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APPLICATIONS OF PARACONSISTENT TOOLS IN THE STUDY OF TOLERANCE TOWARDS CONTRADICTIONS BETWEEN THEORY AND OBSERVATION

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Abstract

This doctoral dissertation aims at applying two paraconsistent formal tools to examine historical cases of inconsistency toleration –in particular, the toleration of contradictions between theory and observation.

In order to do so, the first part of the dissertation addresses both the possibility of finding historical cases of inconsistency toleration in the empirical sciences as well as the value of such cases for the philosophical enterprise. In Chap. 1, I provide an analysis of the type of ignorance that motivates scientists to rationally tolerate some contradictions in empirical sciences. Here I argue that when scientists find a contradiction in their theories, if they recognize to be ignorant regarding either the truth values of the conflicting propositions or segments of their theory's *theoretical structure*, they can be rationally inclined to tolerate such a contradiction.

In Chap. 2 I offer evidence in favor of the plausibility of inconsistency toleration in the empirical sciences. This evidence consists of three case studies that illustrate some of the different types of contradictions between theory and observation in empirical sciences. From the analysis of these case studies I propose a typology of contradictions between theory and observation: (i) those contradictions that show a high degree of observational independence, (ii) those that require the use of an additional theoretical framework to be recognized as contradictions and (iii) those that show a low degree of observational independence between the tested theory and a relevant auxiliary theory.

In Chap. 3 I argue in favor of the philosophical value of these cases - even if they were historically inaccurate and philosophically biased. In particular, I contend that historical reconstructions, even if philosophically biased, can play another equally important role: to enhance our understanding of philosophical theses about science by clarifying some of their concepts or applications.

Having already argued in favor of the evidential value of the case studies presented in Chap. 2, the second part of the dissertation offers formal reconstructions of the reasoning that might underlie the toleration of contradictions in these case studies.

Chapters 4 and 5 seek to address the way in which certain formal tools can help philosophers understand inconsistent nontrivial reasoning. In chapters 4 y 5, I use two paraconsistent formal tools, namely, *Partial Structures* and *Chunk and Permeate*, to provide formal analyses of two cases of inconsistency toleration. I argue that these reconstructions are not only more precise than those provided by the historicist approaches, but also that they shed light on the underlying mechanisms of inconsistency toleration in the empirical sciences.

Resumen

Esta tesis doctoral tiene como objetivo usar dos herramientas paraconsistentes para examinar casos históricos que ilustran tanto la presencia de contradicciones entre teoría y observación en las ciencias empíricas como la tolerancia de los científicos a dichas contradicciones.

Para lograr lo anterior, la primera parte de la tesis aborda tanto la posibilidad de encontrar casos históricos que ilustren la tolerancia a las contradicciones entre teoría y observación en las ciencias empíricas, así como el valor de tales casos para el trabajo filosófico. En el Capítulo 1 presento un análisis del tipo de ignorancia que motiva a los científicos para tolerar racionalmente algunas contradicciones en las ciencias empíricas. En particular, sostengo que cuando los científicos encuentran una contradicción en sus teorías, si reconocen que son ignorantes con respecto a ya sea los valores de verdad de las proposiciones en conflicto o segmentos de la emph estructura teórica de su teoría, pueden estar racionalmente motivados para tolerar la contradicción.

En el Capítulo 2 ofrezco evidencia a favor de la plausibilidad de la tolerancia a las contradicciones en las ciencias empíricas. Dicha evidencia consiste en tres estudios de caso que ilustran las distintas “presentaciones” en las que es posible encontrar contradicciones entre teoría y observación en ciencias empíricas. A partir del análisis de estos estudios de caso, propongo una tipología de contradicciones entre teoría y observación: (i) contradicciones que muestran un grado significativo de independencia observacional, (ii) contradicciones que, para su identificación, requieren la implementación de un marco teórico adicional y (iii) contradicciones que muestran un grado ínfimo de independencia observacional entre la teoría evaluada y alguna otra teoría auxiliar.

En el Capítulo 3 defiendo el valor filosófico de las reconstrucciones históricas de estos casos –incluso si éstas fueran históricamente imprecisas y filosóficamente sesgadas. Aquí sostengo que, incluso las reconstrucciones imprecisas, ficticias o falsas pueden tener un resultado benéfico para la labor filosófica, a saber, promover la comprensión de las tesis filosóficas que dichas reconstrucciones ilustran.

Habiendo ya argumentado a favor del valor evidencial de los estudios de caso presentados en el Capítulo 2, en la segunda parte de esta tesis doctoral me con-

centro en ofrecer reconstrucciones formales del razonamiento que subyace a la tolerancia a las contradicciones entre teoría y observación en dos estudios de caso: el comportamiento anómalo de los núcleos atómicos de los elementos con Números Mágicos y como la medición anómala del flujo de los neutrinos solares. Los capítulos 4 y 5 buscan abordar la forma en que ciertas herramientas formales pueden ayudar a los filósofos a comprender el razonamiento científico inconsistente no trivial.

