals as well within the bounds of acceptability. Consequently we make no very strong claims for the preferability of our system over the alternative: the alternative is slightly less good than ours given our precise goals, but slightly better on alternative goals that are by no means beyond the pale. 'Relativism' in this weak sense seems to me an eminently attractive position.(Field [2000] p. 141)

Indeed, relativism of the sort that Field alludes to in the quote above is just a matter of nonfactualist's comfort with indeterminacy regarding correctness, and so presumably, the examples of this sort of indeterminacy that I appeal to are also examples of nonfactualism's relativistic bent. What Field denies, and rightly, is that he is thereby committed to the sort of "extreme" relativism which Boghossian is accusing the nonfactualist of.[10] In the next section, we will consider just how much relativism the nonfactualist is committed to, and compare it with Boghossian's own preferred theory.

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otherwise about some normative issue, I'll consider their position very carefully and perhaps make it my own.

[10] See Field [2000] pp. 139-144

## III) Solving the Problem of Justifying Deduction

We have seen that, according to Boghossian, one of the main problems with circular justifications is what he calls the problem of "bad company": just about any old terrible rule of inference could justify itself. According to Boghossian, only one metaphysical position vis-à-vis justification is able to solve this problem, and it is his non-naturalism. In this section, I will consider Boghossian's solution and try to show how it compares to my empiricist solution, as also explained in this section.

Boghossian's solution to the problem of justifying logic has two parts. In the first place, he argues for the claim (L): "If M is a genuinely meaning constituting rule for S, then S is entitled to infer according to M, independently of having supplied an explicit justification for M." (Boghossian, 2000, p. 249) His argument is that the only way to raise the question of explicit justification requires an implicit use of the rules in question. That is, since, we can only argue about the justification of a rule if we have understood the rule, and since understanding a rule implicitly requires use of the same rule, we could be forgiven for the entitlement before the justification. After all, we would have no choice in this particular area.[11] His second move is to restrict rule-circular justifications by the following principle (RC): "S's rule-circular argument for a rule of inference M will confer warrant on S's belief that M is truth-preserving, provided that M is a

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[11] I noted earlier that Boghossian agreed with Quine that if we accept a ban on rule-circular justifications, then logic can't be justified a priori. We can now see that his solution to the problem is, in part, to deny the antecedent.

genuinely meaning constituting rule for S."(Boghossian [2000] p. 250) We saw above that we needed some way of justifying the good rules, like modus ponens, while not providing any justification for the bad rules, like modus morons, or the rules associated with 'tonk.' The 'tonk' rules will not justify 'tonk', according to Boghossian, because the rules for 'tonk' are not meaning constituting. As he says, "It is readily shown, by attempting to construct a truth-table for 'tonk' that its introduction and elimination rules do not determine a meaning for it; there is no proposition expressed by sentences of the form 'A tonk B.'"(Boghossian [2000] p. 251) I suppose similar arguments would be true against modus morons. I think Haack is quite right to point out the dubiousness of appeals to truth tables when the discussion is justification.[12] But whatever the strengths of these arguments, Boghossian claims that his nonnaturalism can rule these bad rules out, while justifying the good rules like modus ponens.

Earlier in the chapter, I agreed with Boghossian that since empiricism has the same difficulty justifying logic that every other view does, empiricists will have to address the problem of bad company. I would argue, however, that empiricism too could rule out these bad rules of inference: these are just not going to be very evidentially fruitful rules for us to use. Consider how someone who endorses a weakly empiricist method will evaluate the use of the 'tonk' rules in an evidential system. Since 'tonk'

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[12] See Haack [1985] p. 82 She notes that appealing to truth tables does no good because one can always justify the truth table rule by using the rule in question, as was done above in the text with MM. More generally, any appeal to truth tables and meanings seems to simply reassert the problem at hand but in a different name. For we are always free to ask why we should feel justified in assigning exactly those particular truth table values to a connective, or why we should feel justified in taking the meaning to be one set of truth values rather than another.

takes one from true premises to false conclusions, use of 'tonk' is bound to result in a number of failed predictions and averted actions. For instance, from my belief that I am now in Brooklyn I can infer that I am in Brooklyn tonk I can fly, and from my belief that I am in Brooklyn tonk I can fly, I can infer I can fly. But I also have a great deal of evidence which tells me that I can't fly. This evidence also tells me that if I were to act on my above inference and try to fly, I'll discover a particularly painful example of the uselessness of the rules associated with 'tonk' (if I survive the 11 story drop out my apartment window). Similarly, I'm sure, for modus morons. If this is true, then, at the very least, empiricism about logic is in no worse shape than non-naturalism about logic, when it comes to ruling out counter intuitive rules of inference, like those that constitute 'tonk', and modus morons.

