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Can good science be logically inconsistent?

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Abstract Some philosophers have recently argued that contrary to the traditional view, good scientific theories can in fact be logically inconsistent. The literature is now full of case-studies that are taken to support this claim. I will argue however that as of yet no-one has managed to articulate a philosophically interesting view about the role of logically inconsistent theories in science that genuinely goes against tradition, is plausibly true, and is supported by any of the case studies usually given.

Keywords Inconsistency · Scientific theories · Philosophy of science · Justification

1 Introduction

According to a traditional way of thinking about scientific theories, one of the worst fates that can befall a theory is the discovery that it is logically inconsistent. Because it is impossible for all the elements of a logically inconsistent set of sentences to be true, we do not even need to do any experiments to know that a logically inconsistent theory is false. For this reason (amongst others), logical inconsistency has typically been regarded as the kiss of death for a theory.

This way of thinking about inconsistency was largely taken for granted by early philosophers of science. Popper wrote 'a self-contradictory system must be rejected because it is false', and 'a self-contradictory system is uninformative . . . because any conclusion we please can be derived from it' (see Popper 2002, p.72). Hempel wrote 'logical consistency is called for, because an inconsistent theory . . . tells us nothing about the world. (see Hempel and Jeffrey 2000, p. 79).

In the last handful of decades, however, a counter-tradition has emerged that has been suspicious of many of the traditional philosophical claims about how science is

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and ought to be done. Many philosophers in this counter-tradition have argued *against* the idea that logically inconsistent theories must always be rejected. They have pointed out that many actual scientific theories are logically inconsistent, and that in spite of this scientists have no problem going about their work, making good predictions and sending rockets to the moon. They have noted that scientists are somehow able to avoid the logical anarchy that is supposed to come along with inconsistency, and have claimed that fear of inconsistency is more a neurosis of the philosopher than a real concern for the working scientist.

To support this way of thinking, three types of example of inconsistency in science are typically given.[1] First, there are inconsistencies between theory and observation. Classical mechanics was known to predict a value for the perehilion of Mercury at odds with observation, and yet was not immediately abandoned as a result. Toleration of 'Kuhnian anomalies' is claimed to amount to toleration of logical inconsistency (see Priest 2002, p. 122). Second, there are inconsistencies between different theories. One type of example revolves around the fact that the same system can be described in incompatible ways in different theoretical frameworks. For instance, sometimes physicists will describe a liquid as a continuous distribution of matter (drawing on continuum mechanics), while at other times they will describe a liquid as a large, finite set of point particles in motion (drawing on kinetic theory).[2] The fact that physicists are happy to use both descriptions shows a certain toleration of logical inconsistency, it is claimed. A different type of example of inconsistency between different theories involves deeply entrenched theories from different sciences making incompatible claims, as in the late nineteenth century when physics and geology gave very different estimates of the age of the earth. Third, there are cases in which a single theory is logically inconsistent. One source of examples is the way in which physicists invoked incompatible classical and quantum principles in the early twentieth century. Here, Bohr's treatment of the atom (see Lakatos 1970) and Planck's treatment of blackbody radiation (see Norton 1987) are two popular examples. A quite different example of an internally inconsistent theory provided by Norton is that of Newtonian cosmology (see Norton 2002). Frisch has also claimed that the application of classical electrodynamics to accelerating discrete charged particles yields an inconsistent theory (see Frisch 2005).

According to the counter-tradition, what these examples show is that far from being a kiss of death, logical inconsistency is a fact of life in science, and not a reason for the automatic rejection of a theory. In this vein, Priest talks about cases in which '... it may be rational to accept an inconsistent theory: when, despite its inconsistency, it is markedly better that its rivals on sufficiently many other criteria' (see Priest 2002, p. 125), and Da Costa and French tell us that 'inconsistent theories can be regarded as quasi-true and accepted as such, just like any other theory in science.'

What are we to make of the counter-tradition's non-standard views on logically inconsistent theories? It will help to proceed quite cautiously. The above case studies are are all examples of something—but what exactly are they examples of? This question is trickier to answer than one might at first think. I claim that a close examination

[1] The following taxonomy of examples of inconsistency in science is borrowed from Priest 2002.

[2] This phenomenon is discussed most famously by Cartwright (1983).

of the issues involved shows that there is not really any clear proposal being made by the counter-tradition about logical inconsistent theories that (i) really does go deeply against tradition, (ii) is plausibly true, and (iii) is supported by *any* of the case studies just described.

I will build a case for this in two steps. In the first step, before looking closely at any case studies, I will consider one way of drawing a distinction between the kinds of views about logical inconsistency in science that really do deeply go against tradition from those that do not. The goal will be to identify what might be plausibly described as the most modest proposal possible about logically inconsistent scientific theories that really does go against the dominant tradition in the philosophy of science. My conclusion will be that the counter-tradition must at the very least be proposing that the history of science gives examples of inconsistent theories in which the scientific community had justified belief. The discussion of this will occupy §2.

The next step will then be to try to determine whether the kinds of examples given by the counter-tradition actually provide evidence for this proposal. This will be the main job of §3. I will go through each of the three types of examples of inconsistency just mentioned, and argue that none of them describe situations in which we have justified belief in an inconsistent theory. And so even if the counter-tradition has identified a phenomenon whose existence would be troublesome to traditional views in philosophy of science, they have not succeeded in providing any clear examples of it. My broader conclusion is that the counter-tradition has given us no reason to think that traditional views about logical inconsistency in science are wrong.