En los capítulos 4 y 5, con ayuda de las herramientas paraconsistentes *Estructuras Parciales* y *Separar y Permear*, proporciono análisis formales de dos casos de tolerancia a la inconsistencia en las ciencias empíricas. Sostengo que estas reconstrucciones no sólo son más precisas que las proporcionadas por los defensores de enfoques puramente historicistas, sino que también proporcionan evidencia a favor de la plausibilidad de la tolerancia a las contradicciones en las ciencias empíricas.

To:

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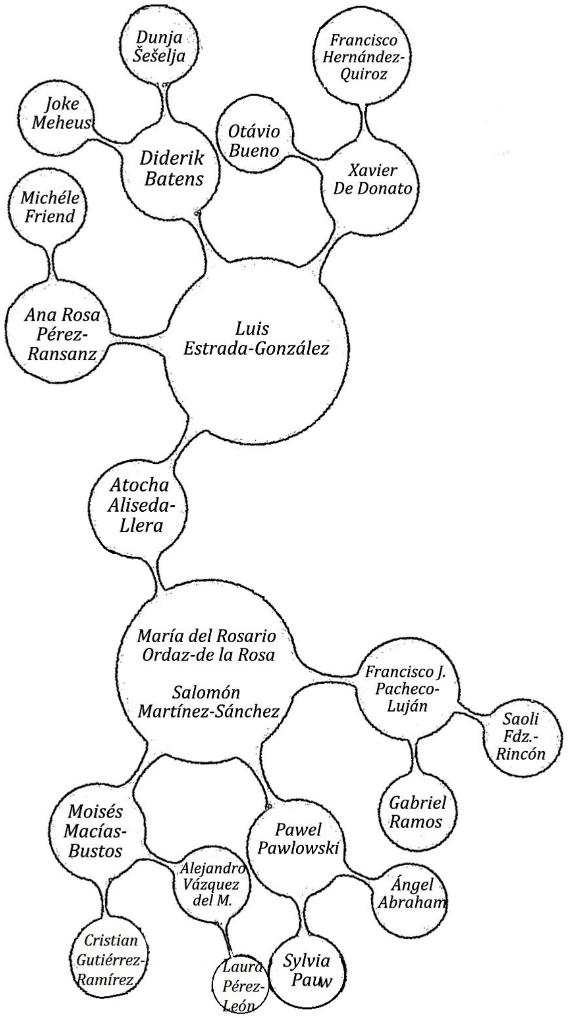

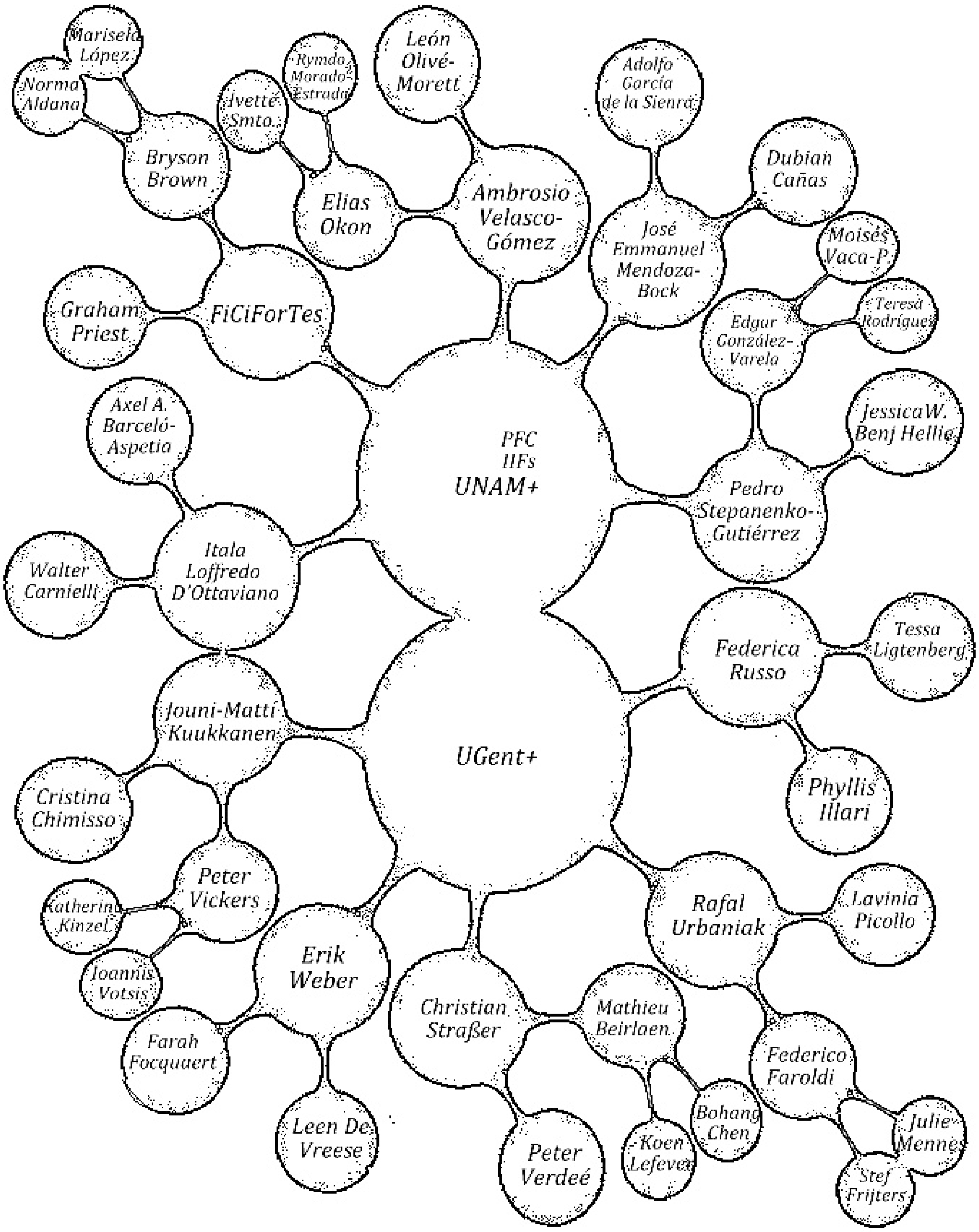
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Introduction

