Inconsistencies between Theory and Observation and the Limits of Chunk and Permeate

ABSTRACT: During the last two decades, the emergence of paraconsistent logics as well as the study of some particular episodes from the history of science have motivated the development of formal reasoning strategies that aim at explaining how it is sometimes possible to reason sensibly from inconsistencies without necessarily arriving to arbitrary conclusions. One of such strategies is Chunk and Permeate (Brown and Priest 2004), in which information is broken up into consistent fragments and allowed to move restrictedly from one fragment to another. In what follows, I will introduce a case study from neutrino physics that, I believe, challenges the possibility of using Chunk and Permeate when dealing with some inconsistencies from the empirical sciences.

0. Introduction

It is common to believe that if the basic principles of classical logic (or any other explosive logic) are assumed then, "an inconsistent theory implies any conceivable observational prediction as well as it is negation and thus tells us nothing about the world" (Hempel 2000: 79). So, if any case of inconsistent (non-trivial) science is provided[1], it seems necessary to offer an explanation about how a scientific theory can be inconsistent and not become trivial at the same time; especially on the face of the classical assumption that an inconsistent set of premises leads us to assume any formula as its result.

Some philosophers and logicians of science have addressed this problem by offering reasoning strategies that aim at explaining how it is sometimes possible to reason sensibly from inconsistencies without necessarily arriving to arbitrary conclusions. As a matter of fact, it has been suggested –at least, by the paraconsistent tradition– that some of such strategies could give a good explanation about how scientists could have reasoned from or with inconsistencies in both formal and empirical sciences. Some of the most popular examples of this are the early calculus (cf. Brown and Priest 2004), Bohr's Hydrogen Atom (see Brown and Priest 2015), the Dirac Delta function (cf. Benham et al. 2014), Lobachevsky's model of hyperbolic geometry for indefinite integrals (Friend 2013) or the inconsistencies related to Carnot's theorem (cf. Meheus, 2002).

However, while internal inconsistencies are well-documented in the literature and have been successfully explained by making use of some paraconsistent reasoning strategies, this does not happen with other types of inconsistencies in science, such as inconsistencies between theory and observation or inconsistencies between theories. And this is not a trivial issue. As a matter of fact, because we want our scientific theories to be able to give us information about the external world, no one can deny that empirical sciences legitimize, through their methodologies, the role of observation as fundamental for the construction, choice and application of scientific theories. Therefore, if one wants to analyze inconsistencies in empirical sciences, the aspects linked to observation should not, in any sense, be marginalized. Said otherwise, attention must be paid to inconsistencies between theory and observation while looking at inconsistent empirical theories (even from a formal point of view).

In the philosophy of science, inconsistencies between theory and observation have been standardly tagged as "anomalies" (see for example Laudan 1977; Kuhn 1977; Priest 2002, 2005).

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[1] In what follows, I will assume that all the scientific theories that are interesting for my analysis are, "despite their inconsistencies, markedly better than their rivals on sufficiently many other criteria" (Priest 2002: 125), *i.e.*, they are not-trivial and functional for particularly relevant scientific purposes (Martinez-Ordaz 2014).

Also, it has been suggested that inconsistencies in science could be successfully modeled through the use of some paraconsistent strategies, such as Chunk and Permeate (Brown and Priest 2004, 2015; Benham et al. 2014). Here I will argue that, if we take into account the lack of observational independence of some empirical theories when using Chunk and Permeate (henceforth, C&P) to model inconsistencies in science, the paraconsistent strategy ends up being insufficient to give account of some inconsistencies from empirical sciences.

In order to do so I will proceed in four steps. First, in Sect.1, I will characterize C&P. In Sect. 2 I will address some criticisms to the strategy that were offered by Heyninck *et al.* (forthcoming). Then, along Sect. 3, I will introduce a case study, the anomaly regarding the measuring of the solar neutrino's flux, to illustrate a case of inconsistent science. Later on, in Sect.4, I will explain why some of the peculiarities of empirical theories cannot be taken into account by using Chunk and Permeate and explore one of the limitations of this formal strategy when dealing with inconsistencies between theory and observation. Such an exploration has the purpose of discovering what are the formal elements and requisites that a paraconsistent strategy has to satisfy if one wants to give an account of more complex types of inconsistencies that might not be rare in empirical sciences at all, this will be briefly presented along Sect. 5. Finally, I will draw some conclusions.

1. Chunk and Permeate

Along this section I will introduce the generalities of the paraconsistent reasoning strategy C&P as they were presented in 2004. Later on, I will introduce an independence condition (regarding chunks and permeability relations) and I will argue that it is possible to claim that, when modeling cases of inconsistent science, C&P requires the boundaries of every chunk to be fixed in a neat way, and each of the permeability relations to be selected independently.