2 What could the main claim be?

2.1 Some proposals

The collection of articles in Meheus (2002) probably constitutes the best statement and defense of the counter-traditional view on logical inconsistency in science. Because this work is a collection of articles from different authors, many different claims about the role and importance of logically inconsistent theories are made there. In order to analyze these points of view more easily, it will be useful to try to identify some sort of minimal 'main claim' about logically inconsistent theories that is supported by most members of the counter-tradition.

We must surely require that the main claim of the counter-tradition be genuinely 'counter' to 'the tradition.' In this vein, consider the following proposal:

Proposal 1 Some theories put forward by scientists have been logically inconsistent.

This is a straightforwardly true empirical claim. In the nineteenth century, some physicists took the Galilean transformation group to be the group of symmetries of Maxwell's equations. The theory consisting of Maxwell's equations plus the proposition that the elements of the Galiliean transformation group are symmetries of the laws of nature is, of course, inconsistent. But this amounts to nothing more than a type of mathematical error. As fallible creatures, we are all capable of making mathematical errors, and physicists are no exception. Neither Popper nor Hempel nor any reasonable philosopher would disagree with this. Proposal 1 is therefore not counter-traditional in any real sense of the word. And so if the case studies listed earlier are to be instances of a genuinely counter-traditional claim about inconsistency in science, they had better be something more than mere examples of logically inconsistent theories that were once put forward.

Note that here and in what follows we work with a very broad conception of what counts as a theory. For instance, one might respond to the example just given by drawing a distinction between the theory given by Maxwell's equations, and an (incorrect) claim about the symmetries of that theory. One might take the later to be a claim *about* the theory, rather than a *part* of the theory. Although there is much to be said for thinking of theories in this way, we will take a theory to be more or less any set of beliefs about the natural world. As such, a claim about the symmetries of nature is to be viewed as *part* of our 'theory of nature'. Working with this much broader conception of a theory only makes it easier for the counter-tradition to identify interesting cases of logically inconsistent theories, and as such, it will be charitable to the counter-tradition to think of theories in this broader way.

Returning to our main line of argument, note that the following proposal suffers from the same sort of defect as Proposal 1:

Proposal 2 Logically inconsistent theories can be important 'stepping stones' on the way to better, logically consistent theories, insofar as playing around with an inconsistent theory can lead to useful suggestions for a consistent theory with which to replace it.

This is also a straightforwardly true empirical claim. For an example, recall that the version of Maxwell's equations in which the displacement current is *not* included in Ampere's law is inconsistent with the possibility of current densities $\mathbf{J}$ for which $\nabla \cdot \mathbf{J} \neq 0$ (see §7.3 of Griffiths 1999.) It was by playing around with the resulting mathematics that Maxwell is believed to have come up with the idea of the displacement current, which is what makes Maxwell's equations fully general and fully consistent. No serious philosopher of science has ever wanted to deny that inconsistent theories can be useful in this sort of way, and so Proposal 2 is not genuinely counter-traditional.

What distinguishes the tradition from the counter-tradition are presumably the normative claims that each camp wants to make about logically inconsistent theories. According to the tradition, the discovery that a theory is logically inconsistent renders the theory as a whole *deficient*. But deficient in what way?

One of the main things we want from a scientific theory is for it to provide us with explanations.[3] An explanation always involves a phenomenon that is explained in

[3] It must be acknowledged here that there are ways of thinking about science according to which physical theories are *not* supposed to provide us with explanations. According to this point of view, a physical theory is just an efficient way of summarizing or modeling empirically observed regularities. This way of thinking about physical science goes back at least to Duhem (1991), and has been developed more recently by Van Fraassen (1980). According to this way of thinking, our epistemic attitude to the basic propositions of physical science need not be that of justified belief. I do not find this conception of science attractive, but will not try to argue against it here. Instead, I will simply take it for granted that good scientific theories ground bona fide explanations. Insofar as they do, I claim that the relationship in which we stand to our theories must be at least in part that we take ourselves to have justified belief in them.

terms of something else. That 'something else' we will assume to be the theory—it is *the theory*, after all, that we take to explain the phenomenon. Suppose then that some proposition P belonging to a theory is used in an explanation of some phenomenon. If the explanation is to count as a bona-fide explanation, it must be the case that we have justified belief in P. So, for instance, although inflationary cosmology might seem to explain the statistical homogeneity of the universe, unless we take ourselves to be justified in believing inflationary cosmology, we cannot get from it a bona-fide explanation of the homogeneity of the universe. Without justified belief in its basic principles, inflationary cosmology gives us nothing more than a compelling science-fiction story in which the universe ends up being statistically homogeneous. In brief, satisfactory scientific theories provide us with bona-fide explanations, and bona-fide explanations are built out of statements in which we have justified belief. Assuming that each part of a good theory does some sort of explanatory work, it follows that if a theory is to be useful for the purposes of explanation, it must be the object of justified belief. Contrapositively, a theory in which we do not have justified belief is deficient in the sense that it cannot be used for the purpose of explanation. I take it to be precisely this sort of deficiency that the tradition attributes to inconsistent theories. The following proposal is therefore genuinely counter-traditional:

Main Proposal It is possible for a scientific theory to be logically inconsistent and for us to nevertheless be justified in believing it.