A *contradiction* is a pair of propositions where one is a negation of the other. A set of propositions is *trivial* if it is possible to derive any proposition from it. According to the *Principle of Explosion*, any set of propositions, if closed under classical logic, is trivial if containing a contradiction.

In light of the above, contradictions have been traditionally regarded as extremely malignant, especially for the development of the sciences and scientific reasoning itself. As a matter of fact, there is a recurring view in the traditional literature of logic and philosophy of science which holds that if, while examining our empirical theories, we presuppose the basic principles of classical logic (or any other explosive logic), then because of the Principle of Explosion, "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. Call this, the *Traditional view*.

However, despite these traditional intuitions about the role of contradictions in the sciences, some historians and philosophers have argued that if one looks closely enough to the historical record, one could recognize that many of our best scientific theories have been, at some point in their development, inconsistent and, despite this, they had not become trivial at the same time. Some of the most famous examples of this are: Aristotle's theory of motion (Cf. Priest & Routley, 1983), Bohr's theory of the atom (Cf. Fowler, 1913; Lakatos, 1970; Brown & Priest, 2015), and Classical Electrodynamics (Cf. Frisch, 2004), among others.

In addition, since the second half of the 20th Century, a significant group of logicians have worked on a variety of formal logics that significantly challenge the Principle of Explosion by allowing the presence of contradictions and still pursuing the avoidance of logical triviality. These logics have been labeled as *paraconsistent logics*. Some of the formal resources that resulted from the development of paraconsistent logics have been used to describe and explain historical episodes that illustrate both the presence of contradictions in the sciences and the absence of logical triviality.

The phenomenon of working with inconsistent information and avoiding triviality at the same time is what has been called *inconsistency toleration*. In the case of scientific reasoning, this phenomenon consists of identifying a contradiction in a theory, model or pieces of reasoning and still being able to reason sensibly with the inconsistent information, this is, they are still able to distinguish between the products of their reasoning that are sensible given a particular context from those that are not (Cf. Meheus, 2002; Carnielli & Coniglio, 2016).

Pace the Traditional view, a more recent standpoint, the *Paraconsistent view*, claims that inconsistency toleration is not only possible (as paraconsistent logics have shown) but also a common and safe practice in the sciences. The main assertion of those defending this view, is that inconsistent theories do not always have to be rejected (Cf. Lakatos, 1970; Laudan, 1977; Smith, 1988; Meheus, 2002; Priest, 2002).

Nowadays, there is still no common agreement between philosophers about the plausibility of inconsistency toleration in the sciences. Defenders of the two main standpoints, the Traditional view and Paraconsistent view, have systematically scrutinized both the historical record and the formal constraints of paraconsistent logics without reaching much agreement on the actuality of inconsistency toleration. In particular, for the case of non-trivial inconsistent scientific reasoning, supporters of the Traditional view have argued that the historical record has not provided any strong evidence in favor of the need of inconsistency toleration in the sciences (see Vickers, 2013: Chap. 3-7; Davey, 2014). With this concerns in mind, the methodology that the supporters of the Traditional view have provided has been mostly historiographical and, allegedly, not motivated by any preferences regarding logic.