1.1. Preliminaries

First of all,

> The enterprise of paraconsistency was designed so as to help cleansing a particular stain, by eschewing the so-called Principle of Explosion:
>
> $$\text{(PE)}\ \forall\alpha\forall\beta\left(\alpha,\neg\alpha\models\beta\right)$$
>
> According to (PE), contradictions are malicious creatures: Whenever they are present in a theory, anything goes, any statement is equally derivable (Marcos 2005: 211)

Taking this into account, many formal explanations of how it is possible to avoid explosion when dealing with inconsistency have been offered through the emergence of paraconsistent logics, and particular paraconsistent strategies, as C&P.

C&P is a (mainly[2]) paraconsistent reasoning strategy that has been used by its main authors to illustrate three different case studies from inconsistent science (the early calculus, the Dirac delta-function and Bohr's Hydrogen Atom), it is expected to be considered as a serious candidate for wider application.

The basic idea behind C&P is to separate a given set of sentences into consistent fragments (henceforth, *chunks*) and let specific information to flow between these chunks. The underlying mechanism does not validate (unrestrictedly) the conjunction of propositions, which

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[2] Even though C&P was initially proposed for working with inconsistencies and preserving classical consistency among fragments of information, it is not necessary to adopt neither classical logic for each fragment nor for every chunk to have the same logic (Brown and Priest 2004:386). Thus, it seems that C&P could model not only cases where inconsistency is faced but also other problematic scenarios (for instance, where vagueness is faced).

assures that, when having contradictory propositions, contradictions cannot emerge; making each chunk to remain all time consistent, even though the general set of sentences can be inconsistent. Only a specific amount of information is allowed to move from one chunk to another, "if *all* the information in one chunk were allowed to flow to another, this would destroy the chunking procedure; so there has to be a limit. Hence, there must be a mechanism for allowing partial flow" (Brown and Priest 2004: 380). This mechanism is what generates the semi-permeability of the chunks, *i.e.*, chunks are separated from each other by membranes that are permeable to sentences of some kind but not to sentences of any kind. Let me say a bit more about C&P's basic elements and mechanisms.

First of all, C&P requires a given a classical language L and a classical consequence relation $\vdash$, where if Σ is a set of sentences given in L, $\Sigma^{\vdash}$ is the closure of Σ under $\vdash$.

(*) Covering: the covering of Σ is specified as "a set $\{\Sigma_i; i \in I\}$, such that $\Sigma = \bigcup_{i\in I} \Sigma_i$, and for all $i \in I$, Σ_i is classically consistent" (idem: 380).

(**) Permeability relation on *C*: ρ is a permeability relation on C if ρ is a map from $I \times I$ to subsets of the formulas of L.

Note that, for C&P, the permeability relation is what defines how information is allowed to flow between chunks and it depends deeply in what the covering of Σ is.

(***) C&P structure: If $i_0 \in I$, $\langle C, \rho, i_0 \rangle$ is a C&P structure on Σ.

Now, if $\Im$ is a C&P structure on Σ, the consequences of Σ with respect to $\Im \models_{\Im}$, are defined as follows:

> First, for each $i \in I$, we define a set of sentences, Σ_i^n, by recursion on n:
>
> $$\Sigma_i^0 = \Sigma_i^{\vdash}$$
>
> $$\Sigma_i^{n+1} = \left(\Sigma_i^n \cup \bigcup_{j\in I} \left(\Sigma_j^n \cap \rho(j,i) \right) \right)^{\vdash}$$
>
> Thus, Σ_i^{n+1} comprises what can be inferred from Σ_i^n together with whatever flows into chunk i from other chunks at level n. Next collect up the results of all finite stages:
>
> $$\Sigma_i^{\omega} = \bigcup_{n<\omega} \Sigma_i^n$$
>
> Finally, $\Sigma \models_{\Im} A$ iff $A \in \Sigma_{i_0}^{\omega}$. The C&P consequences of are thus the sentences that can be inferred in the designated chunk, i_0, when all information of the appropriate form has been allowed to flow along the permeability relation (Ibid:380f).

While more could be said here about the generalities of C&P, I hope it is clear that this strategy works by first, separating (allegedly) inconsistent information into consistent fragments, and secondly, by simultaneously letting specific types of that information to move from one chunk to another while forbidding other types of information to move between chunks. I also hope that it is clear enough that the consistency preservation among chunks is only possible because of the permeability relation that has been introduced above.

1.2. Binary C&P structures

One of the simplest versions of C&P, the one that I will analyze here, involves a *binary* C&P structure, *i.e.* a scenario where there are only two chunks, the *source chunk*, Σ_S, and the *target* *chunk*, Σ_T; and where the information moves only from Σ_S to Σ_T, making the latter the *output chunk*[3]. In addition, it is the permeability relation what allows the information from Σ_S to be aggregated in a controlled way to the information initially contained in Σ_T, as well as it determines how consistency is preserved. If consistency is persevered in the relevant chunks, one can claim that C&P has succeeded at modeling a particular case of inconsistency toleration. Ergo, when examining the efficiency of a specific application of C&P for a binary C&P structure, one should focus only on the target chunk, Σ_T, because is exactly there and only there where consistency is wanted to be preserved after the permeating procedure.