And in fact, I would argue that it's in better shape. For surely what bothers us about circular justifications, and what lies at the root of Boghossian's bad company problem, is the fact that, as Field has pointed out, circular justifications don't seem to take any risks. Rule-circular arguments don't feel like they've really proven anything because they assume the correctness of what they are trying to justify and so it's hard to see how they could fail. However, as soon as you grant that deductive and inductive rules and principles are revisable for pragmatic reasons, you grant that pragmatic evaluations of deductive rules take risks in some sense. The question is in what sense. The risk is not of direct empirical refutation, as Field has shown that this is not possible. But they do risk at least this: that they might be revised as a way of making sense of some new evidence. That is, they risk being revised for modestly empirical reasons. If empiricism about logic is right, then though the justifications of logical rules themselves

may not take risks, in being weakly empirically defeasible, the logical principles which make up those justifications do take risks, and can in principle be overturned if we think that alternative principles might do a slightly better job.

That conclusions regarding the revisability of logic should have repercussions for the justification of logic should come as no surprise, given the connection between justification and revisability delineated in the first chapter. There, I argued that confirmation is simply the adding to and defeating is simply the taking away of justification, more generally. When thought of in this way, it is clear that the problem of revising logic for empirical reasons is mirrored by the problem of justifying logic. After all, the very same fact about logic is at issue: that in order to be doing anything that is evaluative, you must assume some logic. This is a fundamental fact about logic, and it is, what makes logic different than everything else. As Field points out, one needs logic to reason about everything. This is certainly true of our reasoning about geometry, which is the reason Field mentions this fact about logic in the first place.[13] But it is equally true of our reasoning about logic and evidential systems more generally. It is this fact that makes difficulties for justifying logic, and it is also what makes difficulties for revising it.[14] In the former case, the problem is that the very act of justification requires our prior

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[13] When discussing the disanalogy between logic and geometry, and hence why logic might still be a priori even though logic can not be, Field says, "...one needs logic to reason about anything (including geometry). By contrast, there are lots of things (logic is one of them) that one doesn't need geometry in reasoning about; this asymmetry is certainly part of the explanation of why there is no obvious alternative to strong a priorism in logic." (Field [1996] p. 369) By strong a priorism Field means what I called conception (W) in the first chapter.

[14] It is also this fact that makes difficulties, in another way, with nonfactualism about logic. To return to the brief discussion of Gibbard above, the truth of normative statements depends on certain statements following from various specified norms. But this notion of 'follows from' seems to be logical. Indeed, Gibbard says, "Any particular normative judgment *holds* or not, as a matter of logic, *in* the factual-normative world $\langle w, n \rangle$."(Gibbard [1990] p. 95) Its hard to see how the nonfactualist can avoid falling

acquiescence to at least some logical principles, and in the latter, the problem is that logic plays a constitutive role in our evidential machinery such that we can't but treat it as a priori. If what I've argued in the last few chapters is correct, then it would be surprising if the problem with justifying logic was left untouched.

We saw in the second chapter that for non-naturalists about justification like Boghossian, appeal to a rule's being truth preserving will do nothing to help us in deciding between the rules of classical logical and the rules of intuitionist logic, since these latter are equally truth preserving. If what I've just said is correct, then empiricism actually seems to do a little bit better than non-naturalism in ruling out rival logics. Empiricism might distinguish, for instance, quantum from classical logic, and so, at least, empiricism about logic has the chance to rule out more alternative logics than does Boghossian's conceptualism (since the rules of quantum logic are also truth preserving).

With this in mind, we can see that, in theory, Boghossian's favored view suffers more indeterminacy than does the empiricist's. At least taking only the resources of being genuinely meaning constituting, and so truth preserving, as our guides, it is in principle indeterminate which logical rules are more justified. Any other resources for ruling various deviant logics will have to come from outside the specific field of the justification of logic. But the empiricist can, in principle, revise logical rules for pragmatic reasons. And since pragmatic reasons are modestly empirical reasons, the empiricist can revise logical rules for empirical reasons. But this means that logical rules do take risks. Since most philosophers and physicists seem to have decided not to revise

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prey to the sort of objections that Quine raised against conventionalism in "Truth by Convention." See Boghossian [2000] pp 236-8 for a development of this criticism against nonfactualism about logic.

classical logic in favor of quantum logic, we can, at least, say this: we could, once, have revised our logic, but having chosen not to, our logic can be seen as more justified. That, after all, is about as close to "testing" rules and subjecting them to risk, as we are likely to get.