In contrast, the supporters of the Paraconsistent view have argued in favor to more formal approaches for the philosophical scrutiny of science. As a matter of fact, the majority of the research programs of the Paraconsistent view have focused mostly on study of the inferences that scientists could have followed as well as on the different logics that could explain such inferences.

This has made the debate extremely complex as it involves the scrutiny of the historical record, the analysis of the possible doxastic commitments that scientists have had towards contradictions, as well as the study of inferential procedures that could be used when working with sets of inconsistent information.

The above strongly suggests that the study of inconsistency toleration in the sciences is an enterprise that belongs to logicians, epistemologists, philosophers and historians of science.

The aim of this dissertation is to use two (paraconsistent) formal tools to achieve philosophical understanding of the ways in which scientists tolerate (and have tolerated) contradictions between theory and observation in the empirical sciences. For this reason, the work presented here consisted of a combination of espistemological, historical and formal approaches to the study of contradictions in the empirical sciences.

In this dissertation I focus on contradictions between theory and observation. The reason to do so is that inconsistencies between theory and observation are remarkably frequent in scientific practice and, most of the time, they are also considered to be unproblematic. In light of the above, I believe that a correct philosophical analysis of the kind of reasoning involved in the toleration of such contradictions will, in the long run, shed light on optimal ways to understand the general phenomenon of inconsistency toleration in scientific reasoning. Here I discuss if contradictions between theory and observation could be tolerated and if so, how.

This dissertation aims at contributing to two different fields of scholarship: first, to the methodology of philosophy of science, by introducing a novel way to classify contradictions between theory and observation and an account to evaluate historical evidence used to support of specific philosophical theses. Second, to the formal analysis of scientific reasoning, by providing formal reconstructions of the reasoning that underlie the toleration of contradictions between theory and observation in specific historical episodes.

The first part of the dissertation tackles both the possibility of finding historical cases that illustrate tolerance of contradictions between theory and observation in the empirical sciences, and the value of such cases for the philosophical endeavor.

For this matter, in Chap. 1, I explore under which circumstances scientists can rationally tolerate contradictions in the empirical sciences. In order to do so, the chapter is divided in two main parts. In the first one, I focus on describing inconsistency toleration. When doing so, I introduce some of the most traditional arguments against and in favor of tolerating contradictions in the sciences; here I also provide a characterization of the most frequent ways in which contradictions arise in the empirical sciences. The second part of the chapter is devoted to argue that ignorance can play an extremely important role when explaining why scientists have, sometimes, tolerated contradictions in the empirical sciences. In this part, I address the possible connection between inconsistency toleration and ignorance, and explore the different kinds of ignorance that could be identified when scrutinizing cases of (alleged) inconsistent science. This chapter includes two case studies that illustrate different types of ignorance associated to the toleration of contradictions in the empirical sciences; such cases are the contradiction between Continental Drift theory and a Permanentist theory, and the anomaly in the measuring of the solar neutrinos' flux.

Chap. 2 is devoted provide historical evidence that illustrates the toleration of contradictions between theory and observation in the empirical sciences. Here I tackle Davey's (2014) claim that inconsistency is never tolerated in science, but only discretely isolated. I argue that some contradictions are tolerated only *because* they cannot be isolated and I explain how the anomaly in the measuring of the solar neutrinos' flux neatly illustrates this. In addition, I provide a typology of contradictions between theory and observation and argue that to differentiate between different types of contradictions between theory and observation can allow for a more fine-grained study of inconsistencies in empirical sciences. I use three different case studies from physics to exemplify the distinctions described by the typology. These cases are the anomaly in the precession of the perihelion of Mercury, the anomalous *magic numbers* and the anomaly in the measuring of the solar neutrinos flux.

Considering the historical evidence presented in the previous two chapters, Chap. 3 consists on discussing the problems associated with the philosophical use of historical evidence. In this chapter, I address accusations about the misuse of the historical record in order to support Paraconsistent view's philosophical theses. Here, I argue that, even if such worries were adequate and the historical reconstructions that the defenders of the Paraconsistent view have provided are biased, philosophers could still benefit enormously by the study of such reconstructions. In this chapter, I argue that historical reconstructions, even if philosophically biased, can play another equally important role: to enhance our understanding of philosophical theses about science by clarifying some of their concepts or applications. For this reason, the study of historical episodes that allegedly illustrate inconsistency toleration in the sciences would be revealing even if, in the long run, one could discover that the reconstructions of such episodes were philosophically biased. Therefore, the case studies provided in this dissertation are in themselves extremely valuable for achieving philosophical understanding of the reasoning that underlies inconsistency toleration as well as our own philosophical theses about the phenomenon in question.

