Holism, Inconsistency Toleration and Inconsistencies between Theory and Observation

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ABSTRACT

It has recently been argued by Davey (2014) that inconsistency is never tolerated in science, but only discretely isolated. But when talking about inconsistencies in science, not much attention has been paid to the inconsistencies between theory and observation. Here I will argue that inconsistency toleration actually takes place in science, and that when we examine actual inconsistent theories, inconsistencies between theory and observation look anything but homogeneous. I will argue, appealing to certain properties of empirical theories, especially holism, that at least two sub-types of inconsistencies regarding theory and observation can be distinguished: those that satisfy a criterion of observational independence and those that do not.

1. Introduction

In recent years, much attention has been paid to the role and authenticity of inconsistency [1] in science; two different stories have been told about the possibilities and the implications of identifying contradictions in any examples of scientific reasoning.

On the one hand, there is a recurring view in the traditional literature of logic and philosophy of science which holds that, when evaluating scientific theories, inconsistency has to be understood as a death knell for the theory. The idea behind this assumption is that if while examining our empirical theories we

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1 In what follows and unless otherwise indicated, I will use the term 'inconsistent' as a synonym of 'contradictory' for simplicity.

presuppose the basic principles of classical logic (or any other explosive logic), then because of the explosion principle, "an inconsistent theory implies any conceivable observational prediction as well as its negation and thus tells us nothing about the world" (Hempel 2000; 79); which is widely understood as the absolute failure of the theory for scientific purposes.

On the other hand, a more recent view claims that inconsistency toleration in science is not as dangerous as we tend to imagine, and even that it is quite common in scientific activity. This perspective has been enriched by the study of paraconsistent logics and the emergence of case studies from the philosophy of science that seem to illustrate how the presence of some contradictions do not necessarily mean the explosion of the theory in question. The main assertion of those defending this standpoint is that, contrary to what the traditional view might suggest, inconsistent theories do not always have to be rejected (Lakatos 1970, Laudan 1977, Smith 1988, Meheus 2002, Priest 2002).

In favor of the first position, it has been argued that when "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 (...) there is a relatively clear division between the 'solid' part of the theory in which the scientist has justified belief, and the more 'speculative' part of the theory in which the scientist does not" (Davey 2014; 3025). On this account, it seems that whenever an inconsistency is identified we face the dilemma of either being able to separate the 'good' part from the rest of the theory or giving up the theory as a whole. Both horns of the dilemma lead us to deny inconsistency toleration in science.

In addition to this, it is a fact that empirical sciences recognize, through their methodologies, that the role of observation is fundamental in the construction, choice and application of scientific theories. Therefore, if we want to analyze inconsistencies in the empirical sciences, issues linked to observation should not, in any sense, be marginalized; that is, while examining inconsistent empirical theories, we must pay special attention to conflicts between theory and observation. That is the reason why, in this paper I will focus mainly on inconsistencies that involve observation.

I will argue that inconsistency toleration actually does take place in scientific reasoning, and that sometimes we cannot get rid of contradictions, at least not by using Davey's approach. This entails, as I will discuss, that situations in which a clear inconsistency-isolating division cannot be made are the most interesting and revealing ones for the study of inconsistencies between theory and observation.

I will assume that, for defending an interesting philosophical thesis about inconsistency- toleration, it is necessary to provide at least one example from the history of science of an inconsistent theory that although known to be inconsistent, has remained functional. I will also assume that Davey's (2014) main argument denies the existence of inconsistent but functional theories: he commits himself to the possibility of a theory Γ being inconsistent, but holds that once this is known, scientists cut off the part of Γ that causes the inconsistency, call it the *inconsistency-causing part*[2], and continue working with the non-problematic part, saying that Γ-ICP (the original theory, Γ, minus the part of the theory blamed for the inconsistency, ICP) is the real functional theory. So, no actual inconsistent and functional theories exist.

To support these theses, this paper proceeds as follows. In Section 2, I reconstruct Davey's argument against paraconsistent approach to inconsistency management. In Section 3, appealing to the holistic properties of scientific theories, I offer a philosophical response to Davey's main argument. In Section 4, I introduce two case studies, one which is compatible with Davey's position and another which supports my criticism of it. I then offer an explanation of how inconsistencies are sometimes tolerated in science. In Section 5, I argue that some of the inconsistencies between theory and observation look considerably different from each other, I also claim that, when talking about inconsistencies between theory and observation two distinct kinds of inconsistencies can be distinguished: cases where a criterion of observational independence is satisfied and those where it is not. Finally, I outline some conclusions regarding inconsistency toleration in empirical sciences and our way of characterizing inconsistencies involving observation.

2. On Davey's argument

Consistency[3] is a privileged theoretical value: neither an inconsistent set of

[2] I will use 'inconsistency-causing part' to refer to the minimal section of the theory that is believed is necessary and enough for causing the inconsistency, and thus, the part of the theory which is being blamed for the inconsistency.

[3] At least absolute consistency (i.e. not including in the theory every sentence from the language) is certainly privileged and must be, if complete trivialization is to be avoided. I am greatly indebted to the referees for helping me to give a better phrasing of my ideas on this point.

sentences nor they consequences can be trusted. If we want to trust our science, it has to be consistent as well. In his (2014), Kevin Davey argues that, because of their explosive character, inconsistent scientific theories would fail at fulfilling their primary scientific purpose, which is to offer reliable explanations. Thus, they would not be trustworthy in any scientific way and we would not be justified in distinguishing between a theory deficient in this sense and a science fiction story.

However, the paraconsistent tradition (which Davey calls 'counter-tradition') has pointed out several cases where a theory turned out to be inconsistent yet scientists still had confidence in it. And more important, in all those cases, whenever looking at an alleged inconsistent theory, scientist were very capable of distinguishing the theory from a piece of fiction. So, the phenomenon of having trustworthy theories that seem to be inconsistent demands an explanation.

Taking this demand into consideration, it seems that we have to decide whether to trust the classical assumptions that surround contradictions or to trust the examples offered by the paraconsistent tradition and give up our classical commitments. Davey argues that this is a false dilemma, that the case studies offered by the counter-tradition so far only prove that, once faced with an inconsistency, scientists stop trusting the initial theory and separate the good part of the theory from the bad, rendering the new version of the theory consistent.

In what follows I will reconstruct the philosophical argument that Davey (2014) provides, and offer a response to it along with a counterexample to his main thesis; however, I will not discuss any of the particular case studies that Davey offers in his (2014) nor his particular conception of the paraconsistent tradition. Later (in 4.1 and 4.2) I will highlight some of the main issues of Davey's standpoint as well as of Priest's (2002) and Davey's (2014) characterization of an inconsistent empirical theories, in order to draw some conclusions on the role of holism in dealing with inconsistent scientific theories.