I am getting a fair bit of mileage out of my modest empiricism, and someone might worry that I am, in some sense, violating my own nonfactualism. After all, what I seemed to argue above is that really bad rules like those associated with 'tonk' will not bear up to the evidence, and this seems to suggest that there is some fact which those rules are lacking. I have two things to say to this worry. In the first place, notice that, speaking very generally, modest empiricism is wholly compatible with nonfactualism because empirical evidence, as we have seen, is compatible with many alternative explanations and hypotheses. One could easily value one's evidence base while still believing that there is no fact which makes one hypothesis or theory or method better than another hypothesis or theory or method. As Field has pointed out, there is bound to be ample indeterminacy, not only in determining whether some hypothesis or theory or method actually does a good job (as opposed to an okay job) at achieving some goal, but also in the goals themselves, and their relative valuations.

But in the second place, and more specifically, the rules governing 'tonk' are lacking, and it's as easy for a nonfactualist to take note of this as a factualist. Just as with ethics, the problem with the 'tonk' rules can be formulated in terms of our goals with respect to the use of logic in our evidential systems: for instance, these rules justify everything and so do not logically distinguish sentences in a language from each other. The nonfactualist can say that if one values being conservative over the evidence then

'tonk' is probably not the best connective for one to use. The nonfactualist can even adduce what he takes to be pretty good reasons for thinking this. It would justify every thing with out distinction as to the evidence. The nonfactualist can do all this, but in doing so, she cannot make any reference to any sort of fact about justification, such as 'tonk''s rules being unreliable, or their not being genuinely meaning constituting. In sum, nonfactualists can't appeal to facts, but they can appeal to goal oriented reasons (though they must admit that those reasons might not always point determinately towards one method or rule over another).

Field, as we have seen, thinks that logical rules might be revisable for pragmatic reasons. Since, however, he does not think that pragmatic reasons for revision are at all empirical, he does not believe that these rules take enough risks in revision to warrant our thinking of them as justified in any strong sense of the term. According to Field, logic is justified by default. "To call a proposition or rule default reasonable is to hold that it is reasonable to believe or employ it without first adducing evidence or arguments in its favour."(Field [2000] p. 124) Very early on, in Chapter 1, we saw the reason for this: that logic is weakly a priori (and so default reasonable) is beyond doubt. For how could logic not be justified in the absence of empirical reasons, since we would have to use logic to see empirical reasons as *reasons*? This makes logical rules "default reasonable."

Not surprisingly, my view on justification responds to Field's view on justification, a lot like my view on revision did to Field's view on revision. That logic is default justified in the above sense is inarguable: we must get started, and whatever rules those are that we begin with, those simply cannot, themselves, be justified. Nevertheless, there is a sense of justification which is more modest, whereby justification comes about

as a matter of being exposed to risks. Of course, the sense in which logic is justified on my account is still fairly weak: though logical rules take risks, they don't take very many of them, and we seem to do just about anything we can to revise other things first, which is probably as it should be. Weak though they may be, these risks do give us something, and this is enough to think that logical rules at least can be justified for the empiricist. Justified in what sense? In the following: that they are the best rules that we can come up with for basing our own methodologies on, and they haven't yet been defeated for any reason.

This leaves the question how my view differs from Field's. According to both of us, after all, we have a defeasible justification for logical rules based on their being the best rules for evaluating evidence that we can currently come up with. Is there any difference between Field's default justification and my weak justification? There is: according to Field, though logic is default justified, and may even be overturned as a result of certain experiences we might have, it cannot be revised for what should count as empirical reasons. Whereas for my view, logic can be revised for what should count as empirical reasons. On both Field's view of logic and my view of logic, logical rules are provisionally accepted. But whereas Field doesn't think that logical rules can be defeated for pragmatic reasons, or that if they can, this should count as empirical, I do (on both counts). My view differs from Field's, with respect to justification, in that I think that a logic's overall fit with our physical theories constitutes a good reason for thinking it's a good logic to use. Field, on the other hand, thinks that the question of fitting logic and our physical theories makes no sense. Though I have conceded that it doesn't make sense if that fit is the strictly evidential one that Field investigated, I have tried to make clear

that there is another sense of fit on which we can make sense of this idea. And this notion of fit is sufficient to give us some sense in which logic might be thought of as both revised for empirical reasons and justified for empirical reasons.