This considered, the second part of the dissertation provides reconstructions of the reasoning that underlies the toleration of contradictions between theory and observation in order to tackle the way in which certain formal tools can help philosophers to understand non-trivial inconsistent scientific reasoning.

This part of the research is mainly motivated as a response to Vickers' claims on the alleged way in which the supporters of the Paraconsistent view have mistreated the historical record:

> if one looks more carefully at the relevant history o science, most of these cases are not really inconsistent in any significant sense after all. And to reconstruct such cases *as* inconsistent sets of propositions is a highly dubious move. *Often when this is done the motivation seems to be to find an application for a paraconsistent logic, and not to say something interesting or important about how science works, or even could work* (Vickers, 2013: 252, my emphasis)

As a response to this, in Chap. 4 and Chap. 5, I provide formal analyses of two cases of inconsistency toleration in the empirical sciences, and I do this without committing to any particular paraconsistent logic. I argue that these reconstructions are not only more precise than the ones provided by the supporters of the Traditional view, but that they also provide evidence in favor of the actuality of inconsistency toleration in the empirical sciences.

In Chap. 4, I explore the way in which scientists safely combine partial and inconsistent information when building their empirical theories. In order to do so, I first introduce some methodological worries about the different ways in which philosophers can account for inconsistency toleration in the sciences, and I propose that a particular kind of (paraconsistent) formal tools, informed by the history of science, can help philosophers and logicians of science to elucidate the ways in which scientists work with inconsistent information and at the same time, succeed at avoiding logical explosion. Furthermore, I use the *Partial Structures approach* to scientific theories (first introduced in Chap. 2) to scrutinize the reasoning that underlie the inconsistency toleration in the anomaly in the measuring of the solar neutrinos' flux.

Chap. 5[1] is devoted to discuss the scope and the limitations of a paraconsistent tool named *Chunk and Permeate* (first introduced by Brown & Priest (2004)) when used to model and explain actual cases of non-trivial inconsistent scientific reasoning. This chapter helps to shed light on the reasoning that scientists carry out when tolerating contradictions in the empirical sciences. In order to do so, Friend and I first describe *Chunk and Permeate* as a formal tool designed to model and explain the reasoning that underlies inconsistency toleration. Later on, we argue in favor of some needed adaptations for *Chunk and Permeate* in order to improve its performance when used to model complex cases of scientific reasoning. We extend *Chunk and Permeate* by adding a visually transparent way of guiding

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[1] This chapter consists of a joint work with Michéle Friend. It was originally published as [Friend & Martínez-Ordaz, 2018].

the individuation of chunks and deciding on what information permeates from one chunk to the next. This extension is named *Bundle Chunk and Permeate* and, in this chapter we apply it to one of the case studies introduced previously in Chap. 2, namely, the anomalous magic numbers.

In Chap. 6, I introduce the final remarks of this research.

1 Inconsistency toleration and the ignorance behind it

1.1 Introduction

The *Principle of Explosion* says that any theory if closed under an explosive logical consequence relation, if containing a contradiction, is trivial. A *contradiction* is a pair of propositions, where one is a negation of the other. A theory is *trivial* if any proposition is a theorem. Therefore, any inconsistent such theory is trivial.

In addition to the above, it is common wisdom that contradictions are (classically) logically false and that anyone knowingly relying on a falsity is an irrational agent. From this it seems to follow that if a scientist consciously trusts an inconsistent theory (or set of propositions) and reasons with such a set of inconsistent information, she, *ceteris paribus*, must be irrational.

In recent decades, philosophers of science have noticed that, at some point in their development, most scientific theories were thought to be inconsistent and scientists kept working with them nonetheless. Some of the most famous examples of this are: Aristotle's theory of motion, the early calculus, Bohr's theory of the atom and Classical Electrodynamics, among others. This has given the impression that contradictions are not as malign as they are often portrayed to be. The tolerant attitude towards contradictions is often called *inconsistency toleration*, and it consists of the practice of knowingly reasoning with inconsistent information without threatening one's rationality.

Following this line of thought, some philosophers have addressed the possibility of rationally using inconsistent information in the sciences and have provided explanations that interestingly refer to a type of scientific ignorance:

1. When having two scientific statements that contradict each other, scientists tend to assume that, at least, one of them is false (Laudan, 1977: 56).

2. If scientists are able to distinguish which of the conflicting propositions should be regarded as false (due to being an idealization, a fiction, among

others), then they would be able to explain how they could satisfactorily work on seemingly false information.

3. However, most of the time, when confronted with an inconsistent set of propositions, scientists ignore, at least, which of the mutually contradictory statements should be regarded as false (Cf. Bueno, 1997, 2006; Brown, 1999; Priest, 2002).