For instance, given a binary structure N on Σ, a target chunk Σ_T and the information α, $\Sigma_T \cup \{\alpha\}$ is consistent if and only if, rather it is not the case for $\neg\alpha$ to be a logical consequence of Σ_T, or if Σ_T is incomplete with respect to α.

Much more could be said about C&P for binary structures but, for the main purpose of our discussion this will be enough. In what follows I will try to say more about the generalities of C&P.

1.3. Independence and C&P

Now, C&P is a strategy that aims at recuperating possible ways in which scientists could react to inconsistency. As a matter of fact, when proposing this strategy, Brown and Priest assume that that when scientists face inconsistencies in science "a common procedure for handling inconsistent information is to break it up into consistent fragments, and then operate within these" (2004:380). So, if that is the case, and if C&P proceeds in that exact way, then C&P could offer an accurate reconstruction of inconsistency toleration mechanisms in science. Yet some non-formal elements are required in order to put C&P satisfactorily into use.

First of all, I hope that it is straightforward to see that in order to use C&P for modeling actual cases of inconsistent science it is required to provide a recipe of how to separate information into chunks (this one has to shed light on the internal relationships of each chunk and their particular type of logical closure) and another recipe to determine how to transmit information from one chunk to another (this one has to include, at least, the specifications needed to determine the permeability relation)[4].

Thus, it seems clear to me that the effectiveness of the permeability relation relies deeply on the capability for chunks to be sharply distinguished from each other from the very beginning. This general idea is what I will call *General Independence-Condition* (henceforth, $\text{GIC}_{\text{C\&P}}$). Let me press further this point.

This condition could then be expressed as follows:

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[3] Where the selection of the output chunk is generally context-dependent. "Thus, for example, there might be two output chunks, i_0 and i_1, and we might look to i_0 if we want the conclusions of the system concerning micro-objects (or of a certain syntactic form), and i_1 for the conclusions of the system concerning macro-objects (or of a different syntactic form)" (Brown and Priest 2004:387).

[4] It is important that these recipes are exhaustively provided from the very beginning because it seems, so far, that C&P does not allow to modify the specific partitioning for a specific C&P structure once it is assigned, and the same could be said about the permeability relation.

GIC$_{\mathbf{C\&P}}$: Given two source chunks, Σ_{S1} and Σ_{S2}, and a target chunk, Σ_T, the boundaries between Σ_{S1}, Σ_{S2} and Σ_T have also to be clearly distinguishable, as well as the permeability relation between Σ_{S1} and Σ_T, and between Σ_{S2} and Σ_T.

However, GIC$_{\mathrm{C\&P}}$ assumes two main types of independence requirements:

Independent boundaries requirement: Given two different chunks, Σ_1 and Σ_2, the limits of each chunk have to be clear, even if some of the propositions that are contained in the first chunk are also contained in the second one.

Independent permeability relations requirement: For any chunk to be eligible for being a *target chunk*, the direction of the flux of information have to be independently determined for each chunk, *i.e.*, the permeability relation of each chunk does not underlie the permeability relation of any other chunk.

Something important to take into account is that, it seems that this GIC$_{\mathrm{C\&P}}$ will be satisfied if and only if, it is possible for scientists (and logicians) to identify satisfactorily the components of the inconsistency and to separate them in different fragments in such a way that the inconsistency can somehow be dispensed or avoided.

In Sec. 2 I will introduce some general criticisms to the paraconsistent strategy, and in Sect. 3 I will try to challenge GIC$_{\mathrm{C\&P}}$ with a more complex inconsistency from empirical sciences. In Sect.4 I will argue that, despite Brown and Priest’s noble ambitions, C&P faces it limits when dealing with a particular type of inconsistencies between theory and observation. In Sect. 5 I will offer some considerations on what is desirable for any paraconsistent strategy to model inconsistency toleration in science.

2. Against C&P

The aim of this section is to analyze some criticisms to C&P that were raised in Heyninck *et al.* (forthcoming). I begin by introducing them in general terms. Then I will focus on each objection separately and I will try to dismiss two of them by arguing that they cannot harm in any (relevant) sense the paraconsistent strategy.

In Heyninck *et al.* (forthcoming) three reasons are put forward against C&P: first, that the generality of the paraconsistent strategy is highly problematic when it comes to determining which of its applications are legitimate and which ones are misleading. Second, that the formal set-up required for a satisfactory application of the strategy depends fully on the `user'. Third, that the permeability relations (as characterized in Brown and Priest 2004) stand in the way of the strategy allowing for a general guarantee of the consistency of a C&P-structure.

