GRAHAM PRIEST

INCONSISTENCY AND THE EMPIRICAL SCIENCES

1. INTRODUCTION

What role does, or should, inconsistency play in the empirical sciences? This is the question that I will address in this essay. The question is hardly a new one, but the development of modern formal paraconsistent logics has a profound impact on the subject. Paraconsistent logicians have realised that their subject has important implications for the empirical sciences and the philosophy thereof,[1] but discussions of the applications of paraconsistent logic have focused largely on non-empirical areas, such as semantics and metaphysics. It therefore seems appropriate to address the question directly.[2]

I will first address the issue of the specificity of the empirical sciences: observation. Next, we will look at the role that inconsistency has played in science (henceforth in this essay, I will often take the qualifier "empirical" for granted), and the relation of this to paraconsistent logic. This will raise the question of how inconsistent information *ought* to be treated in science: what criteria, for example, may lead us to accepting an inconsistent theory? And should such an acceptance ever be more than provisional? These topics will be addressed in the next sections. An outcome of this discussion will be that, in the light of developments in paraconsistent logic, we may well have to change our attitude to inconsistencies of certain kinds; such a change would open whole new possibilities in science itself.

2. INCONSISTENCY AND OBSERVATION

Before we can address the issue of the role of inconsistency in empirical sciences, it will be important to discuss how such sciences differ from similar inquiries (which are often intricately connected with them), such as mathematics and metaphysics. The standard answer to this question is that in the empirical sciences, but not the others, observation plays a role. In the more developed sciences such observations

[1] See, e.g., Priest and Routley 1989a, 367-79 and Priest and Routley 1989b, 494ff.

[2] I am grateful to Joke Meheus for the promptings to do so, and for her insightful comments on an earlier draft of the paper. I am also grateful to Diderik Batens for thought-provoking discussions on the topic over the years, and for his comments on a draft of the paper. A version of the paper was given at a seminar in the Philosophy Department at the University of Melbourne. I am grateful to many of the participants there for their comments and questions.

J. Meheus (ed.), Inconsistency in Science, 119—128.

are obtained through active experimentation. The observations serve to provide the ultimate *explananda* for science, as well as providing important inputs into the evaluation of scientific theories. This standard answer is not entirely unproblematic: the role of empirical data in science has often been overrated, and its role in areas such as philosophy under-rated—for example, by positivists and empiricists. However, that it plays a much more central role in science than in other inquiries can hardly be denied.

The distinction between what is observable and what is not, is not, itself, entirely unproblematic. Some states of affairs are certainly observable (such as the colour of an insect) and some are certainly not (such as the colour of a quark). But it is impossible to draw a sharp boundary between what is, and what is not. What is observable may depend on what aids to perception (such as a microscope) are used; or on how we interpret our sensory input in the light of accepted theories (such as theories that tell us that when we witness the tracks in a bubble-chamber, we are seeing the tracks of charged particles). However, as with all vague or context-dependant distinctions, the fact that it is impossible to draw a neat line between its two sides does not mean that there is not an important distinction to be drawn. (Compare: being a child and being an adult.)

We can now proceed to an issue of importance in what follows. What can one see?[3] For a start, seeing, in the sense relevant to explanation and confirmation, is propositional. We see *that* something is the case. We see that the stars appear to be in a certain position and ask for an explanation, or use this against a theory according to which they should appear somewhere else.

Can we also see that something is *not* the case? Some have thought not.[4] We always see that something *is* the case, and then infer that something else is not the case. For example, we see that something is red and infer that it is not black. Whilst one may certainly do this, it seems to me that one need not: one can see directly that something is not the case. Try a thought experiment. I show you an ordinary apple and ask: is this black? You compare its colour with a mental paradigm of blackness, and it does not match; you say no.[5] Or again, you enter a room; the whole room is visible from where you stand; there is no one there. You can see that Pierre is not in the room. You do not have to say: the things in the room are a chair, a table ...; Pierre is not a chair, Pierre is not a table...; therefore, etc.[6] Even the very distinction between seeing what is the case and what is not the case is a false one. Some seeings are both. When talking of physical objects, to be transparent is not to be opaque and vice versa. But you can see that something is transparent and you can see that something is opaque.