4. Once this ignorance is acknowledged, if scientists have no better alternative to the inconsistent set of propositions, the toleration of the contradiction becomes the only option at hand.[1] This tolerant attitude towards contradictions is often seen by scientists as a temporary resource According to these explanations, ignorance plays an important role in motivating a *temporary* tolerant attitude towards a specific contradiction. This is, if contradictions tend to be associated to ignoring something such as the truth value of the conflicting propositions, then this tolerant attitude would be conditionalized to such a state of ignorance and would be extremely likely to change once the puzzle is solved.

This type of explanations presupposes a particular understanding of ignorance, namely, ignorance as a lack of knowledge regarding the truth values of the conflicting propositions (henceforth, *factual ignorance*). Here I test this type of explanations when scrutinizing two cases of inconsistent science. The outcome of such scrutiny suggests that there is a limit case of factual ignorance that could be explanatory of why scientists tolerate certain contradictions in the empirical sciences.

This chapter aims at contributing to the epistemology of science. Here, I contend that ignorance is an essential feature when explaining under which circumstances, if any, scientists can be rationally inclined to tolerate a contradiction in the (empirical) sciences. Here I content the following thesis:

> **Thesis *Ign→IT*:** The acknowledgement of their own factual ignorance with respect to a contradiction can be explanatory of when scientists are

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[1] In this kind of situations, scientists face the dilemma of either getting rid of both statements or accepting both *pro tem* –while acquiring the needed information for resolving the conflict. If they take the first option, they are left empty-handed; while, if they take the second option, they are obliged to find a way to preserve sensible reasoning despite the presence of a contradiction. All things considered, a large group of philosophers and logicians of science have suggested that the *most rational* decision would be to tolerate the contradiction while, at least, filling up the blanks (Cf. Batens, 1998, 2002; Meheus, 2002; Bueno, 2017).

rationally inclined to tolerate such a contradiction

I discuss two kinds of ignorance that (sometimes, jointly) are present when scientists adopt a tolerant attitude towards contradiction in the sciences, namely *factual ignorance* and *ignorance of theoretical structure.*

In order to do so, the plan for the chapter goes as follows. In Sec. 1.2, I explain the philosophical relevance of inconsistency toleration in the sciences. In Sec. 1.3, I deepen into the philosophical explanations that relate inconsistency toleration to factual ignorance. Later on, in Sec. 1.4, I provide historical evidence in favor of ***thesis Ign→IT***. In particular, I present two case studies of alleged inconsistency toleration in the sciences. The first, from the early geology, illustrates how factual ignorance could be explanatory of the practice of inconsistency toleration. The second case, from neutrino physics, that requires a more refined explanation. In Sec. 1.5, appealing to the holistic properties of bodies of scientific knowledge, I characterize a limit case of factual ignorance, namely, *ignorance of theoretical structure*, and I explain the second case by referring to this type of ignorance. Finally, in Sec. 1.6, I draw some conclusions regarding the role that ignorance could play when explaining under which circumstances it is rational to tolerate contradictions in the empirical sciences.

1.2 Inconsistency toleration

This section is devoted to providing a general understanding of the phenomenon of inconsistency toleration in the sciences. In order to do so, first, I introduce the *Traditional view*, a philosophical standpoint according to which inconsistency toleration puts in danger scientific rationality. Later on, I present the *Paraconsistent view*, the standpoint, according to which to tolerate a contradiction does not necessarily entail the irrationality of the scientists. Finally, I characterize in more detail the phenomenon of inconsistency toleration.

1.2.1 The *Traditional view*: Contradictions always entail irrationality

Epistemic agents *reason sensibly* if they are able to, at least, distinguish between the products of their reasoning that are sensible given a particular context from those that are not (Cf. Šešelja, 2017; Friend & Martínez-Ordaz, 2018). When logical triviality is around, sensible reasoning is irremediably threatened.

In the literature of logic and philosophy of science, there is a view that submits that the rational acceptance of contradictions is an epistemically undesirable task and that contradictions in the sciences are closely linked to the irrationality of the scientists. Call this standpoint the *Traditional view*. For this view, contradictions are often taken to be extremely malignant because they are (classically) logical falsities, trivializing formulae or a sign of the violation of one of the most important constraints of human rationality. Some of the most famous arguments that have been provided in favor of this are the following.

- ***Contradictions are falsities that must be rejected***: Taking into account the definitions presented just above, if a scientific theory is inconsistent, then, the theory is, ether strictly 'false'(Davey, 2014: 3009) or dangerously 'uninformative'.[2] Given so, inconsistent theories *should* always be rejected.

- ***Contradictions are a sign of irrationality***: If rational belief were to be closed under entailment, because of the Principle of Explosion, "if someone believed a contradiction, they ought to believe everything, which is too much" (Priest, 1998: 410), and in this case, sensible reasoning, which seems to be constitutive of rationality, would be simply impossible.