I take this to be the main proposal advanced by the counter-tradition. Moreover, it seems to me that many members of the counter-tradition take the sorts of examples given in §1 to constitute straightforward evidence for this Main Proposal. I shall argue, however, that none of the examples listed there are examples of logically inconsistent theories in which the scientific community have or had justified belief. And so as far as I can tell, there are no clear confirming instances of the Main Proposal.

Before disucssing this, we briefly consider an obvious criticism of the Main Proposal. Because a logically inconsistent theory entails any sentence whatsoever, one might worry that the Main Proposal commits us to the claim that there are situations in which we must take ourselves to be justified in believing anything. That would obviously be reason for rejecting the Main Proposal. However, one possibility is that the Main Proposal is to be understood in conjunction with the idea that there are situations in which we are not necessarily justified in using all the rules of classical logic or all the machinery of ordinary mathematics in making deductions from a theory. Perhaps the Main Proposal is to be understood against the backdrop of paraconsistent logic; there are, of course, less drastic possibilities too. More generally, perhaps it is enough to reject the claim that justified belief is closed under arbitrary logical (or mathematical) consequence. There are issues here that deserve further thought, but for now it suffices to say that the Main Proposal deserves closer scrutiny.

2.2 Must we believe our theories?

One might worry that we have moved too fast here. Must we really have justified belief in a theory in order for it to be explanatorily useful? Consider, for instance, the idea that a theory need not even be the object of *belief* in order to be explanatorily powerful. We no longer believe classical mechanics, for instance, and yet we continue to take it to be explanatory in a wide variety of situations. Most physicists do not even believe that quantum mechanics is literally true (given complications involving gravity), and yet quantum mechanics is generally taken to have even greater explanatory power. This suggests that even in the case of theories that we take to have great explanatory power, our epistemic attitude to those theories need not be that of outright belief, but rather something more modest—we merely 'entertain' such theories, or 'treat them as if they were true'. Someone who viewed things in this way might well reject the Main Proposal. In fact, this is perhaps precisely the position endorsed by Smith (1988), Da Costa and French (2003), and several other members of the counter-tradition.

In this section, I will defend the idea that insofar as science is explanatory it only invokes claims in which the scientist has outright belief. Because I want to attack the idea that we can use a theory to explain something without believing the theory, my main challenge will be to make sense of how a theory like classical mechanics can still be explanatorily useful—as it seems to be—in spite of the fact that it is no longer a theory in which we have literal belief.

Let us focus then on classical mechanics for a while. It is certainly true that *in some sense* we still believe classical mechanics, while in another sense we do not. The challenge is to articulate these various senses. There is no question that the modern physicist rejects the literal truth of classical mechanics, and so it is fairly clear in what sense he no longer believes classical mechanics. The more interesting part of the challenge is to try and articulate the sense in which we *do* still believe classical mechanics, and take it to still have real explanatory power.

Today, classical mechanics is only used in domains where its consistency with quantum mechanics is well known (or seems reasonable to expect). For instance, no modern physicist would try to apply classical mechanics to understand the details of the nucleus of the atom. Furthermore, even in domains in which there is believed to be consistency between classical mechanics and quantum mechanics, it is only the *approximate* truth of classical mechanics that is ever used—so that even in situations that are amenable to a classical treatment, there are limits on the number of significant digits one can meaningfully include in a classical calculation. But with these limitations understood—that is, so long as classical mechanics is only used in appropriate, well-understood contexts to make claims of limited accuracy—the consequences of classical mechanics are correct, and believed with justification to be so. More precisely, for many physical systems described with a classical Hamiltonian and with appropriate classically described initial conditions, evolving the system forward in time in accordance with Hamilton's equations gives a value for the final state of the system that is (appropriately interpreted) approximately correct—i.e., correct to a few significant figures—and believed with justification to reliably be so. It is because of this that we are justified, for instance, in using classical mechanics to calculate the rough trajectory of a macroscopic projectile. It is in this sense, and presumably this sense only, that we still believe classical mechanics.

Note that I am not invoking any philosophically suspicious notion of 'approximate truth' here. All I am working with is the notion of literal truth applied to claims that involve approximations. It is literally true that when Hamilton's equations are used in the way just described, they give results that are correct to a couple of significant digits. Moreover, we are justified in expecting these sorts of claims to be true, at least for the right sorts of physical systems. Such claims, I take it, can even form the basis of explanations.[4]

All the calculations of modern physics that invoke classical mechanics can therefore be understood as derivations from claims in which we have justified belief. Understood this way, the ‘theory’ being invoked when we use classical mechanics today is not the theory that states that Hamilton’s equations are literally true, but rather the theory that states that Hamilton’s equations applied to certain sorts of systems give somewhat accurate results. Insofar as physicists neither believe nor need to invoke the full, literal truth of Hamilton’s classical equations, the fact that we need to be modest in stating the content of the physicist’s beliefs in this way is not any sort of problem.