It should be remembered that seeing is not simply a matter of light rays hitting the retina. Certainly, the eyes are involved in seeing; but one needs more than eyes to see. If what one sees is to play a role in one's cognitive functioning, one must also

[3] In what follows, I will restrict myself to discussing vision, since this is by far the most important sensory modality in science; but similar comments apply to the other senses.

[4] E.g., Vasil'év (1913).

[5] I am not suggesting that all vision is paradigm-based in this way, as should become clear in a moment.

[6] The example comes from Sartre (1943, ch.1, sect.2).

understand one's visual input. Hence, the categories of the understanding play a role. To see is to interpret ones visual stimuli, by applying these categories, either consciously or preconsciously; and there is no reason why truth functions such as negation or disjunction should not enter into the process of interpretation directly. For example, I can see that this is a photograph of either Ned or Ted, identical twins such that I cannot tell the difference, without seeing that it is a photograph of Ned, and inferring the disjunction.

Next step: if α and β are states of affairs observable at the same time and place,[7] then so is their conjunction. We can see that something is a unicorn; we can see that it is green. Hence, we can see that it is a green unicorn. Applying this: if α and $\neg\alpha$ are observable states of affairs, so is $\alpha \wedge \neg\alpha$. Of course, this is not to say that the conjunction is observed; merely that it is observ*able*—that is, it is of a kind such that if it were to be the case, it could be seen.

One might doubt this. Might it not be the case that our cognitive functioning makes it impossible for us to see an inconsistent state of affairs? Might not the way our perception works impose a 'consistency filter' on what we see? No. Seeing impossible situations is quite possible. This is what we perceive in various visual illusions. Thus, for example, there are many well known impossible figures (of the kind, for example, employed by Escher in his drawings); there are perceptual sets where people report seeing things as simultaneously red and green; there are situations where things appear to be moving and not moving. Let me just describe one of these in more detail, the last.[8] This is commonly known as the waterfall illusion. After conditioning the visual field by showing it constant motion of a certain kind, say a spinning spiral, one then looks at a stationary scene. The scene appears to move in the opposite direction, but nothing in the scene changes its position; for example, an object at the top of the visual field does not move round to the bottom. What we see appears to be both moving and stationary; this is the natural way of describing one's visual sensations. Of course, perception in the cases I have described is not veridical;[9] these are illusions, and things are not really thus; but that is how they would appear if things were thus. There is therefore nothing about our visual system that requires perception to be consistent.

3. TYPES OF INCONSISTENCY

Let us now turn to the issue of inconsistency in science. The inconsistencies in question here are inconsistencies in what are accepted scientific beliefs. Many

[7] From the same perspective—and whatever other qualifications one needs to make to rule out irrelevant counter-examples.

[8] Details of examples such as this can be found in most books on vision and visual illusions, e.g., Robinson 1972. Examples of this kind are further discussed in Feyerabend 1975, 258ff., and Priest 1999a.

[9] Which is not to say that a perception of a contradictory state of affairs cannot be veridical. But then, how do we know that the cases in point are illusions? Many things tell us this. For example, in the case of the waterfall illusion, the apparent motion of, say, the room, is not confirmed by our other senses, or by other people. And if the room were really moving, things would have a tendency fall over, which they do not, etc.

historians and philosophers of science have observed that there are such inconsistencies—indeed that they are common—even in contemporary science.[10] If we distinguish between observation and theory (what cannot be observed), then three different types of contradiction are particularly noteworthy for our purposes: between theory and observation, between theory and theory, and internal to a theory itself.[11] Let us look at these three in more detail.

Inconsistency between theory and observation is the most obvious example. We have a well-received theory, T, with a certain observable consequence, α. We then run an experiment and observe that $\neg\alpha$. Simple-minded falsificationists would suggest that this shows that T is wrong and is to be ditched. But many philosophers of science have pointed out that this does not necessarily happen.[12] The contradiction may be treated as the site of an anomaly: both T and $\neg\alpha$ may be accepted *pro tem*. T will not be jettisoned until we have a better alternative; $\neg\alpha$ will not be jettisoned until we have an explanation of why our observation was incorrect. Examples of this kind in the history of science are legion. The precession of the perihelion of Mercury, at odds with Newtonian celestial dynamics, was known for a long time before the rejection of Newtonian dynamics in favour of special relativity. Prout's hypothesis was widely accepted by many chemists even though it was known to be at odds with empirical data (much of which was subsequently rejected). And so on.