- ***Contradictions are trivializers***: Any inconsistent scientific theory would be logically trivial. Because of its trivial character, such a theory would not say anything trustworthy about the world (Hempel, 2000: 79). That considered, the theory would irremediably lack scientific character and thus, it should be immediately rejected (Davey, 2014).

Therefore, to knowingly tolerate a contradiction should be taken to be, at its best, as an act of irrationality, and at its worst, as an impossible task. For a long time, as inconsistency toleration was regarded as an extremely implausible task; this made the phenomenon to be neglected by the vast majority of philosophers and logicians.

1.2.2 The *Paraconsistent view*:

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[2] Due to the Principle of Explosion, any inconsistent scientific theory would be trivial and would not say anything trustworthy about the world. Therefore, any such inconsistent theory should be irremediably rejected as it fails at fulfilling any of the most important goals of empirical theories.

Contradictions can be tolerated

Pace the Traditional view, a more recent approach to logic and philosophy of science claims that inconsistency toleration in science is not as dangerous as we tend to imagine. This perspective has been enriched by the study of paraconsistent logics[3] 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. Call this view the *Paraconsistent view.*

The main assertion of those defending this standpoint is that inconsistent theories do not always have to be rejected (Lakatos, 1970; Laudan, 1977; Smith, 1988; Meheus, 2002; Priest, 2002; Batens, 2002; Bueno, 2017). In order to support their claims, the defenders of the Paraconsistent view have untiringly argued that inconsistent theories do not always need to be rejected. Let me press further this point by presenting some of the most prominent arguments in favor of this view.

- ***Falsities do not always have to be rejected***: Even set of premises that are strictly false, do not need to be irremediably rejected. As a matter of fact, in the corresponding literature, it has been argued that false statements can, sometimes, have a positive epistemic value, even if they are already known to be false; some examples are abstractions, highly idealized models, and fictions, among others. "Hence if false scientific beliefs can often be beneficial for scientific inquiries, then it follows that false scientific beliefs can be epistemically useful" (Pritchard, 2016: 7).

- ***It is possible to rationally accept a contradiction:*** First, "acceptance only involves a commitment to the reliability of a theory, then accepting an inconsistent theory can be compatible with our standards of rationality, as long as inconsistent consequences of the theory agree approximately and to the appropriate degree of accuracy" (Frisch, 2004: 544).

  In addition, rational belief is very likely to be not-closed under entailment (Cf. Priest, 1998: 411); this is, even if a scientist accepts a contradiction, this might not be sufficient for her to start accepting any possible proposition. Therefore, it seems to follow that if one wants to keep the triviality

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[3] In general terms, a logical consequence relation is said to be 'paraconsistent' if it is not explosive, this is, if it does not validate the Principle of Explosion.

objection, one has to provide additional evidence in favor of the possibility of rational belief to be closed under an explosive logical consequence relation.[4]

- ***No inconsistent empirical theory has ever been trivial***: The historical record has shown that none of the (alleged) inconsistent theories has ever been trivial. Some of the most famous examples of this are: Aristotle's theory of motion, the Newtonian Cosmology, the Newtonian Mechanics, the early calculus, Bohr's theory of the atom, Classical Electrodynamics, among others. Thus, it seems fair to think that contradictions in the sciences are not necessarily trivializers.[5]

All this considered, it is not difficult to see that the Traditional view would struggle if wanting to defeat all these arguments without (dramatically) weakening their own. Therefore, the Traditional view, as it was presented here, might be irremediably doomed.

1.2.3 Inconsistency toleration

The significant differences between historical examples of (alleged) inconsistency toleration has lead the supporters of the Paraconsistent view to conclude that there is a large variety not only of cases of inconsistency toleration but also of the ways in which scientists have satisfactorily dealt with contradictions. The study of different exemplars as well as the ways in which scientists have worked

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[4] The supporter of the Traditional view can still argue that, even if triviality is avoided because rationality is not closed under an explosive logical consequence relation, a serious problem still remains: If a scientist is justified for believing a proposition, α, as well as for believing another proposition, $\neg\alpha$, she will have epistemic justification for believing a contradiction, this is, JB_{Sc} $(\alpha \wedge \neg\alpha)$. But if contradictions are falsities and this is well known by any scientist, now the scientist in question is justified for believing a falsity that is known to be so. All this comes in clear conflict with the Traditional view on scientific rationality.

As a response to this objection, it is often replied that this undesirable consequence of tolerating contradictions is not possible as "'justified belief' is not closed under conjunction. But it is not because justification is not closed under conjunction, it is because probability is not closed under conjunction" (Sutton, 2007: 68) and thus, this occurs: $JB_{Sc}(\alpha)$, $JB_{Sc}(\neg\alpha)$ $\nvdash JB_{Sc}(\alpha \wedge \neg\alpha)$. Therefore, even if rational belief would be at danger if having justified belief over a contradiction, even just to reach that point seems extremely far-off.