But perhaps it is just at this point that I might be accused of missing the whole point of the counter-tradition. Perhaps, after all, it might be the *very point* of the counter-tradition that in spite of the formal inconsistency of the set of sentences the physicist manipulates, the *actual beliefs* that the physicist has about the world are perfectly consistent. That the physicist, through manipulating an inconsistent formalism, is able to arrive at a coherent and consistent conception of the world without falling into logical anarchy is a phenomenon that cries out for explanation. The fact that the physicist has a perfectly consistent conception of the physical world should not distract us from the fact that at a formal level the sentences he writes down are often inconsistent; it is this latter fact, and not the former, that the counter-tradition seeks to analyze and understand. And so perhaps my insistence on focusing on the *beliefs* that physicists have about the world, rather than the formalism they use to represent it, makes me blind to a possible counter-traditional project that does not involve endorsing the Main Proposal.

But I do not think that a counter-traditional project that goes along these lines is either attractive or promising. When a physicist writes down Hamilton’s classical equations, he does not intend them to be understood as literally true assertions about reality. Instead, the physicist intends them to be understood as claims that are true only when appropriately hedged and modified—that is, with restrictions on the domains in which they may be applied and restrictions on the accuracy with which they may be applied. Likewise for Schrodinger’s equation. It is only by ignoring the intention of the physicist and removing these assertions from their contexts of utterance that we turn the assertions of physics into a set of formally inconsistent sentences. But I do not see what we can hope to infer from the fact that it is possible to turn the assertions of physics into an inconsistent formalism in this way.

To see the problem even more starkly, let us consider an analogy. Suppose that in a conversation with a meteorologist inspecting some very accurate equipment I say ‘the temperature at noon yesterday (in a very specific location) was 30.1 °C’, and that a few minutes later I say to a friend on the street ‘the temperature at noon yesterday (at that particular location) was 30°C.’ Because of the particularities of the different conversational contexts, it would be perverse to say that I have contradicted myself.

[4] Understanding how explanations function in the presence of idealizations can be complicated for reasons that I have not discussed here. For a more detailed discussion, see Davey (2011).

And yet by removing these sentences from their contexts of utterance it is possible to make it look like I have an inconsistent theory of the world—after all, if $x \neq y$ then the temperature cannot be both $x\,^{\circ}C$ and $y\,^{\circ}C$ at the same time and place. This inconsistency, however, involves a fairly clear misrepresentation of the content of my utterances. When I talk about the weather in this way with these different people there is surely *absolutely no* sense in which I am in the grips of an inconsistent theory, somehow mysteriously avoiding logical anarchy in a way that needs to be accounted for.

From the mere fact that it is possible to create an inconsistent theory from someone's utterances in this way we cannot conclude that they are somehow in the grips of an inconsistent theory *at any level*. Likewise, the fact that a physicist might invoke the classical equations of Hamilton in one context and Schrodinger's equation in another context amounts to a inconsistency on the physicist's part only if we willfully (and perversely) ignore the relevant contexts of utterance. From the fact that it is possible to create an inconsistent theory from the physicist's utterances in this way, we have no grounds to conclude that the physicist is in the grips of an inconsistent theory, somehow avoiding logical anarchy in a way that cries out for explanation. In light of this, this way of trying to motivate a counter-traditional project that does not endorse the Main Proposal seems to me to rest on a mistake.

With all these considerations in mind, I conclude that the counter-tradition must really be committed to something like the Main Proposal if they are to be claiming something that is genuinely and deeply counter-traditional, and that has a chance of being interesting, substantive, and true.

3 Case studies

In the previous section, I argued that the counter-tradition must be committed to the view that it is possible for a good scientific theory to be logically inconsistent and for us nevertheless to be justified in believing the theory. This proposal—the 'Main Proposal'—goes against much traditional wisdom in both the philosophy of science and epistemology. Most philosophers' natural reaction would be to reject it.

The counter-tradition, however, wants to argue that we find actual science to be riddled with inconsistencies. Many of these inconsistencies are quite transparent, and all of the beliefs involved seem as justified as it is reasonable to demand. Given this, one might think that we have no choice but to accept the Main Proposal.

In this section I will ask whether it is really true that we find evidence for the Main Proposal in the practice of science. I will argue that we do not. Although this does not constitute a definitive argument against the Main Proposal, the lack of genuine examples surely counts as a major strike against it. I will consider in turn each of the three classes of examples mentioned in Priest's (2002) taxonomy—inconsistency between theory and observation, inconsistency between different theories, and internally inconsistent theories.

3.1 Inconsistency between theory and observation

A well-accepted theory can sometimes conflict with what is observed. In the early nineteenth century, Newtonian gravity was taken to predict an orbit for Uranus different from what had been observed. Even after this was resolved with the discovery of Neptune, Newtonian gravity predicted a value for the precession of Mercury's orbit in conflict with observation. If we consider our best physical theory to consist not just of what we take the laws of nature to be, but also the observational data of which we are most confident, then these are situations in which we have justified belief in a theory which is inconsistent, and known by us to be so. One might even take the existence of this sort of phenomenon to be the whole point of Kuhn's discussion of anomalies Kuhn (1996).

It is not clear, however, that examples like this provide evidence for the Main Proposal. Consider first the case of the orbit of Uranus. By the early nineteenth century, scientists were justified in their belief that Uranus had the orbit that it in fact has. The evidence for Newtonian gravity was still overwhelming, and so the scientists of the time were also justified in believing the conjunction of classical mechanics and the inverse square law. Once the inconsistency in question was known, however, it is fairly clear that the scientific community no longer took themselves to be justified in assuming that there was no significant matter in the solar system past Uranus. The fact that most efforts to explain the discrepancy in Uranus' orbit involved revising traditional beliefs about the composition of the outer solar system confirms that the scientific community at this point took their knowledge about the outer solar system to be conjectural at best. So once the scientific community recognized the logical inconsistency of the theory in question, they no longer took themselves to be justified in believing it. This particular moment in the history of science therefore cannot count as a positive data point for the Main Proposal.