2.1 Preliminaries

Davey holds that most of the claims made by the paraconsistent tradition do not really count against the classical perspective and, more important, that they are not supported by any of the case studies provided by this standpoint.

In order to defend his position, he focuses on what he calls the 'Main Proposal of the counter-tradition' which reads as follows: "It is possible for a scientific theory to be logically inconsistent and for us to nevertheless be justified in believing it" (Davey 2014; 3013).

Having stated what he believes constitutes the core of any paraconsistent approach in his Main Proposal, Davey presents a philosophical argument against this broad claim and then inspects, one by one, six case studies that are often taken as examples of inconsistency toleration in empirical sciences. In the end he dismisses their purported support for the counter-traditional approach.

In this section, I will focus on the philosophical argument that Davey offers in his (2014). In order to do so, I will first provide some definitions[4] so as to make it easier to understand the story that Davey is telling us. Then I will reconstruct his argument in a very concise way.

One of the most important concepts involved in Davey′s main argument is "justified belief". Here I will use a broad interpretation of it from a reliabilist standpoint, which goes as follows: one has a justified belief in a theory if and only if the belief is produced by reliable processes, that is, by processes tending to produce mostly true beliefs.

Later, because we will focus on empirical theories, it is important to offer a raw definition of what we will understand by 'empirical scientific theory': following Davey, "we will take a theory to be more or less any set of beliefs about the natural world" (Davey 2014; 3012), and assume that such beliefs could be expressed, in a convenient language, by collections of sentences.

Such a theory could be inconsistent with itself, with other discoveries or empirical descriptions that have been well accepted for its discipline, or with other theories or models of explanation that are well accepted by the relevant community (Kuhn 1977). Similarly, Graham Priest noted that:

> If we distinguish between observation and theory (what cannot be observed), then three different types of contradiction are particularly noteworthy for our purposes: between the theory and observation, between the theory and theory, and internal to a theory itself (Priest 2002; 122).

[4] The definitions presented here are intended to be broad enough to make it easy for both Davey and the paraconsistent approach to sustain their main theses.

Given the Kuhnian interpretation of consistency and the distinctions offered by Priest, an empirical theory would be inconsistent if it satisfies any of the conditions below[5]:

(a) There is an α such that:
$\Gamma \models \alpha$ and $\Gamma \models \neg\alpha$.
(b) Given two empirical theories Γ and Δ:
$\alpha \in Cn(\Gamma \cap \Delta)$ and $\neg\alpha \in Cn(\Gamma \cap \Delta)$
(c) If α is a consequence of Γ, and $\neg\alpha$ is an observation report applying to the empirical domain of Γ, we have:
$\Gamma \models \alpha$,
$\neg\alpha$

Since the following sections will focus on inconsistencies between theory and observation, it might be useful to say a little bit more about this particular type of inconsistency; in particular, that it could also be described as follows:

Theory-observation inconsistency: There is an empirical theory Γ, (where Γ has been well received by the scientific community) that has α as an observational consequence; and an experiment is made which leads to a report that $\neg\alpha$.

This type of inconsistency is generally tagged under the term *anomaly*, and it has been argued that neither Γ nor $\neg\alpha$ should, necessarily, be abandoned; but rather, in most of the cases they're accepted *pro tem* while a new theory (that can accept $\neg\alpha$) or better instruments are designed which show that the result, $\neg\alpha$, was an observation error. Some examples of this type of inconsistencies are the precession of Mercury's perihelion and Prout's hypothesis (Laudan 1977).

Many more things can be said as regards to all these notions, but for the purposes of our discussion, this will suffice. Now, let us examine Davey's main argument.

2.2 Davey's argument in detail

From the outset, it should be highlighted that Davey's main concern is not to

[5] I will say more about this taxonomy in 5.1.

deny that inconsistencies sometimes appear during the development of some scientific theories. Rather, he aims to show that whenever a contradiction arises in a theory in use, scientists are able to choose and to trust only the consistent version of the theory, as inconsistency toleration not an option in the empirical sciences. In order not to miss this point, I will from now only concentrate on empirical theories that could be considered as *functional* ones.

Functionality requires the following aspects: the theory should describe the external world in a way that helps the pertinent scientific community to describe, explain, predict, measure or experiment with phenomena that correspond to a particular empirical domain. To be considered as functional, an empirical theory has also to possess other 'characteristics of a good scientific theory', such as simplicity as well as others that have been described by Kuhn (1977).[6] To this very last part, I will add an instrumentalist condition, meaning that a fruitful theory (typically) should be actually used in science (within the theory′s orginial discipline and related areas of knowledge).

All this said, we can finally focus on Davey's argument; his main thesis can be expressed through the following six points.

First of all, we want our scientific theories to be able to give us information about the external world, information that can help us to measure, predict, anticipate, and modify some aspects of particular empirical domains (Hempel & Jeffrey 2000). For these theories to allow us to do so, they have to offer explanations and also guide us getting[7] about the studied domain. As a matter of fact, giving explanations (and predictions) is the main goal of a scientific theory, without predictive or explanatory power an empirical theory would not be anything but a collection of sentences that talk about empirical entities in the same way they could be talking about falsehoods.

Secondly, if the predictions of a theory are fulfilled and explanations actually help us to understand in a better way the empirical domain that we are studying, one can assume that the belief in the theory is justified (Davey 2014; 3012). For Davey, the bona-fide consequences of the theory are the only thing

[6] A theory "should be simple, bringing order to phenomena that in its absence would be individually isolated and, as a set, confused. (...) [A] theory should be fruitful of new research findings: it should, that is, disclose new phenomena or previous unnoted relationships among those already known." (1977; 322).

7 For the sake of the argument, I will not assume the symmetry between explanation and prediction asserted by Hempel (1965). Nowadays, some of our theories in use do not give a large number of explanations, but they indeed offer a large number of reliable predictions, and vice versa; thus, to reject the symmetry between both of them, will allow us to consider a greater number of functional theories.

that makes the theory different from a science fiction story; it is in this sense that, if a theory fails to give reliable predictions or explanations, it irremediably lacks scientific character.

Thirdly, Davey emphasizes that all of the sentences that form an empirical theory are needed and used for explaining or predicting some relevant phenomenon. Thus, for Davey, if the explanation ends up being trustable, the sentences involved are trustable too, and if all the explanations and predictions of the theory come out to be reliable, the theory is reliable as well. "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 object of justified belief (...) a theory in which we do not have a justified belief is deficient in the sense that cannot be used for the purpose of explanation"[8] (Davey 2014; 3013).