The second kind of inconsistency, between theory and theory, is less frequently noted, but certainly occurs. This is when we have two well-accepted theories, T_1 and T_2, which have inconsistent consequences. Again, though this may be noted as the site of a problem, both T_1 and T_2 are retained until a suitable replacement theory for one or both is found. An example of this concerns the age of the earth in late 19th century science. According to evolutionary theory (which was, by that time, well accepted), the age of the earth had to be hundreds of millions of years; but according to received thermodynamics, the sun—and so the earth—could not possibly be that old. (The issue was resolved only in the 20th century with the discovery of radioactivity, a hitherto unsuspected form of heat-generation.) Another, contemporary, example: the theories of relativity and quantum theory are known to be mutually inconsistent.

The third example of an inconsistency is when a theory is self-inconsistent. This could arise because a theory has inconsistent observational consequences, though I know of no interesting cases of this in the history of science. What there certainly have been are inconsistent theories where the inconsistencies are located internally, away from observational consequences. For example, for over a hundred years Newtonian dynamics was based on the old calculus, in which infinitesimals had inconsistent properties (being both non-zero, at one point in computations, and zero at others). Another, particularly striking, example is Bohr's theory of the atom, which included both classical electrodynamic principles and quantum principles that

[10] For example, Lakatos 1970, Feyerabend 1975, ch.5, which can be consulted for details of many of the following examples.

[11] A good general discussion of kinds of problem-situations in science, including those involving various kinds of contradictions, can be found in Laudan 1977, chs.1, 2.

[12] E.g., Kuhn (1970), Lakatos (1970), and Feyerabend (1975).

were quite inconsistent with them. Though the theory was certainly considered as problematic, its empirical predications were so much better than those of any other theory at the time that it had no real competitor.[13]

4. HANDLING INCONSISTENCY

As we have seen, the corpus of scientific beliefs may, or is even likely to be, inconsistent at any time. But from things accepted, scientists infer other things that they accept, and they do not infer arbitrary conclusions. It follows that the inference procedure employed here must be a paraconsistent one (where an arbitrary α and $\neg\alpha$ do not entail an arbitrary β). What paraconsistent inference procedure is employed in inconsistent cases is another question; there is no *a priori* reason to suppose that it must be one of the standard monotonic paraconsistent logics, or even that it must be monotonic at all. Nor should one suppose that it must be the same in every case. What is guaranteed is that there must be some systematic procedure for drawing conclusions, and which does not permit drawing an arbitrary conclusion from a contradiction. What procedure is or was employed in any given case is a matter for detailed investigation.

I do not intend to discuss detailed examples here; but for what follows, it will be important for us to distinguish between two different kinds of paraconsistent logic: adjunctive and non-adjunctive. In adjunctive paraconsistent logics, such as standard relevant logics and da Costa's *C* systems, the rule of adjunction, $\alpha,\ \beta \vDash \alpha \wedge \beta$, is valid. In non-adjunctive systems, such as discussive logics, it is not. Non-adjunctive paraconsistent logics often employ some chunking procedure. Because of the failure of adjunction, one cannot simply put arbitrary premises together and draw conclusions. If the inference procedure is not to be impoverished, it is usually necessary to be able to put *some* premises together (into a chunk). A simple procedure is to put together any bunch of premises that are mutually consistent; but there are more sophisticated ones.[14]

One thing that is clear from a fairly cursory consideration of the scientific handling of inconsistent information is that the inference procedure employed is often of a non-adjunctive, chunking, variety. For example, given the dispute about the age of the earth at the end of the 19th century, no one conjoined the views that the earth was hundreds of millions of years old, and that it was not, to infer that the

[13] It might be suggested that in the situations mentioned, the scientific community did not really accept inconsistencies. Rather, they had degrees of belief in various propositions, and their degrees of belief in the contradictory α and $\neg\alpha$ were less than 1. Now, it may well be the case that there are degrees of belief, and that acceptance is to be understood as having a sufficiently high degree; but this suggestion will not really help. For example, given the inconsistency between the theory of evolution and thermodynamics, it would follow that one or both of these theories was believed to degree ≤ 0.5, which is to say that one or both of these was not accepted at all, which is untrue. Or if both were accepted to degree 0.5, and one sets the level acceptance at this figure, then inconsistencies were accepted, as claimed.