## IV) The Justification of the Pragmatic Method?

We have seen, that in addition to deduction and induction, there are also pragmatic rules which govern scientific inference. To my knowledge, no one has ever considered the question of whether or not attempts to justify the pragmatic method face similar difficulties. What, then, can we say about this question? Clearly, the pragmatic method can't be justifed deductively. How about inductively? I guess this justification would aim to show that uses of the pragmatic method lead to successful theories. While I'm sure this is quite true, I'm equally sure that use of pragmatic methods leads quite often to unsuccessful theories, and I don't think I'd want to bet on what I'm guessing is a fairly dismal track record. So this leaves the pragmatic method itself. Here, the problem would seem to arise just as it did above with deduction and induction: any pragmatic justification of pragmatic method would seem to have to presuppose what it was supposedly proving.

Except that we have seen that pragmatic methods, unlike deductive and inductive methods, can revise their own constitutive rules and principles. Or I should say, we have seen no specific good reason to think that they can't revise their own constitutive rules and principles. But if this is true, then this is very relevant to the problem of pragmatically justifying the pragmatic method. Since the pragmatic method can revise its

own rules and principles, it can also justify its own rules and principles. As long as those rules are in some sense open to revision for empirical reasons, then they are always, in some sense, at risk. The risk, of course, is very small, as it would be in keeping with the modest nature of the empiricism being explored here. But nonetheless it is something.

## Conclusion

In this chapter, I have tried to show in what sense logic can be justified for the empiricist. I have argued that the problem with justifying logic can be alleviated for the empiricist to the extent that logical rules and principles might be revisable: in allowing that these rules can be revised for pragmatic reasons, my claim is that these rules are exposed to some risk, however minimal, and this risk is sufficient to give some sense to our feeling justified in using the rules that we do. I have argued that empiricism does better at justifying logic than does Boghossian's conceptualism, and I have tried to show that Boghossian's criticisms of nonfactualism are misplaced. In particular I have tried to explain how it is that in spite of allowing some indeterminacy regarding epistemic correctness, the nonfactualist is no rampant relativist, and can rule out obviously bad logical rules like MM or tonk. If what I have said is correct, then the rules of logic are as justified for us as our best scientific theories: they are the best way we can think of, now, for weaving together the evidence of our senses into a plausible and understandable story.

## Conclusion from the History of Philosophy: Carnap and Quine

In this work, I have argued for several claims. In the first place, I have tried to show that there are two ways that a warrant might be thought to be empirically defeasible (or indefeasible), a strong and a modest one. I have also tried to show that though logic might not be strongly empirically defeasible it is modestly empirically defeasible. Thus, I have argued that logic might be both modestly empirical but a priori. I have defended my notion of modest empiricism on many grounds. I have tried to show that it is sufficiently guided by evidential concerns to be called "empirical", and I have tried to show that it, itself, is modestly empirical, as one would expect of an empiricist theory. In addition, I have argued that empiricists about logic can make at least some sense of how we might justify logic. Overall, my conclusion is that empiricism about logic in my modest sense is a coherent position. Thus, though Field may be right that logic is not strongly empirical, it is at least modestly empirical, and this, I claim, is enough to have genuine empiricist teeth.

To conclude, I want to switch gears and consider how my conclusions fit in with one particular debate in the history of philosophy: Quine and Carnap on the line between internal and external questions. Logical positivism was marked, in part, by an attempt to

distinguish real philosophical issues from nonsensical ones. Certain philosophical questions, e.g., what is good, what exists, how can we be sure of the existence of the external world, were deemed devoid of cognitive content, and were accordingly denigrated as mere pseudo-questions. One contribution to this effort was Rudolf Carnap's famous paper, "Empiricism, Semantics, and Ontology." [1950] The main topic of this paper was metaphysics: in particular, it concerned the question of what, ultimately, existed. Was the world made of objects or of sense data? Were numbers abstract objects or nominalized particulars? Carnap tried to dismiss these sorts of questions by distinguishing internal from external questions. Internal questions concerned the existence of certain sorts of entities from within a linguistic framework. External questions concern the adoption of one linguistic framework over another. Linguistic frameworks, according to Carnap, embody our ways of thinking and speaking about the world: the linguistic framework which adopts the thing language (as opposed to the sense data language), takes it as a matter of meaning that the world is made of spatio-temporal objects. From within this framework, the answer to the question, Is there a chair in front of me?, is a purely empirical matter, answerable in principle by empirical investigation. However, trying to discover whether there is really a chair there, in some deeper metaphysical sense, is an external question and is not investigable, either empirically or otherwise.