The case of the precession of Mercury may be dealt with similarly. By the mid to late 19th century, the conjunction of Newton's laws with the inverse square law of gravitation, the prevailing view about the distribution of matter in the solar system, and the observational facts about Mercury's orbit were known to be mutually inconsistent. Prior to the development of the special theory of relativity scientists typically took their belief in classical mechanics to be quite solid, and so expected that the problem with Mercury's precession would somehow be resolved within the Newtonian paradigm. Because of this, most scientists no longer took themselves to be justified in believing the prevailing story about the distribution of matter in the solar system. Some postulated a hitherto undiscovered inner planet 'Vulcan' Baum (2003); others postulated a cloud of dust between the Sun and Mercury. The feeling that some sort of hypothesis like this must have been true was widespread, and it seems to me entirely justified. And so we see again that once the inconsistency in the larger theory was known, scientists no longer took themselves to be justified in believing it. Once more, this is *not* a case of the scientific community taking themselves to have justified belief in something known to be logically inconsistent.

As an aside, note that the fact that the scientists of the time no longer took themselves to be justified in believing the prevailing story about the distribution of matter in the solar system is the sort of attitude that would have been reasonable even if the prevailing story about the distribution of matter in the solar system had subsequently been proven entirely correct, and that changes in Newtonian gravity had been sufficient to solve the problem of the precession of Mercury.[5] One can correctly take oneself to not have justified belief in something that subsequently turns out to be true, just as one can correctly take oneself to have justified belief in something that subsequently turns out to be false.

It seems to me that none of the usual examples of 'scientific anomalies' are examples of situations in which scientists have taken themselves to have justified belief in something that they have known to be inconsistent. Once an anomaly is understood to be an anomaly, scientists typically recognize that there is some component of their world-view in which they do not really have justified belief. Whether the component of their world view in which they no longer take themselves to have justified belief turns out to be true or false may differ from case to case. Regardless of how the details of these stories play out, it seems to me that these sorts of examples do not provide us with positive data points for the Main Proposal.

3.2 Inconsistency between different theories

Sometimes two distinct, well-accepted theories can appear to come into logical conflict with one another. There are two quite different ways in which this can happen, and I shall discuss each of them separately.

The first way in which distinct, well-accepted theories can appear to come into logical conflict with one another occurs when a physical system is described differently in different contexts. So for instance, from the point of view of the Navier–Stokes' equations a liquid is a continuous distribution of matter, while from the point of view of a simple version of kinetic theory it consists of a large number of point particles executing a type of random motion. In spite of the incompatibility of these two ways of thinking about liquids, physicists draw freely on the Navier–Stokes' equations when it seems appropriate, and kinetic theory when it seems appropriate. This might be thought to be an example of inconsistency in science.

It is difficult, however, to see how this is supposed to be evidence for the Main Proposal. No physicist would be justified in claiming *either* that liquids are literally continuous distributions of matter *or* that they are literally large sets of point particles executing random motions, let alone both, and so it is difficult to see how this is supposed to be an example of a scientist in the grips of an inconsistent theory. Of course, the physicist *is* committed to the claim that for certain purposes it suffices to treat a liquid as a continuous distribution of matter, and that for other purposes it suffices to treat a liquid as a large set of point particles executing a type of random motion—but there is nothing logically inconsistent in that, any more than there is an inconsistency between the mother who in ordinary circumstances says that her son is 5 feet tall and the very careful doctor who says that he is 5.01 feet tall. Both the mother and doctor are making claims that are to be understood as true within certain contexts,

[5] Even though the solution to the problem of Mercury's precession did not depend on something like the discovery of new matter in the solar system, one cannot say that the prevailing story about the distribution of matter in the solar system was unchanged by the development of special relativity, given that the very metric of the space-time in which matter resides is radically different in classical mechanics and special relativity.

and which may be used as the basis of some sorts of inferences and not others. The only way to view the mother and the doctor as disagreeing would be, for instance, to view both the mother and the doctor as committed to the literal, unqualified truth of their assertions about the height of the boy. In normal situations, this would just be to misinterpret the content of their claims. Short of this sort of misinterpretation, there is no clear sense in which the mother and doctor are even disagreeing. Similarly, in order to force the physicist into the grips of a logically inconsistent theory we must view her as committed to the literal truth of all the claims she makes when modeling a liquid in the different ways described. But to do this would surely be to misunderstand the phenomenon of approximation in the sciences. The bottom line is that it is difficult to see how on any charitable reading of the methods of physics there is some sort of inconsistency here. If this is right, then this sort of example is not a place where we can find support for the Main Proposal.

The example of the conflict between quantum mechanics and general relativity may be treated in a similar way. In their best current formulations, quantum mechanics and general relativity paint different (and incompatible) pictures of the world. But no physicist is committed to the literal and final truth of either quantum mechanics or general relativity—at least, not in their present form. Physicists *are* committed, however, to the claim that these theories are very useful tools for understanding a disparate set of physical phenomena with great accuracy. None of this involves anything close to belief in a logical inconsistency.