[14] For a general account of paraconsistent logic, see Priest 2002. On non-adjunctive strategies, see, especially, section 4.2.

earth really had a contradictory age.[15] Similarly, in the Bohr theory of the atom, the drawing of conclusions was restricted to well-defined consistent chunks, possibly in accordance with some pragmatically determined—but still determinate—considerations.[16] A conclusion drawn may then have been fed into another consistent chunk which contained information inconsistent with that in the first chunk.[17] Whether adjunctive paraconsistent logics have, historically, ever been used in handling inconsistencies is a different matter, and is rather doubtful. At any rate, I know of no examples where this is clearly the case.

5. ACCEPTING INCONSISTENT INFORMATION

We have seen that inconsistent information has sometimes been accepted in the history of science; we have also seen, at least in outline, why this does not lead to disaster. None of this shows that the inconsistent information *ought* to have been accepted (even provisionally). But the situation seems to have arisen so frequently that it is implausible to level at the scientific community charges of blatant and frequent irrationality. This, therefore, raises the question of the conditions under which it is reasonable to accept an inconsistent theory or other body of information.

The question of what makes it reasonable to accept any theory is a familiar one in contemporary philosophy of science, and a very difficult one. Neither do I intend to advance a detailed answer to that question here. One thing that is, I think, fairly universally agreed is that the doxastic goodness of a theory may be evaluated under a number of orthogonal criteria. Let met explain.

A major criterion is empirical adequacy. For any observational consequence, α, that a theory entails, if α is observed, this is a good mark. If it is not, this is a black mark. Empirical adequacy is, perhaps, the most important criterion in science. It is, at least, the one that mainly distinguishes the empirical sciences from similar investigations. But it is certainly not the only one; nor can it be. It is at least theoretically possible to have different theories that are both empirically adequate; more commonly, it happens that no theory in the field is entirely empirically adequate. Hence other criteria have also to be employed. There is a multitude of these;[18] philosophers may disagree both about what they are, and about how, exactly, to understand them, but the following have certainly been suggested, and are very plausible. Good-making features include: simplicity, ontological leanness (Ockham's razor), explanatory power, a low degree of *ad hoc*ness, unity, fruitfulness. The converse features are bad: complexity, ontological extravagance, explanatory poverty, much *ad hoc*ness, fragmentation, barrenness. I have not

[15] Which is not to say that people did not notice that the theories together entail something of the form $\alpha \wedge \neg\alpha$, and so conclude that there was a problem here; merely that the corpus of accepted beliefs was not closed under an adjunctive logic.

[16] Many non-monotonic (paraconsistent) logics incorporate pragmatic features of this kind; for example, in the ordering relation with respect to which minimisation is defined. See Priest 1999b.

[17] For a more detailed discussion, see Smith 1988, Brown 1990 and, especially, Brown 1993. See also the discussion of the inconsistent early quantum theory of black body radiation in Norton 1989 (esp. pp. 330ff.).

[18] See, e.g., Quine and Ullian 1970, ch.5, Kuhn 1977, and Lycan 1988, ch.7.

mentioned the pair consistency/inconsistency in these lists, though they are frequently put there. For the moment, let us grant this: I will come back and examine (and qualify) the situation in a moment.