Fourthly, Davey argues against inconsistency in at least two ways. On the one hand, he says that "because it is impossible for all the elements of a logically inconsistent set of sentences to be true, (...) a logically inconsistent theory is false" (Davey 2014; 3010); in this sense, if the theory is false, it would mean that some of its predictions or explanations are false as well; ergo, the theory is an unreliable one. On the other hand, "[a]ccording to the classical consistency presupposition, contradiction have an explosive character: wherever they are present in a theory, anything goes, and no sensible reasoning can thus take place" (Marcos 2005; xv).

If assumed both the possibility of suitably formalizing empirical theories and the constraints of classical logic, a set of beliefs expressed as a collection of sentences will explode immediately once a contradiction is added. If explosion is reached, triviality is warranted. In this sense, an inconsistent empirical theory is an uninformative theory that does not say anything trustworthy about the world and, if that is the case, it seems quite obvious that this inconsistent theory should be immediately rejected.

Fifthly, Davey points out that because of the negative connotation of inconsistencies, in practice, scientists always feel the need of finding a way to

[8] This suggests that it is not possible to provide satisfactory explanations using sentences that we do not take to be true, which clearly conflicts with our constant use of simplified scientific models. As a matter of fact, scientists usually use known falsehood in their predictions or explanations without taking such falsehoods to be true, but (in general) seem to hold that they can still be relied on when reasoning about particular applications, so success does not always turn on belief. I will not try to argue against it in this section; however, I will say more about this issue in Sect. 3.1.

avoid contradiction; as a matter of fact, to avoid contradiction; as a matter of fact, in the most popular cases that the paraconsistent tradition has offered, this is quite clear. Once the scientists face inconsistency, they find themselves no longer justified in believing the inconsistency-causing part of the theory; for instance, when a theory's prediction fails, the scientists stop trusting both the subset of sentences involved in the entailment of this prediction and the subset of sentences involved in the construction of the observational report, leaving the still reliable part of the theory consistent; from now on, we will call this *inconsistency isolation condition.*

According to Davey, this condition will be satisfied if and only if, it is possible for scientists to identify satisfactorily a part of the theory as the 'source' of the inconsistency and also if it is possible for them to isolate this part in such a way that the inconsistency can somehow be dispensed with or avoided.

Finally, when discovering an inconsistency in our empirical theories, Davey says that, in order to fulfill the points listed below regarding reliability in science, we have two alternatives: either we select some consistent part of the theory and judge ourselves justified only in trusting that part or, in case we cannot make the "cut", we abandon the theory as a whole, appealing to our classical presuppositions. Ergo, no inconsistency is actually tolerated in science.

Now, one can reply to Davey's argument in at least two different ways: one can object to his classical commitments, or one can object to the third and fifth parts of his argument (regarding reliability and the actual possibility of separating the consistent part of the theory from the inconsistency-causing part). In this paper I will argue that, even accepting Davey's classical commitments, his argument is not solid enough to rule out inconsistency toleration in science. In the next section I will offer some philosophical reasons that I hope could be supported by a case study offered in Sect. 4.2.

3. About theories and holism: in favor of inconsistency tolerance

My central commitment in this section is to discuss some implications of Davey's inconsistency isolation condition presented in the previous section, when facing inconsistencies in empirical sciences. First, in order to narrow the notion of 'empirical theory', I will introduce an argument against Davey's naïve description of how scientific theories work; later on, I will argue that, even with a more sophisticated notion of 'theory', the inconsistency isolation condition cannot be fulfilled because it ignores from the outset the holistic properties of standard empirical theories.

3.1 On the relations between theories and justified beliefs

If we assume an empirical theory to be exclusively a set of statements about an empirical domain, which could be object of justified belief depending on the predictive and explanatory success of the theory itself; and if nothing else is said about differences between these beliefs, then we might be facing the following situations:

First, many different types of statements are often involved in actual empirical theories seem to be diverse. The sentences that express our theories can have the form of general laws, auxiliary statements, empirical constraints, etc.; and if we ignore this, we are ignoring how actual theories really are (Kuhn 1970).

Later, in actual scientific practice scientists sometimes tend to add "false elements" to the theory in order to get more accurate predictions or to make the derivation of predictions more simple –for instance, sometimes they tend to treat the Sun and Earth as the only members of an isolated physical system, something that we all know, is false (Putnam 1981).

Finally, if we do not distinguish between different types of propositions involved in our empirical theories, we are ignoring one of the main characteristics of scientific theories in general. Nevertheless, that is not the only difficulty that we will be facing, if we do not make any distinction between the uses of sentences of a theory, appealing for instance to their particular purposes, then it will be very hard to distinguish between the different ways in which we can achieve justification for our beliefs. For instance, scientists are often justified to believe some assumptions but not because they are supposed to be true, but because they are strongly successful.[9]

In cases where false statements are used and corresponding predictions are successful, then – as a consequence of Davey's commitments – one has to rely on all the sentences that played any role in the development[10] of the predictions in question, including the false assumptions. And more importantly, if we state that reliability comes only from corroboration, it is well known that it is impossible to

[9] Some scientific models are good examples of this, they are successful most of the time but they are also known to be not-true, and so if scientists believe such models, they cannot believe them in the sense to be true.
[10] This does not mean that scientists rank all the sentences equally, nor that all of them are literally believed.

corroborate all the logical consequences of empirical theories (Putnam 1981), which would make it impossible to actually be justified in believing in any empirical theory complex enough to describe a relevant empirical domain.

Thus, if we want Davey's argument a well as our reply to hold for actual scientific theories, we have to recognize at least that, sometimes due to the diversity between the components of our empirical theories, justification does not come directly from predictive or explanatory success, and more important, that sometimes scientists are justified to believe some assumptions because they trust their pragmatic benefits but not anything similar to their "truth".

3.2 Holism and the withdrawal of the inconsistency isolation possibility

It seems right to say that, in many of the possible case studies that we could analyze while looking for inconsistencies, the distinctions expressed above have to be made in, at least, a general sense.

However, the differences between statements and ways to achieve justification are not the only problem that Davey's standpoint faces; there is indeed a more challenging situation about his stance: in some cases the holistic nature of empirical theories will not allow the inconsistency isolation condition to be fully satisfied–at least not in the way Davey said that it would. This does not mean that we assume that the holistic properties of empirical theories will always make impossible to isolate the problematic parts of a particular theory. What is claimed here is that sometimes it is too complicated, or even contextually impossible, to separate satisfactorily the parts of a theory blamed for the inconsistency once an inconsistency is recognized. Let me press further this point.