The second class of examples of distinct theories coming into logical conflict with one another involves focusing on the consequences (rather than the presuppositions) of different theories. Priest cites the example of Kelvin on the age of the Earth (Priest 2002, p. 22) as an example of this. Suppose that the earth began its life as a 3,900 °C ball of molten lava, that the earth is more or less homogeneous, that no heat is generated inside of it, and that its exterior is heated only by the sun. Then using the laws for the diffusion of heat, Kelvin in the 1860s argued that the earth is approximately 20 to 400 million years old. The biologists and geologists of the time, however, required the earth to be billions of years old in order to allow enough time for evolution and various geological phenomena. For several decades, then, physics found itself in contradiction with the best judgments of biology and geology. The arguments from biology/geology were basically sound—the problem, it is often said, turned out to be that heat is generated inside the earth as a result of radioactivity. This renders Kelvin's calculation completely invalid. But radioactivity was not even discovered until the 1890s, and so Kelvin was surely justified in ignoring it. If one thinks of the best physics, biology and geology of the 1860s as a theory, then at that time one would have been justified in believing this theory, even though it was logically contradictory and known to be so. This seems to provide a strong positive data point for the Main Proposal.

But as has been noted by others, the conventional wisdom on this story is not quite right. In the 1890s, Perry pointed out that Kelvin's argument presupposed that the earth was a rigid body. If one supposes that the earth has a fluid interior (as it does), then the kind of calculation done by Kelvin gives an age for the earth in the billions of years, in agreement with biological and geological evidence. The correction introduced by the presence of radioactivity turns out to be minor by comparison. (See England et al. 2007.) The philosophical question, then, is whether Kelvin was justified in assuming that the earth was homogeneous and rigid, even once he knew that the consequences of this assumption were at odds with the biological and geological wisdom of the time. I find a negative answer compelling here. The only real reason Kelvin had for thinking that the entire earth was completely rigid was that its exterior is more or less rigid. Kelvin's calculation therefore used at least one highly idealized assumption that was nothing more than the result of a fairly weak induction. Assuming, for the sake of argument, that the conclusions of the biologists and geologists were justified, the natural (and actual) response to the contradiction in question was Perry's—to regard Kelvin's assumptions with suspicion. The bottom line is that it does *not* seem right to describe the scientific community of the late nineteenth century as having justified belief in the conjunction of Kelvin's theory of the earth and the broader biological/geological theory of the earth. And so this does not provide us with a positive data point for our Main Proposal.

When two theories contradict one another and are known to do so, I claim that in one or both of the theories there will be found to be at least one assumption in which we do not have justified belief. The above case studies bear this out. So long as a weakness of this sort exists in one or both of the theories, we fail to have a positive data point for the Main Proposal.

3.3 Inconsistencies within a single theory

We turn now to the case of internally inconsistent theories.

3.3.1 Newtonian cosmology

An example originally presented by Norton (2002) is that of Newtonian cosmology. Newtonian cosmology consists of Newton's three laws, the inverse square law of gravitation, and the claim that space is infinite, Euclidean, and filled with a completely homogeneous matter distribution. Consider a test mass in such a universe. For any force $\vec{F}$ of any magnitude and direction, Norton shows how Newtonian cosmology entails that the net force on the test mass is $\vec{F}$. Because the net force on a test mass should be given by a unique vector, it follows that Newtonian cosmology is inconsistent.

I do not think, however, that this argument shows the inconsistency of Newtonian cosmology. If our test mass has mass m, lies at the origin, and the mass-density of the surrounding space-filling matter distribution is μ, then the net force on the test particle will be given by:

$$\vec{F} = \int dxdydz \, \frac{Gm\mu}{r^2}\hat{r},$$

where $\hat{r}$ is the unit vector that points from the origin towards (x, y, z), and $r^2 = x^2 + y^2 + z^2$. The x-component of this force is given by

$$\vec{F}_x = \int dxdydz \, \frac{Gm\mu}{r^2} \cdot \frac{x}{r}.$$

This integral, however, is not absolutely convergent—i.e.,

$$\int dxdydz\,|\frac{Gm\mu x}{r^3}| = \infty.$$

This means (as Norton's argument confirms) that the magnitude of the net force on the test mass depends on the order in which the individual contributions are summed. From a strict mathematical point of view, the correct thing to say is that the expression:

$$\int dxdydz\,\frac{Gm\mu}{r^2}\hat{r}$$

for the net force is not mathematically well defined. And so Newtonian cosmology, rather than telling us that *every* force is the net force on the test particle, fails to tells us that *any* force is the net force on the particle. Thus, there is no logical inconsistency between Newton's laws of motion, the inverse square law of gravitation, and the assumption of a homogeneous, isotropic matter distribution—there is only an inconsistency between these suppositions and the assumption that the usual laws of calculus may be applied blindly to integrals that are not absolutely convergent.