The exact number of, and details concerning, criteria of these kinds, though a highly important and interesting question, need not detain us here. The important points are (a) that there is a multitude, and (b) that the criteria do not necessarily hang together. One theory, say Bohr's theory of the atom, may have a high degree of empirical adequacy, be very fruitful, but inconsistent. Another may be consistent, have a lesser degree of empirical adequacy, and be rather *ad hoc*. In such circumstances, when is one theory to be rationally preferred? When it is clearly better than its rivals. And when is this? When it is sufficiently better on a sufficient number of criteria. This is all very vague. Perhaps ineradicable so. It may be tightened up in a number of ways,[19] but this is unnecessary here. The rough qualitative account is sufficient to demonstrate a number of things. It shows why theory-choice is a messy business: there is no simple algorithm. It shows why, within certain limits, there may be room to disagree over which theory is better: if no theory is overall best, people may reasonably disagree. All this is familiar from standard philosophy of science. Perhaps less familiar, it shows how and when it may be rational to accept an inconsistent theory: when, despite its inconsistency, it is markedly better than its rivals on sufficiently many other criteria. Finally, it shows when it may be right to reject an inconsistent theory, even when inconsistency may be rationally tolerable: when a rival theory scores higher on sufficiently many criteria. That is, it shows how theories may be 'falsified', even if inconsistencies are sometimes tolerable.

6. INCONSISTENCY AND TRUTH

But should an inconsistent corpus of belief be accepted only provisionally, until a better one can be found; or can it be accepted as a candidate for the final truth? Several comments are pertinent here. First, there is no such thing as certainty about anything in science. Any theory or set of theories, whether consistent or inconsistent, should be endorsed fallibly. All theories go beyond the data—which is itself, in any case, 'theory laden'. In this sense, the acceptance of anything is only ever provisional.

But is there something special about inconsistency in this regard? Here, it seems to me, the nature of the inconsistency is relevant. Note, first, that if a theory is empirically inadequate, however acceptable it is, the received information is not a candidate for the truth. If a theory entails an observable consequence α, and α is not perceived, something is wrong, either with our theory or with our perceptions; something needs to be fixed. In particular, then, if a theory entails $\beta \wedge \neg\beta$, where β is some observation statement, then if such a contradiction is not observed, the theory cannot be correct. As I have already argued, $\beta \wedge \neg\beta$ is a perfectly observable state of affairs. Moreover, if the inconsistency in the scientific corpus is between a

[19] See Priest 200a.

theory and an observation, something needs to be revised. For if theory T entails α, but $\neg\alpha$ is observed, not α, we again have an empirical inadequacy. It may be retorted that if inconsistencies are acceptable, maybe $\alpha \wedge \neg\alpha$ is true after all. But again, since $\alpha \wedge \neg\alpha$ is an observable state of affairs, and one that is not observed in the situation described, we have an empirical inadequacy: if $\alpha \wedge \neg\alpha$ were true, so would α be, and this is precisely what is not observed.

What if a contradiction is one between theory and theory, or internal to a theory, not spilling over into observation? Here, the situation is more complicated. Suppose, first, that one is an instrumentalist; then all one cares about is the empirical adequacy of a theory; if a contradiction is located deep in the heart of theory, this is of no moment. But if, as I think correct—though I shall not argue it here—one should, in general, be a realist about scientific theories, the matter is different, and depends crucially on how this inconsistency is handled. If it is handled by a chunking strategy, then the theory is not a candidate for the truth. If α is true and $\neg\alpha$ is true, then so is their conjunction. If a theory refuses to allow this move then the theory cannot be correct, and we know this.[20]

If, on the other hand, the inconsistency is handled with an adjunctive paraconsistent logic, there is no reason, as far as I can see, why we should not suppose the theory or theories in question to be correct. In particular, then, a theoretical inconsistency that is handled adjunctively is not, in itself, a negative criterion for acceptability. Any argument to the effect that such inconsistencies are ultimately unacceptable must be a quite general and *a priori* defence of the Law of Non-Contradiction: any contradiction is known, in advance, to be a sign of untruth. This is not the place to discuss the issue; but let me state, for the record, that I know of no such argument that works. All fail, usually by simply begging the question in some way.[21] Thus, if we are realists, we will let our best theory, provided that it is not ruled out as a candidate for truth on other grounds, inform us as to what reality is like; and if our theory is inconsistent, there is no reason to suppose that the theory does not get it right: reality itself is inconsistent. In other words, inconsistencies of *this* kind in science do not mandate that the acceptance of the theory or theories in question be provisional in any special way.[22]

7. INCONSISTENT MATHEMATICS

It is here that the impact of paraconsistent logic is revisionary—indeed, revolutionary. The Law of Non-Contradiction has been well entrenched in Western thought—and so science—since the canonisation of Aristotle, whose defence of the

[20] More generally, if the inconsistency is handled with an inference mechanism that does not respect truth-preservation, the same conclusion follows.