In 1906, while talking about crucial experiments, Pierre Duhem pointed out that once a hypothesis fails or a prediction cannot be corroborated, in principle, it could be too difficult to identify clearly and precisely where things went wrong:

> A physicist decides to demonstrate the inaccuracy of a proposition; in order to deduce from this proposition the prediction of a phenomenon and institute the experiment which is to show whether this phenomenon is or is not produced, in order to interpret the results of this experiment and establish that the predicted phenomenon is not produced, he does not confine himself to making use of the proposition in question; he makes use also of a whole group of theories accepted by him as beyond dispute. The prediction of the phenomenon, whose nonproduction is to cut off debate, does not derive from the proposition

> challenged if taken by itself, but from the proposition at issue joined to that whole group of theories; if the predicted phenomenon is not produced, not only is the proposition questioned at fault, but so is the whole theoretical scaffolding used by the physicist. (...) The physicist may declare that this error is contained in exactly the proposition he wishes to refute, but is he sure it is not in another proposition? (Duhem 1991; 185)

This means that our empirical theories are shaped not only by different types of statements, but also by a diversity of connections between these statements, and so when an anomaly takes place, it is common that scientists are not sure about which part of one's network of hypotheses is to blame for an anomaly. In addition, sometimes when we identify an assumption that contradicts another one or a prediction that is incompatible with an empirical report, the problem does not lie only in the propositions known to be in explicit conflict; it could be bound to many more parts of the theory that sometimes are not easy to identify and that are also justified by appeal to statements that we fully trust (despite their contribution to the now-recognized inconsistency).

Nevertheless, considering the way in which holism has been described here, Davey might reply to our objection by saying that even if it holds, its application is restricted only to the analysis of the internal relationships of our empirical theories; which means that sometimes it could be too complicated to locate and isolate internal inconsistencies and all the elements involved in their derivation, especially because we can compromise much of the theory in question while cutting out all the problematic part of the theory.

Yet, the landscape for external inconsistencies must be different: once an inconsistency, whether with observation or between theories, is identified, the price to be paid has to be minimal. For instance, when scientists realize that there is an inconsistency between particular predictions and observational reports regarding a planet's orbit, they know that if they isolate the elements that involve the behavior of that exact planet they would be able to get rid of the inconsistency, which –on Davey's view–seems to work just fine.[11]

To that point, I will only say that when talking about holism, two main aspects have to be considered: internal holistic relationships and external holistic

11 In Section 4 I will offer an example of how Davey's inconsistency isolation condition works while facing an inconsistency between theory and observation.

relationships. On the one hand, as was pointed out through Duhem's quote, "the theoretical description of any (physical, economic, etc.) system generally involves an extensive complex of hypotheses. This complex includes the basic principles of one more empirical theories, especial laws, auxiliary hypotheses, boundary conditions, etc. Although the emergence of a conflict with the available data basis means that this complex has failed as a whole, it is by no means clear which of these components is at fault and needs to be modified" (Gähde 2002; 69, 70).

On the other hand, in science, different disciplines and research domains are never completely independent of each other. Thus, it must be recognized that holism will not only apply to elements of one isolated theory –because such a thing as an isolated set of beliefs is more likely to be the exception to the rule in science:

> [T]he theoretical description of a system rarely takes place in isolation, but is instead correlated to the theoretical description of other systems in multiple ways (...) These correlations are of major consequence in the event of a discrepancy between theory and observation. If such a conflict arises, modifications need not necessarily start in the theoretical description of the system where the conflict was observed. Instead, correctional attempts may start with the theoretical treatment of some other system correlated to the first (Gähde 2002; 69, 70).

This means, in general terms, that when a problem, such as the presence of a contradiction, is discovered in a theory, to make any modification to the theory in order to fix it–adjustments like inconsistency isolation- will require indirectly modifying not only other parts of the theory itself but also parts of other related bodies of knowledge, and even then it would not be known for sure that the inconsistency has been removed. So, while talking about inconsistency detection, appealing to to holistic properties sometimes present in empirical theories, it will have to be said that sometimes the degree of holism of the theory does not allow the cut to be made, and if it is wanted to keep the theory in use, then the inconsistency would have to be tolerated at least temporarily[12].

However, Davey might reply to our characterization of the *inconsistency isolation condition* by saying that he is talking about something less strong than

[12] A similar stand point is presented in (Bueno 2006) and (Priest 2002); see (Vickers 2013, Chap. 8) for a critical voice.

that: that when finding a theory Γ to be inconsistent in certain context, we stop trusting its consequences regarding that particular framework and we understand that Γ is not functional in that exact context, we (might) reject the particular inference(s) in that exact context as unreliable, while maintaining that the theory is reliable in other contexts. Now, it seems that Γ would be considered as a functional theory, if we can take off the scenarios where the theory lacks consistency.

So here we have one answer Davey could give to us. Yet, the cut seems to be arbitrary: if it was initially expected that the theory would give an account of this particular context, the failure of consistency does not seem to be a genuine scientific criterion for excluding the application of Γ in this particular context as a legitimate application of the theory. Then, unless Davey gives more information about how this separation has a scientific justification is not merely an *ad hoc* type of adjustment, then, if what has been said here is along the right lines, Davey's criterion for the isolation of inconsistent application contexts lacks a satisfactory scientific justification.

Summing up, when talking about scientific theories in a more accurate way, it is necessary to consider the different types of statements that shape our theories and the way they are connected to each other. When dealing with inconsistencies in empirical sciences, it is initially required not to forget the possibility of holism linked to our empirical theories. The natural question at this point is whether there are any historical cases that illustrate the kind of holism that we have characterized above.

In what follows, I will present two different case studies: the first one, offered by Davey, will be useful to understand how the inconsistency isolation condition works; while the second one will help us to illustrate the point that, if a theory possesses a substantial degree of holism, then Davey's account of how inconsistencies are avoided cannot hold in general and inconsistency must sometimes be tolerated.

4. Case studies and inconsistency toleration

To this point, the arguments presented above provide theoretical reasons for inconsistency toleration, but in philosophy of science, historical evidence supporting the thesis in question is always welcome. Providing such historical data is the main purpose of this section. As I have already said at the end of Sect. 2.2., to sustain that the inconsistency isolation condition can actually be imposed in science, Davey would have to offer at least some historical and relevant cases that support his thesis. In 4.1. I will present one of the six case studies offered in (Davey 2014). I will provide an introduction to the anomaly in the precession of the perihelion of Mercury and then reconstruct Davey's argument in favor of considering this historical case as evidence for his thesis. In 4.2. I will introduce the anomaly in measurements of the solar neutrino's flux. I will then argue that this case study shows how inconsistency could be (and some times is) actually tolerated in science.

4.1 Mercury's support

According to Kepler's laws and Newton's gravitational theory (including Newtonian mechanics), all the planets orbit around the Sun by following a fixed elliptic trajectory. However, in 1859, (and even though Newton's theory was a very well-received theory), Le Verrier discovered that Mercury's orbit presented a problem: when its orbit was finished it did not return to the same point at the end of each orbit. The French astronomer had noticed that Mercury's perihelion was moving.