2.1. Against Generality

With regard to the first point, the generality of the strategy, Heyninck *et al* say that the problem arises because “we can use C&P-structures for modeling inconsistent theories but we might just as well use it for the emulation of logical pluralism (Priest 2014). However this puts the burden of the application of C&P fully on the `user'” (forthcoming: 9). So, it seems that, because Brown and Priest (2004) did not provide a collection of rules that enables one to know which cases are worthy of being modeled by their strategy and which ones are not; there is no guidance about how to distinguish legitimate cases from misleading cases when using C&P. Moreover, it seems that the strategy is sufficiently general that it can be used to model any sort of inconsistency, making it appear like a legitimate application of the strategy.

However, it seems to me that this is not going to be a problem for C&P (at least, in general terms). On the one hand, it seems that Heyninck’s objection is mostly motivated by the fact that some uses of C&P, especially the one on logical pluralism, seem to be a little *un-natural*. However, it has to be pointed out that while C&P has been used several times for modeling inconsistent reasoning, it has been put into use also in different ways: as a tool for offering rational non-historical reconstructions (Brown and Priest 2004, 2015; Benham et al. 2014), as a tool for offering historically-sensitive rational reconstructions (Sweeney 20013) and as a tool for metaphorically modeling particular scientific episodes (Friend 2014). So, if these distinctions are taken into account, maybe the sense in which C&P was used for describing the logical pluralism scenario (Priest 2014) is different from the one used when describing the early calculus; and thus, with this option still open, Heyninck’s criticism is far from being overwhelming.

On the other hand, while (Brown and Priest 2004) wonder if, given certain cases of alleged inconsistent science, the strategy could model them satisfactorily, Heyninck is concerned with asking a more general question, namely how can we recognize which are the legitimate objects to be modeled by specific paraconsistent strategies. While I think this is a very interesting problem, I do not see why the proponents of C&P should be committed to answering it *a priori*. As a matter of fact, Heyninck’s question could be put forward against most formal strategies. Since it seems not to be a particular problem for C&P it is not clear how it can harm C&P irredeemably.

2.2. Against Freedom

Heyninck’s second objection against C&P goes as follows: “while the definition of a C&P structure still guarantees consistency when partitioning, this is not the case for the permeability relation" (forthcoming; 10). As a matter of fact, because there are no rules about how to sensibly select a specific permeability relation, "the slightest change of the theory might compel us to design a completely new permeability relation" (idem). Thus, it seems that the formal set-up required for a successful application of the strategy depends fully on the user's choice for particular permeability relations, making all this *freedom* to hinder the use of C&P as an inconsistency handling device.

Nevertheless, I believe that this freedom, instead of being a disadvantage for C&P, could be seen as one of the main philosophical benefits of the strategy. Let me press this point further.

First of all, when formal strategies are expected to put into use for modeling actual scientific reasoning, it is mandatory for them to allow for an authentic sensitive relationship between scientific reasoning patterns and the specific formal tools. Secondly, when analyzing C&P's general structure, it is obvious that the parts of the strategy that allow for portraying the scientific reasoning patterns are both the partitioning and the permeability relations. In addition, it has been recently argued that philosophy of science tends to employ empirical data in order to support particular philosophical theses, instead of trying to use such data as an orientation to build more accurate descriptions and explanations for the actual scientific phenomena (Schickore 2011, Vickers 2013). Finally, it seems that in order to model scientific reasoning sensibly, logicians and philosophers of science should allow the (history of) science to determine which are the relevant patterns to be portrayed in each particular case. Now, if what has been said here is just along the right lines, if C&P aims at describing satisfactorily inconsistency handling mechanisms (that could be the ones used by scientists under particular circumstances), it should possess a sufficiently flexible structure such that it allows for the specific (empirical or historical) data to sensibly determine particular permeability relations for each case.

Thus, I believe that what Heyninck fears to be a dangerous level of freedom, is indeed a philosophical advantage of the formal strategy, such that prevents the permeability relations from being arbitrarily determined and the strategy from being absolutely insensitive to actual (history of) science. Then, it is not clear how this degree of freedom could actually threaten the effectiveness of the strategy, especially if C&P is willing to model something more than toy examples.

2.3. Against Permeability

Heyninck *et al* summarize their third objection as follows:

> "There is no general guarantee for the consistency of a C&P-structure. Only the combination of the theory together with a cleverly chosen permeability relation allow us to arrive at a consistent theory. To prove the consistency of such a carefully chosen C&P-structure, a proof has to be tailored especially for this structure. So whenever there appear slight changes to the theory, the proof has to be revised or even started anew" (forthcoming: 11)

This objection is closely related to Heyninck's second criticism about the excessive degree of freedom of C&P, so, I will not focus on explaining once again why I believe that part does not harm in any relevant sense the paraconsistente strategy. Instead, I will explain why I believe the the objection regarding the need of new proofs once slight changes take place, seems to be well motivated.