One way of trying to force Newtonian cosmology into a logical contradiction is by insisting that the expression:

$$\int d\vec{r}\,\frac{Gm\mu}{r^2}\hat{r}$$

has a unique numerical value, and that the rules of the calculus apply to it. More specifically, it suffices to insist that for any countable collection $O_1, O_2, \ldots$. of disjoint open regions of $\mathbb{R}^3$ with $O = \cup_i O_i$,

$$\int_O d\vec{r}\,\frac{Gm\mu}{r^2}\hat{r} = \sum_{i=1}^{\infty}\int_{O_i} d\vec{r}\,\frac{Gm\mu}{r^2}\hat{r},$$

and that for any open region O, if the integral

$$\int_O d\vec{r}\,\frac{Gm\mu}{r^2}\hat{r}$$

is absolutely convergent, then it should be assigned a value in accordance with the usual rules of the calculus. With these additional hypotheses in place, Newtonian cosmology becomes genuinely logically inconsistent. But the problem is that these additional hypotheses are inconsistent on their own (whether Newton's laws or the hypothesis of a homogenous universe is adjoined to them or not), and so the inconsistency really has nothing to do with cosmology at all—all we have are an inconsistent set of mathematical claims that have been grafted onto an otherwise consistent physics. More to the point, rudimentary knowledge of the calculus shows that these additional mathematical hypotheses are not justified. Insofar as we are not justified in believing these additional hypotheses, we still fail to have a positive data point for the Main Proposal.

3.3.2 Blackbody radiation

For another example of an internally inconsistent theory, I turn to one of the counter-tradition's favorite examples—Planck's theory of the thermodynamics of electromagnetic radiation. In Planck (1959), Planck's main goal is to derive the spectrum for blackbody radiation from a statistical-mechanical treatment of electromagnetic radiation. In the process of doing this, Planck needs to consider the result of adiabatically compressing a gas of electromagnetic radiation. Planck's treatment of this is explicitly classical, invoking only Maxwell's equations. When finally calculating the spectrum of blackbody radiation, however, Planck introduces a theory of electromagnetic radiation that is fundamentally quantum mechanical in nature, and hence incompatible with Maxwell's equations. So although Planck's theory paved the way for the quantum revolution, it rested on inconsistent foundations.[6] Fully quantum mechanical treatments of the photon gas have since been developed, and are now included even in many elementary textbooks on statistical mechanics (see Pathria 1996, for instance).

There has been much debate about precisely what Planck did or did not achieve in his work leading up to and including Planck (1959). Kuhn argues in 1978 that Planck's early work proceeded from a entirely classical point of view, and that it is a mistake to think of the early Planck as 'quantizing' the oscillator. If this is right then there is nothing intrinsically inconsistent about Planck's theory, and we can no longer use it as a positive data point for the Main Proposal.[7]

Another way of thinking about Planck's work would involve claiming that while it was of immense historical and conceptual importance, it did not provide the scientific community with a story about the spectrum of blackbody radiation in which the community could really have taken themselves to have justified belief. Some have suggested, for instance, that the early Planck underestimated the strength of Rayleigh's arguments, or that he did not fully appreciate the consequences of the equipartition of energy theorem. (See Klein 1962, and Galison's survey 1981). On this point of view, Planck's account of blackbody radiation was simply not the sort of mature theory in which the scientific community could ever have had justified belief. And so on this way of thinking too, Planck's theory cannot be used as a positive data point for the Main Proposal.

It is not easy to read Planck's work in such a way that it has the conceptual coherence needed to view it as a story in which the scientific community might have had justified belief, while taking seriously the idea that the story is, at points, genuinely non-classical. One might do better to look at later work on blackbody radiation such as that of Einstein, which although much more mature, still invoked a mixture of classical and quantum principles. (Norton's papers 1987, 1993 contain a good discussion of the relevant history). The natural way to read this more mature work, however, is as proceeding under the assumption that when calculating the effect of adiabatic

[6] There are other moments in which Planck (1959) treats heat radiation from a purely classical point of view—I have picked only one of the more salient examples.

[7] Whether there might be some other inconsistency in Planck's work is a separate question; at any rate, those who have argued for the inconsistency of Planck's theory have argued that it is inconsistent in so far as it simultaneously invokes a classical and non-classical account of electromagnetic radiation.

compression on a gas of electromagnetic radiation, or deriving Wien's law, one can safely ignore the quantum mechanical nature of the gas.[8] There is, however, no logical contradiction involved in pointing out that although matter is quantum in nature, certain systems may be treated classically in certain calculations. An interesting historical question is whether physicists such as Einstein were justified in making this assumption. If they were not, then it seems to me that their accounts of blackbody radiation could not have been the objects of justified belief. If, on the other hand, such an assumption was justified, then we are left with a justified theory which is perfectly consistent. Either way, we fail to have a positive data point for the Main Proposal.

To get a positive data point for the Main Proposal, one would have to argue that when someone like Einstein used Maxwell's equations in his calculations, he did so because he thought that Maxwell's equations were literally true, and that when he used quantum mechanics elsewhere in the same calculations, he did so because he thought that quantum mechanics was also literally true. One would also have to maintain that this sort of approach was completely justified. No-one has credibly made the case that this is the right way to read Einstein, Planck, or any of the luminaries associated with the quantum revolution.

My goal here has not been to take sides in the debate about the right way to talk about Planck and the quantum revolution. All that matters is that on none of the standard views of what Planck or Einstein was doing do we find a positive data point for the Main Proposal.

3.3.3 Classical electrodynamics

In his book Frisch (2005), Frisch claims that classical electrodynamics applied to discrete, charged particles is an inconsistent theory. More specifically, he argues that the following 4 propositions are inconsistent:

1. There are discrete, finitely charged, accelerating particles.
2. Charged particles act as sources of electromagnetic fields in accordance with Maxwell's equations.
3. Charged particles obey the Lorentz force law.
4. Energy is conserved in particle-field interactions.