The problem rested in the fact that, even though all of the planets present a precession in their perihelion, Mercury's case stood out the degree of this precession. In 1859, Le Verrier announced the difference between prediction and observational reports on Mercury's orbit it lasted 38 arc-seconds per century (Harper 2007; 937). According to Newton's laws, its orbit's ellipse should precess by 432 arc-seconds per century, but in the observation he noticed that it precessed at a rate of 474 arc-seconds per century; in general terms, the relevant theory predicted no precession and could not explain the movements in the orbit of Mercury.

And even though several astronomers offered auxiliary hypotheses to resolve the problem, a good account of this orbit was never obtained from the theory. Given that the difference between the prediction and the observational report was significantly larger than the margin of error at that time, which was determined through the analysis and successful explanation of the precessions of the other planets' perihelions, it is plausible to assume the observational consequence (prediction) of the theory Γ is inconsistent with the observational reports. So, in this case that we have a largely functional but observationally inconsistent theory.

About this case study, Davey is very clear: the discovery of the inconsistency made the scientists stop trusting the theory as a whole, and made them a bit skeptical on the consequences of the theory regarding Mercury's behavior; this would show that the scientific community was not willing to tolerate inconsistency in any sense. In addition, Davey states that the presence of new auxiliary hypothesis offered at the moment, reveals that the scientific community had located the inconsistency-causing part of the theory, which they stopped relying on, and started working exclusively in it in order to fix the problem as soon as possible: "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" (Davey 2014; 3018).

In sum, when Davey analyzes the anomaly in Newton′s theory, he emphasizes that it is very important to notice that the central hypotheses that were offered to solve the problem were the presence of a new planet and the presence of a cloud of dust between Mercury and the Sun. Once one has pay attention to this fact, it is very easy to understand why philosophers like Davey tend to sustain that scientist at the time were, clearly, confident about the theory and cautious about the empirical data that they had regarding Mercury's behavior. More specifically, it seems that these two hypotheses reveal that scientists thought that the information they had about the planet was incomplete, and that once they realized it, they tried to fix it.

While more could be said here, I hope it is clear that Davey's rejection of inconsistency toleration needs the inconsistency isolation condition, and that without it, his thesis is rather weak; I hope also that it is clear enough that inconsistency isolation condition can only hold if it is possible to plainly separate the theory into specific subsets, where one of these subsets contains exhaustively the problematic part of the theory.

4.2 The Solar neutrino anomaly

As I have said in Sect. 3, the holistic properties of a particular theory sometimes make it impossible for Davey's inconsistency isolation condition to hold. In what follows I will offer a case study that, I believe, shows how this could happen.

Neutrinos were introduced in 1930 by Pauli as hypothetical particles that were necessary to account for the reactions that later would be known as 'β-decay". In this kind of decay, particles that lack mass and electric charge (carry ½ unit of spin) are released (Pinch 1986; 50). In 1933, Fermi named these particles 'neutrinos', building the first theory of β-decay based on their existence (Bilenky 2012).

Solar neutrinos are subatomic particles that are generated from the solar fusion; it was believed, that this type of particles has neither electric charge nor mass. For a long time, the greatest evidence of neutrino existence was only circumstantial, and this was the main motivation that the community had for looking for alternative ways to detect neutrinos in a more precise (and direct) way.

In the 1960 scientists felt confident enough to begin a project for the detection and measuring of the solar neutrino flux; this enterprise involved, at least, four distinct areas of knowledge: radiochemistry, nuclear physics, astrophysics and neutrino physics (Pinch 1986; 47); and it required the development of a group of particular theoretical tools.

In 1962, there was not yet a theoretical model that allowed scientists to make calculations about the solar neutrino flux, so John N. Bahcall recognized the need to offer a detailed model about the behavior of the Sun that would enable the scientists to make the flux of solar neutrinos not only measurable but observable as well (Bahcall 2003; 78). In 1963, Bahcall offered the first model that helped to predict the flux of solar neutrinos: that theoretical tool was named 'Standard Solar Model' (*SSM*).

The *SSM* is a theoretical framework derived from the application of laws about energy conservation and transport; this model can be applied to any star that is composed by gas and that has a spherical shape, and that also possess the luminosity, the radio, the age and the composition of the Sun. In general terms, the *SSM* consists of a set of assumptions both theoretical and empirical, that -depending on the interpretation of the *SSM* that is used- could efficiently describe features of a particular empirical domain, in this case the Sun. It has also the capability of providing descriptions of specific phenomena, predictions and guidance for experiments on the phenomena it describes. One its applications is to describe and thus allow scientists to make predictions regarding the flux of solar neutrinos. Therefore, given to our broad conception of empirical theory, we take the *SSM* to be an empirical theory.

By the end of the 1960's, Bahcall offered what he believed was a final version of the Standard Solar Model, which was expected to enable predictions about the flux of solar neutrinos, predictions that could be tested in an intensive way through an experiment that was designed by Ray Davis, and described as follows:

> Because neutrinos are massless (or were thought to be until recently) and

> chargeless particles which only interact via the weak interaction, an experimenter cannot in any way straightforward way ‘see’ solar neutrinos. The presence or absence of neutrinos can only be revealed indirectly with the aid of (a)? sophisticated measuring instrument. In this case the apparatus is rather bizarre: it consists of a 100,000-gallon tank of perchloroethylene (C_2Cl_4 - better known as dry-cleaning fluid), located a mile under the Earth in a disused mineshaft in Lead, South Dakota (...) The C_2Cl_4 contains an isotope of chlorine, Cl-37, with which neutrinos can interact. [1] As a result of the interaction (Cl-37 + v → Ar-37 + e-), a radioactive isotope of argon, Ar-37, is formed. The presence of Ar-37 in the tank is the evidence for the passage of neutrinos. (...) the entities to be observed - solar neutrinos - can only be detected from their interaction with other entities. (...) In practice what happens in that after a period of time (...) the accumulated Ar-37 atoms are extracted from the tanks of cleaning fluid by sweeping it with helium gas (...). The Argon is collected on a super-cooled charcoal trap, and placed in a tiny Geiger counter where it decays with the emission of electrons of characteristic energy (Auger electrons). It is these electrons which the Geiger counter registers. (Pinch 1986; 122f)

Yet, not even the clicks that were reported by the Geiger counter could be understood as the final observational outcome; as a matter of fact, some of these clicks were generated by other sources, so in order to identify the correct measurement of the solar neutrino flux, it was necessary to incorporate into the experiment anti-coincidence devices (highly sophisticated electronic devices) and strategies for the measurement and evaluation of the information produced. The following diagram shows the basic elements involved in this experiment:

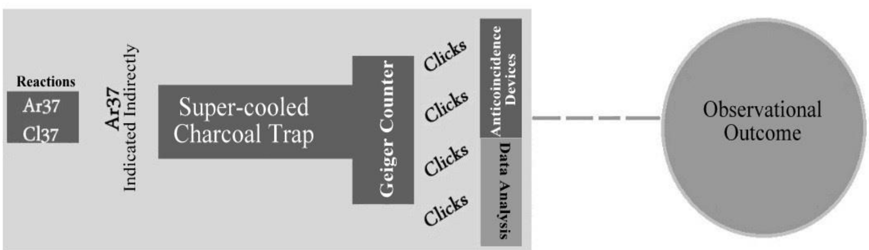

Figure 1: Davis and Bahcall’s experiment

During 1967, Davis (in South Dakota) ran the experiment described; however when the results came out, Bahcall’s predictions turned out to be 2.5 times larger than the results reported by Davis (Bahcall, 2003; 79). Davis

> gave the number of counts he had detected; the number of background counts; the number of neutrino-induced events, $\Sigma\psi\sigma$, (SNUs); and the boron-eight neutrino flux, $\psi_{B\text{-}8}$.(...) he compared Bahcall's latest predicted value of this flux, ($\psi_{B\text{-}8} = 1.4\,(1 \pm 0.6) \times 10^7$ neutrinos cm^{-2} sec^{-1}) with his own observed value, $\psi_{B\text{-}8} \leq 0.5 \times 10^7$ neutrinos cm^{-2} sec^{-1}). His result was so low that it could not be reported as a signal with an error; it had to be expressed as an upper limit. In other words, the neutrino flux could be even lower. (Pinch 1986; 121f)

At this point, Davis and Bahcall did not know where the problem was. While Davis blamed Bahcall's calculations, Bahcall attributed the conflict to the experiment that Davis had directed. In 1968, the two scientists dedicated themselves to check both of the contributions; nonetheless, despite the modifications that were made to both the experiment and the *SSM*, the difference between the predictions and the observational results was still large enough to dismiss a margin of error situation, making the observational outcome impossible to be considered as evidence in favor of (or at least, compatible with) the *SSM*.

Many auxiliary hypotheses were offered to make the theory and observation consistent: first it was said that the experiment relied on the lack of fully reliable information available regarding the cross sections of Ar-37 and Cl-37, which were known with too little precision at the time. That led the scientific community to change the experiment in order to take those elements out of the equation, but it did not change considerably the difference between the predictions and the observational outcome. Another hypothesis was that solar neutrinos were not massless, yet that suggestion was rejected very quickly because a significant part of the scientific community considered it to be conflicting with some basic assumptions of the SSM at the time. A third option implied that neutrinos were nothing more than theoretical entities and were not observable in any sense, that suggestion was rejected because if neutrinos did not exist, the success of the predictions and explanation regarding phenomena as 'β-decay' needed a miracle argument in order to be explained. In addition, the hypothesis of the neutrino oscillation was proposed several times; however, for different reasons (some experimental limitations, and conflicts between the hypothesis and some basic assumptions of the theory), this thesis was dismissed few times before it was finally accepted. Finally, in the 1990's the hypotheses of neutrinos being of different types and having mass were considered as serious candidates for explaining this phenomenon [13] . These were indeed the modifications that in the long run helped to solve the anomaly in 2001[14].

4.3 On how inconsistency does not necessarily mean disaster

Since the main goal of introducing this last case study was to offer an example of an inconsistent theory complex enough to prevent the inconsistency isolation condition from being satisfied, it seems right to argue, first, in favor of high level of holism of the theory itself, and later, in favor of the idea that inconsistency toleration actually took place in the case of the anomaly of the measured solar neutrino flux.

First of all, the *SSM* is a complex body of statements of different kinds. On one hand, it includes very general assumptions including the law of energy conservation, which scientists usually trust unconditionally. On the other hand, the *SSM* also includes very specific statements, such as particular empirical descriptions and statements regarding observational characteristics of the Sun, which most of the time, scientists are not fully committed to. This shows the diversity of components that shape our empirical theories to which we referred in the previous section.

Secondly, if we look at the different hypotheses that were offered in order to solve the anomaly of the measured solar neutrino flux, we will realize they were not focused on a single aspect of the theory; as a matter of fact, some of them were about the *SSM* and others suggested that the inconsistency was related to the experiment (materially and theoretically) in terms of its execution and its design.

Thirdly, as has been said above, since it was very difficult for the scientific community to point out where the inconsistency was originated –they did not agree for long time which part of which theory had to be modified–it was impossible for them to satisfactorily isolate the inconsistency-causing part of the theory. Different scientists had different ideas about which part of the theory or of the experimental elements needed to be rejected, and each of them did propose different isolations; however, it seems that for several years, none of

[13] In recent works by Takaaki Kajita and Arthur B. McDonald it has been shown that the characterization of neutrinos as massless was mistaken; basically because for neutrinos to change flavours, they have to possess mass.
[14] More can be said regarding this case study, but for the main purpose of our discussion this will be enough. For a more comprehensive reconstruction of the problem, see Bahcall (2003) and Franklin (2003).

those cuts was really successful, in the sense that none of them was able to prevent the inconsistency from reemerging.

However, none of this made scientists stop trusting the theory as a whole and shelve it, nor made did it make them give it up. As a matter of fact, they kept using it in order to give explanations and predictions regarding the behavior of the Sun, including information about solar neutrinos.

Finally, it was far from clear in this case where the problem lay, whether it was related to, say, the instruments, the SSM, models of the flux and how it interacted with the equipment, or to our understanding of particle physics. In the light of this case, the suggestion that inconsistencies in science are, in general, avoided by giving up, in an agreed way, some of the commitments that gave rise to them, seems very hard to defend.

If what has been said here is along the right lines, this particular case study illustrates the holistic internal and external relationships of our empirical theories. This historical case is an example of how sometimes holism could make it too difficult for the scientific community to satisfactorily isolate an inconsistency and how an inconsistency may not be as destructive as it is supposed to be. More importantly, this case study also displays how inconsistencies could be tolerated for long time (in this particular scenario, around 30 years) without exploding and destroying the theories involved for good. Yet, that is not the only thing that this case study reveals; in the next section I will address one last issue that the anomaly of solar neutrinos exposes.

5. Inconsistencies between theory and observation: some considerations

So far, I have presented a case that problematizes Davey's'isolation and excision' response to inconsistency in science. In addition, I have suggested that sometimes inconsistency toleration is the only option in science, especially when a high level of holism is present; yet, this one is not the only thesis that could be supported by these case studies.