As I suggested in Sect. 1.3 and in Sect. 2.2, the efficiency of C&P relies deeply on both, the partitioning and the permeability relations; however, the way C&P is characterized in general terms, makes the permeability relations stiff in a very problematic way: once that a particular group of permeability relations are assigned to a C&P-structure they cannot be modified (basically, because of non-defeasible nature of the strategy's underlying mechanism); so, if the assignation is mistaken (or nonproductive), a new C&P-structure has to be provided[5]. Thus, I agree with Heyninck about the relevance of this problem, however I do not consider this to be an exclusive difficulty of the permeability relations. As a matter of fact, I do believe that the same could happen when the partitioning is not satisfactorily.

Now, while I understand that the problems that could emerge for C&P according Heyninck's third criticism need to be developed in more detail in order to be seen as conclusive, I hope to have motivated the intuition that when analyzing the efficiency of C&P one needs to pay special attention to both, the partitioning and the permeability relations. Following such intuition, along Sect. 3 I will try to provide a case of a complex inconsistency, and in Sect. 4, I will explain how it illustrates strong difficulties when offering a satisfactorily partition.

3. The anomaly in the measuring of solar neutrinos' flux

During the last two decades the emergence and strengthen of paraconsistent logics have been complemented by the study of particular scientific episodes that seem to illustrate the presence and the tolerance of some significant contradictions (cf. Vickers 2013, Meheus 2000, Laudan 1977). However, if inconsistency is actually tolerated in science, this might suggest that our alleged inconsistent theories have failed for scientific purposes. There is a recurrent claim made in the traditional literature of logic and philosophy of science which says that "an inconsistent theory implies any conceivable observational prediction as well as it is negation and thus tells us nothing about the world" (Hempel 2000, 79). In general terms, when presupposing classical logic –especially the principle of explosion –, a contradiction is seen as a synonym of triviality, ergo, as a synonym of scientific failure. In what follows I will present a case study from neutrino phyisics and I will argue that it illustrates satisfactorily the presence and the tolerance of a contradiction.

3.1. The case study

Once neutrinos were successfully introduced to account for the reactions that later would be known as 'β-decay", in the 1960 the scientific community felt confident enough to begin a project for the detection and measuring of the flux of solar neutrinos[6]; 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. However, because in 1962, scientists did not have the required theoretical instruments to make calculations about the solar neutrino flux, the need to offer a detailed model about the behavior of the Sun emerged. And in 1963, John Bahcall offered the first model that helped to predict the flux of solar neutrinos: that theoretical tool was named 'Standard Solar Model'[7] (henceforth, SSM) (Bahcall 2003: 78).

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[5] Along this paper I will not focus on possible alternatives to improve the efficiency of the paraconsistent strategy (as the one offered in Heyninck *et al.*(forthcoming]) however I do believe that to incorporate defeasible mechanisms could only benefit the way C&P models scientific reasoning when dealing with inconsistency.

[6] Solar Neutrinos are subatomic particles that are generated from the solar fusion; it was believed, that this type of particles did have 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 alternatives in order to detect neutrinos in a more precise way.

[7] The SSM is a theoretical framework derived from the application of laws about energy conservation and transport; this model can be used regarding 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

By the end of the 1960's, Bahcall offered a final version of the SSM that was expected to enable making testable predictions about the flux of solar neutrinos, as well as to guide the corresponding experiment (designed by Ray Davis). The experiment required very complex knowledge from radiochemistry, neutrino physics, nuclear physics, among other areas of knowledge, as well as complex apparatus and instruments, as a super cooled underground tank and a Geiger counter[8]. During 1967, Davis (in South Dakota) ran the experiment described below; however once the results came out, Bahcall's predictions were 2.5 times larger than the results reported by Davis (Bahcall, 2003; 79). Davis

> [G]ave the number of counts he had detected; the number of background counts; the number of neutrino-induced events, Σψσ, (SNUs); and the boron-eight neutrino flux, ψ B8.(…) he compared Bahcall's latest predicted value of this fux, (ψ B8 = 1.4 (1 ± 0.6) x 107 neutrinos cm-2 sec-1) with his own observed value, ψ B8 < 0.5 x 107 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; 121-122)

At this point, both, 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 directed. In 1968, the two scientists dedicated themselves to check both of the contributions; nonetheless, despite the modifications that were made on both sides, 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 the SSM. As a matter of fact, the problem remained open until 2001, when it was discovered that neutrinos are of different types and that they have mass; whit this, it was clear that to ignore those facts was what originated the anomaly regarding the measuring of their flux.

3.2. Interpreting the case study

Since the main goal of introducing this case study was to offer a peculiar example of inconsistency between theory and observation, it seems right to argue in favor of the idea that

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empirical, that -depending on the interpretation of the SSM that is used- could efficiently describe a unique empirical domain, in this case the Sun. It has also the capability of giving descriptions of specific phenomena, predictions and guidance for experiments on the phenomena it describes, one its applications is to describe and allow to make predictions regarding the flux of solar neutrinos.