(See Chap. 2 of Frisch 2005 for details.) Does classical electrodynamics therefore provide us with a positive data point for the Main Proposal?

The situation is not simple. First of all, we have to decide whether by a 'discrete' charge we mean a point particle or something spatially extended over a small region. As both Belot (2007) and Muller (2007) have pointed out, it is difficult to reconcile the existence of point particles with classical electrodynamics (henceforth CED), especially insofar as we want to meaningfully talk about energy conservation. Muller goes so far as to say that '... according to CED electrons are not point particles' and that '... point particles fall outside the domain of CED' (p. 267). For Muller then, textbook applications of classical electrodynamics to point particles are mathematically

[8] Norton's argument in 1987 essentially vindicates the claim that the quantum mechanical nature of the gas can be ignored in the derivation of Wien's law.

unrigorous (though extremely useful) applications of an otherwise entirely consistent theory. I find this point of view compelling. But more to the point, insofar as physicists have for the most part *not* thought of elementary particles as point-like structures, it is difficult to see how Frisch's inconsistency argument run on point particles can count as a positive data point for the Main Proposal. In such a setting, the most that Frisch's inconsistency argument can do is convince us even more strongly of our antecedently held belief that the fundamental carriers of charge are not point-like structures.

What happens then if we think of discrete charges as extended over small regions of space? Whether Frisch's argument gives a genuine contradiction in this case depends on whether our formulation of the Lorentz force law includes only the external field or the total field consisting of the external field plus the self-field. As Frisch himself points out, the inconsistency argument depends critically on formulating the Lorentz force law using only the external field. (This point is discussed in detail by both Belot (2007) and Muller (2007).) Including only the external field in the Lorentz force law is, however, surely unjustified; for presumably every part of the total electromagnetic field—be it the contribution from the external field or the contribution from the self-field—will exert a force on any particle located anywhere that these various fields are non-zero. In particular, it is surely the total field, and not just the external field, that acts on the original particle itself. Moreover, explicit solutions *have* been constructed for Maxwell's equations and the total-field formulation of the Lorentz force law for accelerating, rigid distributions of charge (see Bauer and Durr 2001), and so this theory is definitely *not* inconsistent. Insofar as Frisch's inconsistency argument depends on an external-field formulation of the Lorentz force law, it is therefore difficult to see why his argument shows us anything other than the inadequacy of the Lorentz force law thus formulated. And so again, it is difficult to find any evidence for the Main Proposal here.[9]

Having said this, I do not mean to deny that classical electrodynamics is much more philosophically interesting than has often been supposed (and that Frisch deserves credit for bringing this to the attention of the philosophical community.) For instance, although models for (extended) discrete charges obeying Maxwell's equations and the (total-field) Lorentz force law have been constructed, the particles they describe are non-relativistic. The literature does not seem to contain any widely accepted constructions for fully relativistic models of extended discrete charges obeying Maxwell's equations. That classical electrodynamics can have thrived for so long without a solid foundation of this sort is quite surprising; that it has thrived so well—so much so that these deficiencies in its foundations are barely known to anyone but the expert–is perhaps even more remarkable. None of this, however, makes classical electrodynamics a positive data point for the Main Proposal.

Although classical electrodynamics is surely a wonderful philosophical example of *something*, I think that the question of *what* it is an example of requires further thought. In more recent work, Frisch has actually slightly shifted his rhetoric away from the claim of outright inconsistency ('... I am inclined to agree with my critics that this inconsistency in itself is less telling than my previous discussions may have

[9] For a somewhat different point of view, see Vickers (2008).

suggested', Frisch 2008, p. 94), and has focused instead on other more subtle ways in which classical electrodynamics challenges traditional views of theory acceptance.[10] This change in tone seems right. If we misdiagnose the ailments of classical electrodynamics, we will likely under-appreciate or even miss these deeper and more interesting issues.

4 Conclusion

I claim that whenever we find a logical contradiction in a body of scientific beliefs, we also come to realize that one or another of these beliefs (or at the very least, their conjunction) is unjustified. The case studies presented above bear this out. That is why the discovery that a theory is logically inconsistent is typically followed by an attempt to identify and replace any unjustified assumptions in the theory. Identifying and replacing the unjustified assumptions in a theory need not be easy, and there are no simple rules as to how this sort of thing is to be done. However, this does not necessarily leave the scientist empty-handed. Faced with a theory that is known to be inconsistent, the scientist will still be able to trust consequences of the theory that are based on especially well-confirmed parts of the theory. In most cases of inconsistent theories that we have examined, there is a relatively clear division between the 'solid' part of a theory in which the scientist has justified belief, and the more 'speculative' part of the theory in which the scientist does not. In such cases, the scientist can continue to make justified inferences by using only those parts of the theory in which she has justified belief. This, and not the adoption of some sort of paraconsistent logic, is what allows the scientist to continue her job prior to the identification of a consistent successor theory.

None of this story requires that we abandon any of the traditional views about logical consistency in science. And so although the case studies discussed by the counter-tradition have a lot to teach us about the methodology of science, I do not think that they show us that traditional philosophical views about logically inconsistent theories are wrong.

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[10] See especially section 3 of Frisch (2005).

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