In what follows I will argue that while talking about inconsistencies between theory and observation, and because the holistic properties of the theories cannot be ignored, the division offered by Priest (2002) becomes too abstract to highlight any relevant aspect of the conflict in question, and too naïve to allow the identification of pertinent similarities between different case studies – at least regarding conflicts between theory and observation. In Sect. 5.2, I will argue that if what has been said is along the right lines, it is possible to recognize at least two subtypes of inconsistencies between theory and observation.

5.1 On how not to separate inconsistencies

Prima facie, the anomaly in the measuring of solar neutrinos flux might be considered as an inconsistency between theory and observation (Sect. 2.1), but things are a bit more complicated than that. This particular type of inconsistency involves exclusively a theoretical consequence, α, and an observational outcome, $\neg\alpha$.

For instance, regarding the anomaly in the precession of the perihelion of Mercury: the large difference between the prediction's results and the observational outcomes made it possible to take the first as α and the latter as its negation. It was also a fact that the theories behind the design of instruments used for observing the phenomena (specifically the telescope) did not include the basic and relevant assumptions of the theory in question.

However, the anomaly in the measuring of solar neutrinos flux shows that sometimes it is impossible not only to identify the origin of the conflict, but also to isolate the theory in question from the other auxiliary theories that were involved (either in the design of the experiment or in the interpretation of results).

As a matter of fact, the *SSM* involves theoretical elements of distinct disciplines: radiochemistry, nuclear physics, and astrophysics, among others. At the same time, the experiment designed for measuring the solar neutrino flux, takes basic assumptions of the same areas of knowledge; meaning that, even though the experiment designed by Davis does not assume completely and explicitly the theory in question, it is possible to find basic (and relevant) assumptions that are shared by the experiment and by the *SSM*.

This entrenched relationship between theories, conjoined with the interpretation of this particular case study as an inconsistency, challenges the characterization of empirical inconsistent theory offered in Sect. 2.1. The main issue is that this inconsistency is neither a clear instance of a conflict between theory and observation (at least not in the sense defined previously), nor a clear instance of a conflict between rival theories; as a matter of fact, if this conflict involved two theories, these would not be rival ones, but one would be an auxiliary to the other.

This suggests that the division between types of inconsistencies and the characterization of them presented by Priest (2002) and Davey (2014) is still incomplete and naïve.

First, it does not account for many of the theoretical entities that can be involved when inconsistencies arise in science –as the conflicts between auxiliary theories and observational outcomes or main theories and auxiliary ones, etc.

Second, the taxonomy of inconsistencies still seems to assume a very naïve interpretation of what a scientific theory is, making the possible scenarios few and stunted. For instance, it seems that once we identify any two inconsistencies between theory and observation, they both will look more or less the same; but if we compare the anomalies in the precession of the perihelion of Mercury and in the measuring of solar neutrinos flux, we find very few similarities and many differences. This leads us to question if an understanding of the first can help us to understand the second.

5.2 On how not to unify inconsistencies

I think the unification of inconsistencies between theory and observation under the one and only label of 'anomaly' has been a mistake. Assume that the anomaly in the measuring of solar neutrinos flux is an example of an inconsistency between theory and observation in the same way as the anomaly in the precession of the perihelion of Mercury; both cases could easily give us the feeling of being before of a scenario where the predictions of the theory are contradicted by the observational results. Yet, as I have suggested in 5.1, there are some considerable differences between both cases, the level of holism that applies in each situation, for example.

If we understand an anomaly to be the presence of a statement (generally some kind of observational outcome) such that when combined with a particular theory and with a ceteris-paribus clause the statement becomes a potential falsifying statement for the theory (Lakatos 1978; 40), then both of the case studies presented in Sect. 4 could be legitimately understood as anomalies. Let us assume that anomalies can be of two different types: lacunae shaped (Kuipers 1999, 2000) or logical contradictions (Laudan 1977). Both the anomaly in the precession of the perihelion of Mercury and the one in the measured solar neutrino flux could be positively identified as logical contradictions. However, so far these two particular case studies look more different than similar; here I will try to explore this intuition.

Within this section, I will argue that sometimes holism could lead to the non-satisfaction of an observational independence criterion and that this fact could play an important role when studying inconsistencies between theory and observation. In order to sustain this thesis, I will first look at the anomaly in the precession of the perihelion of Mercury and argue that in this particular case it is possible to point out the part of the theory responsible for the inconsistency mostly because the observational outcome is achieved by using auxiliary theories that have no significant overlap with the theory in question. Later, I will argue that the presence of this type of overlap is the key to understanding the difference between the two case studies presented in Sect. 4.

First of all, if we want to characterize the anomaly in the precession of the perihelion of Mercury (Sect. 4.1) as a clear inconsistency between theory and observation, it is mandatory to discuss in favor of two basic points:

i) There is at least one (good) reason for interpreting as $\neg\alpha$ the observational outcome regarding the orbit of Mercury.
ii) That if (i) is the case and the anomaly of Newton's gravitational theory presents an inconsistency, this involves exclusively an observational consequence α and an observational outcome $\neg\alpha$.

On the one hand and regarding the first of these two points, as has been established in Sect 4.1, appealing to a clear violation of what was considered at the time to be the margin of error (for both, the observations of Mercury's orbit and for the calculations based on Newtonian models). Therefore, it is possible for us to interpret the anomaly in the precession of the perihelion of Mercury as a case where the observational consequence of the theory was α and the observational reports, $\neg\alpha$.

On the other hand and regarding (ii), it seems that in order to clarify the scenario and show that the inconsistency in question involves exclusively a prediction and an observational outcome, it is necessary to incorporate some kind of criterion for the theory-observation relationship:

> **Observational Independence Criterion:** The set of propositions that underlie the design of instruments and methods used to evaluate the observational consequences of Γ, ideally, are achieved totally independently of the propositions belonging to the theory in question.

This condition stipulates that, as far as possible, "something counts as observation more than as an inference when (...) the group of theories in which lies are not linked with the facts about the subject of study" (Hacking, 1996; 214) it is indispensable to discard cases in which the inconsistency comes from the interior of the theory (Γ), or the relation between an assumption of Γ and another one that is used for the designing of the experiment, or the relation between an assumption of Γ and one of the theories used for the interpretation of the observation results, cases that do not fulfill the basic criteria for inconsistencies between theory and observation.

In the case study regarding the orbit of Mercury, it seems that the criterion of observational independence is satisfied because the auxiliary theories involved did not overlap with the tested parts of the Newtonian theory of gravity. Yet, the situation regarding the anomaly in the measuring of the solar neutrino's flux looks a bit different. Let's explore this case.