[8] The experiment could be 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 sophisticated measuring instrument. In this case the apparatus is rather bizarre: it consists of a 100,000-gallon tank of perchloroethylene (C2 Cl4 - better known as dry-cleaning fluid), located a mile under the Earth in disused mineshaft in Lead, South Dakota (…) The C2 Cl4 contains an isotope of chlorine, Cl37, with which neutrinos can interact. [1] As a result of the interaction (Cl37 + v - Ar37 + e-), a radioactive isotope of argon, Ar37, is formed. The presence of Ar37 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 Ar37 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: 122)

However, to identify twhat was going to be considered as the result of the experiment, the one that was expected to be contrasted with the SSM prediction, was a very difficult task. 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 needed to incorporate to the experiment anti-coincidence devices (highly sophisticated electronic devices) and strategies for the measurement and evaluation of information.

first, this actually could be seen as an inconsistency, and second, that toleration actually took place in the case of the anomaly in the measuring of solar neutrinos' flux.

First of all, I hope that it is straightforward to see that although several auxiliary hypotheses were offered during a period of more or less 30 years[9], the SSM could never explain satisfactorily the anomaly in the measuring of the solar neutrinos' flux until 2001. And more important, considering that the difference between the prediction and the observational reports was significantly larger than the relevant margin of error at that time, one can interpret the prediction of the SSM for the solar neutrinos' flux as α , and the outcome of the experiment as $\neg\alpha$.

Secondly, after the results of the first experiment came out –this is, after the inconsistency was first noted–, neutrinos remained being successfully used to explain (and predict) the phenomenon of β-decay, thus scientists were still justified in believing the possible existence of these 'theoretical entities'. Furthermore, when scientists, using the SSM, ran the first experiment for the detection and measuring of the solar neutrino flux and the results were not satisfactory, scientist were justified to doubt about rather the predictions offered by theory or the outcomes of the experiment, or both.

In addition, 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 has to be modified–, it was impossible for them to satisfactorily isolate the problematic part of the theory. Different scientists had different ideas about which part of the theory or of the experimental elements was needed to be rejected, and each of them did make different isolations; however, it seems that for several years, none of those cuts was really successful (in the sense that none of them was able to prevent the problem to reemerge). However, even though the problem lasted for long time, and scientists were not sure about what part of the theory was the responsible one for the emergence of the anomaly, they were not emptyhanded. Indeed, they kept the theory in use and they continued experimenting with the solar neutrinos phenomena, as the reports from the Kamiokande, the SAGE, the GALLEX, and SuperKamiokande could show (Bahcall and Davis Jr. 1976, Bahcall 1981, 2000, Pinch 1986, Franklin 2003). So, I think this is enough to say that scientists kept using the SSM as a trustable theory despite the discovery of the inconsistency, could only be understood as if they were tolerating the inconsistency.

That said, I take this section to have shown that the reconstruction of the case study provided here shows satisfactorily that an inconsistency took place and that this did not mean that the theory had to be immediately rejected, but that it was possible to tolerate the inconsistency without destroying the theory in question.

4. C&P and the anti-case study

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[9] Many auxiliary hypothesis 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 Ar37 and Cl37 (which were known with too little precision at the time). That made 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 conceptually 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. Finally, in the 1990's another hypotheses came out, namely that neutrinos are of different types and moreover, that they have mass. These were indeed the modifications that in the long run helped to solve the anomaly in 2001.

A clear way to avoid the formal complications related to inconsistency is not to assume specific commitments from classical logic, such as the principle of explosion. However, if taking this option and willing to defend cases of inconsistent science, one has to choose a particular way to reconstruct the reasoning that helped to avoid explosion (despite the presence of a contradiction) in each particular case and argue in favor of how the studied cases are *mainly* recuperated by the chosen strategy. In what follows, I will try to analyze the possibility of reconstructing the anomaly in the measuring of solar neutrinos' flux with C&P (in the way it was presented in Sect. 1).

4.1. C&P + Observation

First of all, as it has already been said along Sect. 1, C&P aims at having important uses in empirical science (Brown and Priest 2004:386). Now, because 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), then we want our theories to be consistent with observational (or experimental) results as well. So, when analyzing inconsistent empirical theories, it seems necessary to pay special attention to conflicts between theory and observation.

That is why, along this paper I have focused mainly on issues related to theory-observation conflicts.

Furthermore, along Sect. 1.3, I said that the three basic elements that are required in order to put C&P satisfactorily into use are: (1) a recipe of how to separate the inconsistent information into chunks, (2) a recipe to determine how to transmit information from one chunk to another and (3) the satisfaction of the *General Independence-Condition* ($\mathrm{GIC}_{\mathrm{C\&P}}$). In addition, in Sect. 2, I argued that the anomaly in the measuring of the solar neutrinos' flux is a case that illustrates not only an inconsistent theory, but also the toleration of such inconsistency for an extended period. Thus, if one aims at explaining, with C&P, the procedures involved in the toleration of the inconsistency from this case study, one has to provide a way to break up the information into chunks, as well as a way to determine the required permeability relations; more important, one has also to offer an interpretation of $\mathrm{GIC}_{\mathrm{C\&P}}$ such as it allows to recuperate the observational elements involved in the contradiction.