[21] The Law of Non-Contradiction is taken on in Priest 1987. A number of other arguments for the Law are discussed and rejected in Priest 1998.

[22] This raises the following question. Suppose that we have a theory based on a non-adjunctive logic. This, as I have argued, is not an ultimately acceptable theory. Why can we not turn it into one, simply by changing the underlying logic to an adjunctive (truth-tracking) one? This may be a possibility, though not if changing the logic results in the theory being empirically inadequate—which is normally why a non-adjunctive procedure is used in the first place.

law has rarely been challenged.[23] Hence, scientists and philosophers have not been prepared to brook the thought that an inconsistent theory of any kind might be true. But subscribing to the Law is not rationally mandatory, as the development of paraconsistent logics has played a large role in showing. Once this fact is digested, scientists may well—justifiably—take a different attitude to inconsistent theories of the appropriate kind. Indeed, they may even develop inconsistent theories, if these have the right empirical consequences, just as paraconsistent logicians have articulated inconsistent theories of semantics to handle the paradoxes of self-reference.

In modern science, the inferentially sophisticated part is nearly always mathematical. An appropriate mathematical theory is found, and its theorems are applied. Hence, a likely way for an inconsistent theory to arise now in science is via the application of an inconsistent mathematical theory. Though the construction of inconsistent mathematical theories (based on adjunctive paraconsistent logics) is relatively new, there are already a number: inconsistent number theories, linear algebras, category theories; and it is clear that there is much more scope in this area.[24] These theories have not been developed with an eye to their applicability in science—just as classical group-theory was not. But once the paraconsistent revolution has been digested, it is by no means implausible to suppose that these theories, or ones like them, may find physical application—just as group-theory did. For example, we might determine that certain physical magnitudes appear to be governed by the laws of some inconsistent arithmetic, where, for example, if n and m are magnitudes no smaller than some constant k, $n + m = k$ (as well as its being the case that $n + m \neq k$).[25] There are, after all, plenty of episodes in the history of science in which we came to accept that certain physical magnitudes have somewhat surprising mathematical properties (being imaginary, non-commuting, etc.). Why not inconsistency? Which is not to say that an inconsistent mathematical theory *must* be interpreted realistically. Such theories may have instrumental uses, just as much as consistent theories.

8. CONCLUSION

I believe that the development of modern formal paraconsistent logics is one of the most significant intellectual developments of the 20th century. In challenging entrenched Western attitudes to inconsistency that are over 2,000 years old, it has the potential to ricochet through all our intellectual life—and empirical science wears no bullet-proof vest. As we have seen, empirical scientists have always

[23] An analysis of Aristotle's arguments for the Law of Non-Contradiction can be found in Priest 1997.

[24] For inconsistent arithmetic, see Priest 2002, section 9. On inconsistent mathematics in general, see Mortensen 1995.

[25] For a thought experiment illustrating how this might come about, see Priest 200b. There is even one place where an inconsistent mathematics might possibly find an application already. In the two-slit experiment in quantum mechanics, the causal anomaly can be resolved by supposing that the photon does the impossible, going through the two slits simultaneously, and handling this with an adjunctive paraconsistent probability theory. For details, see Priest and Routley 1989a, 377ff.

tolerated, and operated within, inconsistency in certain ways. One of the liberating effects of paraconsistency should be to allow us to understand better exactly how this proceeded. Such an understanding is bound to reflect into our understanding of the rationality of theory-choice, in the ways that I have indicated. Perhaps most importantly of all, paraconsistency may open the gate to important new kinds of theory within science itself. Where this will all lead, one cannot even begin to speculate.

Department of Philosophy, University of Melbourne, Australia

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