As I had argued in Section 5.1, both the *SSM* and the design of the experiment involved elements from very diverse disciplines; as a matter of fact, even though the experiment did not explicitly assume relevant parts of the tested theory, it was possible to find basic and relevant statements shared by both the experiment and the theory. Therefore, this particular case study does not clearly fulfill the criterion of independence that has been offered above, but neither does it present an undermining level of ad-hocness which could make us reject either the experiment or the theory. What is happening in this particular scenario is that the level of holism, not only internal but also involving other theories, is high enough to prevent the observational independence criterion being fully satisfied. So, the question is: can we find a way to classify this example as an anomaly and at the same time differentiate it from other types of anomalies such as the one regarding the orbit of Mercury? I believe we actually can do this, and in what follows I will provide an alternative to draw this distinction clearly.

I do believe that both cases are anomalies. I do believe both cases are logical inconsistencies; however, I also believe that they are different from each other and that the difference should not be ignored. I explain this by saying that the holistic properties of standard empirical theories sometimes make impossible for the relationship between theory and observation to fulfill the observational independence criterion. Yet, if the conflict involves observation and if the objects of our interest are inconsistencies between theory and observation, features about observational outcomes cannot be dismissed only because the independence criterion is not thoroughly fulfilled.

As a matter of fact, in the particular case regarding solar neutrinos, the high level of holism between theories is what entails the impossibility of the observation being fully independent of the theory in question. However, holism is characteristic of scientific theories in general, so we face a dilemma: either we must reject some cases as inconsistencies only because the observational outcomes did not come about independently, or we find a way to understand inconsistencies between theory and observation such that leaves room for this kind of historical episode.

If the considerations that I have advanced in this section are correct and and if we want to analyze cases from actual scientific practice, it is mandatory to incorporate in our way to characterize inconsistencies features expressing observational independence (and the lack of it). Following this intuition, I suggest that at least two different subtypes of inconsistencies between theory and observation could be identified:

> **Inconsistency T-O (Indp)***:* Given an empirical theory Γ that has α as an observational consequence, if an experiment is made, $\neg\alpha$ is reported; also, there is an empty intersection between the subsets of the relevant assumptions of Γ and the relevant assumptions of the theory behind the design of the instruments and the design of the experiment.

> **Inconsistency T-O(Aux):** Given an empirical theory Γ that has α as an observational consequence, if an experiment is made, $\neg\alpha$ is reported; yet, there is a non-trivial overlap between the relevant assumptions of Γ and the relevant assumptions of the auxiliary theories involved in making the observation (including the design of the instruments, the interpretation of the observational outcome and/or the design of the experiment).

This does not mean that these anomalies are not logical contradictions, it only means that some logical contradictions that involve conflicts between theory and observation are not as simple as we sometimes tend to imagine, and that in order to study them and get the greatest amount of information about the theories in question and about science itself, we need to be able to differentiate one type from the other. I suggest we do this by appealing to levels of observational independence.

I want to warn the reader that what I had identified as the main difference between these two case studies does not (necessarily) affect directly the structure of the inconsistency itself, yet it seem to play a crucial role in the analysis of the inconsistencies regarding theory and observation and the possible responses to the presence of the inconsistency –it seems clear to me that, sometimes, changes that could be made to remove the inconsistency could affect both parts, the observational results and the theoretical model.

I hope to have shown that many more questions remain to be explored regarding inconsistencies between theory and observation, and also that this insight will stimulate further investigations in this field.

Conclusions

I claim that inconsistency toleration is not only a possibility in science but that sometimes it is also the only option that we have to keep doing science. First of all, I recognize that indeed scientists do tend to clearly identify successful instances of their theories (at least at an empirical level), and that they also tend to be able to show cases of clear failure (rather predictive or explanatory) of their theories regarding specific empirical domains (as Davey seems to suggest along his paper). However, I don't believe that this implies that, when facing an anomaly, scientists can identify the elements (rather theoretical assumptions or empirical commitments) that may be giving rise to the problem in question.

Here we examined an objection offered by Davey (2014) against inconsistency toleration in science. I have said that he proposes that every time an inconsistency is identified, scientists are able to separate a part of theory that is responsible for the inconsistency and to keep working with a consistent, ergo trustable, part of the initial theory, implying that inconsistency is never to be tolerated, but only discretely isolated and excised.

I have dealt with Davey's main thesis in two steps: first, I offered an argument the holistic nature of standard empirical theories, makes it too difficult and indeed almost impossible for most theories to satisfy the inconsistency isolation condition required by Davey. Second, I offered a case study –the anomaly in the measuring of solar neutrinos problem- that supported my argument by showing how if the holism of some theories and the relevant observational practices prevent Davey's strategy from working it does not mean that the theory has to be immediately rejected, but that the inconsistency can be tolerated without destroying the theory in question.

Third, even if the analysis of the precession of Mercury's perihelion offered by Davey was correct, I do not think this happens as often as he seems to suggest. So, I sustained that the anomaly regarding the measuring of the solar neutrino's flux could help us to see how the alleged relatively clear division between the solid and the speculative parts of a theory could be missing.

I noted that if one wants to study inconsistency toleration in empirical sciences some broad philosophical notions have to be narrowed down:

(a) When talking about scientific theories, at any level, it is mandatory to distinguish between the diverse types of statements that shape our theories and also to distinguish between the different ways to achieve justification regarding those statements.[15]
(b) When talking about inconsistencies in science, we cannot ignore the holistic properties of the theories and of the clusters of theories, because –in most cases- they are what pushes us to tolerate inconsistencies.
(c) When referring to the types of inconsistency in science the divisions offered by Priest (2002) and Davey (2014) are too abstract to highlight any relevant aspect of the conflict in question, and too naïve to allow the identification of pertinent similarities between different case studies.

At the end, a comparison between both of the case studies presented here came naturally; the main result of this appraisal was the discovery of the importance of the similarities and the differences between these two anomalies. Both cases illustrate inconsistencies between theory and observation. Nevertheless, the levels of holism presented by each theory and the levels of observational independence are so dissimilar that it seems important to pay attention to them.

I argue that they both are legitimate cases of logical contradictions, in the same degree, but that they should be separated by appealing to the level of observational independence that applies in each case. I maintain that to ignore the holistic properties of the theories and their consequences is to ignore a big part of the object of our study, so even though the lack of observational independence does not make a theory more inconsistent, it has to be incorporated in our way of characterizing inconsistencies involving both theory and observation.

[15] Also, it has to be taken into account that Davey's idea of justification demands a lot, and so it is not surprising that, having set the bar so high, Davey argues there are no 'real' cases to be found. After all, his approach makes 'real' inconsistency in science involving theoretical commitments impossible for any but strong realists.

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