An intuitive way to make a division between the statements involved in the anomaly in the measuring of the solar neutrinos' flux (considering the nature of the inconsistency) is to separate a set of sentences that were representative of the SSM, from the sets of sentences that were representative of the theoretical commitments used in the design of the experiment and the ones representative of the experimental results. If doing so, we will have at least three possible chunks: the one that contains (theoretical and empirical) sentences from SSM, Σ_{SSM}, the one that contains (theoretical and empirical) sentences involved in the experiment, Σ_{Exp}, and the one that contains the observational reports Σ_{R}. For the purposes of reconstructing the scientific episode, the third one will be the target chunk because, as the reconstruction illustrates, part of the information contained by the SSM was combined with some experimental elements in order to generate the observational reports, so what is wanted to be modeled here is how that information was transmitted without reaching explosion.

Furthermore, given this division, a particular interpretation of $\mathrm{GIC}_{\mathrm{C\&P}}$ that could be a good candidate for recuperating observational elements (as well as the Independent boundaries requirement) could be the following one:

> **Observational Independence Condition:** The set of propositions that underlie the design of instruments and methods used to evaluate the observational consequences of a particular theory, ideally, are achieved in a totally different way (or mostly) of the propositions that are used to define 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 seems clear to me that this condition is indispensable to discard cases in which the inconsistency comes from rather the interior of the theory, or the relation between the theory in question and an (auxiliary) theory.

I hope that, to this point, it is straightforward to see that this division seems as a neat and intuitive way to satisfy the prerequisites of C&P for empirical theories.

4.2. The anti-case study

That said, along the following paragraphs I will try to explain why even though the proposed division could look natural to us, it is in fact not a good candidate for analyzing the anomaly in the measuring of the solar neutrinos' flux.

First of all, the anomaly regarding the measuring of the solar neutrino's flux illustrates how even though scientists could point out cases of clear success and clear failure of the theory in question, this did not mean that they were able to locate the origin of the inconsistency.

Secondly, I believe that if one looks at the hypotheses that were proposed to solve the anomaly, one would find that, depending on the historical moment and the particular scientific community, very different hypotheses were offered[10]; this could be easily seen as a reinforcement for the point that I just made: they had no idea about where the inconsistency was coming from.

Thirdly, as it has been said above, since it was very difficult for the scientific community to point out where the problem was –they did not agree for long time which part of which theory has to be modified–, it was impossible for them to satisfactorily isolate the problematic part of the theory. Different scientists had different ideas about which part of the theory or of the experimental elements was needed to be isolated, and each of them did make different isolations; however, it seems that for several years, none of those cuts was really successful (in the sense that none of them was able to prevent the problem to reemerge. However, any of this made scientist to stop trusting the theory as a whole and store it, nor made them to 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 laid on, whether it was related to, say, the instruments, the SSM, the flux models and how they interacted with the equipment, or to our understanding of particle physics, among others.

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[10] Sometimes, the design of the experiment was questioned, particularly some assumptions coming from Radiochemistry that underlie the design of the experiment. Other times, the formal apparatus of the SSM itself was the one that was thought to not be entirely reliable. Sometimes, it was suggested that the mistake laid in the assumption about the exclusive relationship between a particular SSM and a particular level of solar luminosity. In other moments, the ontological status of solar neutrinos (that is, that they were more than just theoretical entities, that they were measurable, and finally, that they were of only one kind) was questioned. Sometimes (almost since the beginning of the debate), the hypothesis of the neutrino oscillation was proposed; 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. And in recent works it has been shown (by Takaaki Kajita and Arthur B. McDonald) that the characterization of neutrinos as massless was mistaken (in order to be able to change identities, neutrinos must have mass).

Now, if C&P aims at modeling inconsistent information by separating it into fragments, where the division prevents contradictions to be formed and the problem to reemerge, and if what has been said here about the case study is just along the right lines; then, even if the proposed division ($\Sigma_{SSM}, \Sigma_{Exp}, \Sigma_{\mathrm{R}}$) holds, it will be of no use for avoiding contradictions to be formed and explosion to take place. This, basically, because of the scientists' lack of understanding of the inconsistency regarding the measuring of the solar neutrinos' flux.

However, one might worry that maybe only the proposed division is what is problematic for modeling this case study with C&P. Let me offer a quick response. It seems to me that C&P aims at modeling a considerable amount of cases from inconsistent empirical science, yet it requires way too much in order to be able to do it. For C&P to model actual cases of inconsistent science an incredibly profound understanding of the *scientific theory* that is wanted to be modeled is required; perhaps this type of understanding is the case for few instances of inconsistent science, but my suggestion is that that is far from true for scientific practice in general. To me, to assume that scientists and logicians are able to know deep enough scientific theories to the point where they can break them into clear consistent chunks and to stablish clear types of consistency-preserving-relations between them, seems as a non-realistic requisite. Ergo, the proposed division might not be the real problem for C&P when dealing with inconsistencies from empirical sciences, but the general idea of how information has to break up, is what ends up being clearly problematic.