In this paper, Carnap argued that questions about which logic we should use were external questions, as opposed to internal ones. This meant that questions of which logic we should use were to be decided by simply choosing one language over another. In particular, it was a matter of choosing which L-rules or analytic sentences we wanted to

use. Its not that L-rules were unrevisable, but they were not revisable from within the linguistic framework. We could decide to revise our logic, but that would mean simply switching to a new linguistic framework. Decisions regarding which linguistic framework we could use were, in some sense, not rational: at the very least, they were not subject to any sort of empirical or evidential considerations. It was in response to this last claim in particular that Quine argued, in "Ontological Relativity" [1969] that there was no clear line between internal and external questions. According to Quine's famous web of belief, questions of logic and questions of science were conceived as continuous with eachother. No belief, even logic, was in principle unrevisable, and the difference between science and logic was not a matter of kind, but of degree.

Recently, Michael Friedman has claimed that Carnap (and Reichenbach before him) were able to articulate a notion of a priority which made sense, even given Duhemnian holism and Quine's thesis of universal revisability. Friedman calls it the relativized a priori. This notion clearly bears similarities to the one offered by Field. Friedman notes that, given this, Quine's response to Carnap is, in a sense, very puzzling. As Friedman points out, Carnap endorsed the Duhemian thesis that we test theories, not hypothesis by hypothesis, but as a whole. And since both L-rules and non-logical framework rules, P-rules, were as revisable for Carnap as they were for Quine, its sort of hard to see what Quine thought he was being critical of. Considering exactly what Quine might have meant, Friedman says,

> ...Carnap is able to accept the basic idea lying behind Duhemian holism without dissolving the desired distinction between conventional and factual, constitutive and empirical. And this shows, it seems to me, that Duhemian holism and the general revisability of all theoretical principles are together quite insufficient to

> yield the collapse of the empirical/nonempirical distinction and thus Quinean holism.(Friedman [1999] p. 72)

Or put in other words, since Carnap already agrees with both the principles of Duhemian Holism, and the idea of universal revisability, what was Quine was complaining about?

If what I have argued in the previous chapters is correct, Field may be right that logic is a priori in the sense that he claims, but logic is, nonetheless, still empirically defeasible in the more modest sense that I have been advocating. It is this sense, I think, that Quine meant as the corrective to Carnap. Quine and Carnap can, if my arguments are correct, both be right. There is a sense of the a priori in which logic is a priori. But to the extent that we think that science is our paradigm of empirical pursuits, logic and science are continuous in many ways. In particular, their status with respect to our methods, more realistically conceived, are very hard to distinguish from each other. Similarly, Field and I can both be right. Just as I can't show that Field's argument for the a priority of logic in his sense is wrong, so I hope that Field can't show that there's anything incoherent in the view put forward in the current work.

How could we both be right? Where Field and I primarily disagree is in what standard we hold empirical reasons for revision to: Field thinks they must proceed according to one of our more strictly evidential methods, whereas I think that sometimes, they legitimately extend beyond these methods. But for this disagreement, however, my views are obviously in close agreement with Field's. And though I do think my own side here is preferable, I also see what motivates Field: there are reasons for wanting to try to delineate a class of beliefs that are more directly responsive to evidence. I just don't think we should find this class to be the only one of interest, as empiricists. More

particularly, when such important beliefs in science go beyond this class, I think it is reasonable to also examine their justificational status, and see what we can say about their relative empiricality or a priority.

So, I hope that I have shown that empiricism about logic makes sense. In spite of Field's arguments that logic is a priori, there is a view which is credibly empiricist that makes clear how logic, and our reasoning more generally, can be conceived as being tied to evidence. This modest sense of empiricism can live happily side by side Field's minimal sense of a priorism, because in essence, logic is both. Logic is unusual in that it is constitutive of evaluation in general, but this doesn't mean that it is completely beyond the reach of evidence.

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