An important thing to point out is that I do not think that it is irretrievable that C&P cannot give account for some inconsistencies from empirical sciences. On the contrary, I believe the other applications of the strategy (Brown and Priest 2015, Benham *et al* 2014) are indeed very successful; however, I wanted to present a scenario where the current limitations of C&P could be easily distinguished, in order to make it clear that many more can be explored regarding basic requisites of C&P and inconsistencies between theory and observation.

5. Some considerations on what is needed

Along the previous section, it has been argued that when applying the standard version of C&P to complex inconsistencies between theory and observation few problems emerge: the strategy fails at recuperating the complexity of the scenario and more important, the strategy has been shown to be not sophisticated enough for providing a realistic reconstruction of how scientists determined and changed continuously the partitioning and the permeability relations, i.e., how both, the partitioning and the permeability relations were actually highly flexible.

What I understand to be the morale of the case study presented in Sec. 3.1 is that if a paraconsistent reasoning strategy such as C&P aims at modeling scientific reasoning when dealing with inconsistencies between theory and observation the most important requirement that it should meet is the capability of the strategy for *allowing for realistic reconstruction of natural reasoning in inconsistent contexts*[11]. What this requirement means is that the paraconsistent strategy should recuperate the use of the most natural information-transmitting inferences into, at least, conditional operations (Meheus 2002). If a model aims at describing scientific reasoning under certain circumstances, it should not require to give up all what is natural to scientific reasoning in general, for instance, defeasibility. Now, I strongly believe that a way to satisfy such requirement involves three elements: generality, freedom, and flexibility relative to the

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[11] This requirement was before presented in Meheus 2002 for paraconsistent logics that aim at sensibly and naturally modeling scientific reasoning in inconsistent contexts.

information-transmitting processes and structures (for C&P, relative to permeability relations and to partitioning, respectively). Let me press further this point.

The first element that I recommend for any paraconsistent strategy that aims to model scientific reasoning in inconsistent contexts is generality (as it was described in Sec. 2.1). If a paraconsistent strategy is sufficiently general about the domain of application, i.e., if there are no narrow constrains on how and when to apply it, many of the resulting models could end up being philosophically interesting , sensitively developed, and more important, formally and philosophically revealing.

The second element, freedom (as it was presented in Sec. 2.2), is a very important component because it assures that the applications of the strategy are strongly determined by the peculiarities of the modeled cases and not by the formal constrains of the strategy itself. Evidently, this is very much linked to the ideal of realistically modeling scientific reasoning for particular cases.

Finally, the flexibility relative to the information-transmitting processes and structures is, I believe, the most important element for realistically modeling scientific reasoning. In order to defend so, let me first introduce a case where the failure of the strategy lies on the lack of such flexibility.

Take C&P in its standard version (Brown and Priest 2004), one of the problems that the strategy faces is that if a partitioning ends up being unsuccessful, the model constructed with such partitioning ends up being useless, and so, a new model with a different partitioning has to be developed[12]. If after few failed attempts, one ends up with an adequate partitioning, the model, although formally satisfactory, does not portrait realistically the dynamics of how scientists dealt with the inconsistency (and the heuristics that guided them to the finally successful model). That said, it may be clear that a paraconsistent strategy that aims at realistically model scientific reasoning instances should portrait not only how explosion could be avoided, but also some of the dynamics that allowed scientists to construct the final model.

If what has been said here is just along the right lines then, when willing to realistically model scientific reasoning in inconsistent contexts, a paraconsistent strategy should meet at least three conditions: generality, freedom and flexibility relative to the information-transmitting processes and structures.

6. Concluding remarks

Here I have sustained that C&P faces a serious difficulty when recuperating complex cases from inconsistent empirical science.

Along Sect. 1, I introduced C&P as a non-adjunctive paraconsistent strategy that breaks up inconsistent information into consistent chunks (and carefully restricts the transmission of information from one chunk to another) in order to avoid explosion; here I also argued that some independence-criteria have to be taken into account when willing to put C&P satisfactorily into use. In Sect. 2 I presented three criticisms against the paraconsistent strategy and I argued that two of them represent no risk to C&P, and I also motivated the intuition of the third one being worth of pursuit in general terms. In Sect. 3, I presented the anomaly in the measuring of the solar neutrino's flux, and I argued that it illustrates an inconsistency between theory and observation, making it a possible candidate to be modeled with C&P. Along Sect.4, appealing to this particular case study, I problematized the C&P independence requirements and argue that

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[12] This prohibition to make modifications to the same model must be understood as a lack of flexibility.

C&P, as it was presented in (Brown and Priest 2004), cannot be used for modeling some complex cases of inconsistent science. Finally, in Sect. 5, I argued that generality, freedom and flexibility are three important elements that any paraconsistent reasoning strategy should meet if wanting to provide realistic reconstructions of natural reasoning in inconsistent contexts.

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