[5] This historical evidence could (and should) be seen as a great challenge for the Traditional view, making of prominent importance to explain either why we get the constant false impression that contradictions are tolerated or why contradictions are sometimes tolerable without putting at risk the rationality of the scientists.

with inconsistent information, has allowed philosophers to distinguish between at least two stances of inconsistency in the sciences: at the *theory level* and at the level of *epistemic justification (doxastic).*

The former, inconsistency at the theory level, takes place when a specific theory contains a contradiction (Cf. Vickers, 2013:150-56). This stance includes internal inconsistencies, but also inconsistencies between theory and observation and between theories. The most characteristic aspect of this type of inconsistency is that it is independent of the scientists' doxastic commitments towards the mutually contradictory statements, as well as independent from the scientific practices associated to the use of such an inconsistent theory.

The latter, inconsistency at the level of epistemic justification, occurs if the epistemic agents consider a pair of propositions (where one is the negation of the other) to be candidates for the truth, or if they provide an explanation for the success of their scientific practices and such an explanation is inconsistent (Cf. Vickers, 2013: 156-58).

Once an agent recognizes a contradiction in either her theory or her doxastic commitments, if she is also able to identify specific inferential mechanisms that allow her to work with the inconsistent set of information and still preserve sensible reasoning, one call this *inconsistency toleration.*

If inconsistency toleration were a rational practice, scientists would need to provide an explanation that addresses: (i) under which circumstances scientists would be rationally willing to tolerate a contradiction, (ii) how they could preserve sensible reasoning while using inconsistent information and (iii) how they could work relying on seemingly false information. This chapter is devoted to shedding light on the first issue, namely, why scientists could be rationally inclined to tolerate a contradiction in their theories or pieces of reasoning. In the following section, I argue that factual ignorance could be explanatory of why scientists are open to inconsistency toleration in the first place.

1.3 Contradictions and factual ignorance

In this section I aim at addressing the role that ignorance plays in inconsistency toleration. In particular, I explain how whenever scientists find a contradiction in their theories, if they ignore the truth values of the conflicting propositions and acknowledge to do so, they can be rationally inclined to tolerate such a contradiction. In order to do so, I proceed in two steps: first, I discuss some of the traditional explanations of inconsistency toleration and their relation with ignorance; second, I argue that the type of ignorance that is involved in the toleration of contradictions is mostly ignorance of truth values.

1.3.1 Inconsistency toleration and ignorance

Considering the negative consequences that the presence of contradictions could have for scientific rationality, there is common agreement on the fact that contradictions have to be, if ever tolerated, treated with extreme care and only tolera,[6] never forgetting that inconsistency toleration should be seen as a last resource. Here I address the issue of why and when inconsistency toleration could be a good option for the scientists.

As empirical sciences involve, most of the time, the use of incomplete information, it does not come as a surprise that our scientific theories lack, at different points in their development, important epistemic virtues, such as consistency. However, the constant presence of incomplete information in the sciences does not entail that inconsistency toleration constitutes, prima facie, a rational practice. As a matter of fact, when facing contradictions it is always necessary to ask ourselves if it is rational to accept a specific inconsistent theory. This is,

> One theory, say Bohr's theory of the atom, may have a high degree of empirical adequacy, be very fruitful, but inconsistent. Another may be consistent, have a lesser degree of empirical adequacy, and be rather ad hoc. In such circumstances, when is one theory to be rationally preferred? When it is clearly better than its rivals. And when is this? When it is sufficiently better on a sufficient number of criteria. This is all very vague. Perhaps ineradicable so. It may be tightened up in a number of ways, but this is unnecessary here. (...)Perhaps less familiar, it shows how and when it may be rational to accept an inconsistent theory: *when, despite its inconsistency, it is markedly better than its rivals on sufficiently many other criteria.* (Priest, 2002: 215, my emphasis)

For the case in which there are no real competitors, it is less rational to leave scientists empty-handed than to request them to find ways to satisfactorily reason with the inconsistent set of information (an argument for similar scenarios

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[6] *Dialetheism* is the view according to which *some* contradictions are true. Given so, Dialetheists might disagree on the temporary character of the toleration of *some* contradictions, specially in the formal sciences (Cf. Priest, 1998); however, even Dialetheists will agree on the fact that the majority of contradictions that emerge in the empirical sciences are solely temporarily tolerated meanwhile seeking for a better alternative for the inconsistent set of propositions (see Priest, 2002).

could be found in [Lakatos, 1978: Cap. 1, Sec. 2.b]). A resulting extremely well-spread consideration is that, when explaining inconsistency toleration as a rational maneuver, it is necessary to appeal to a certain type of ignorance. Let me press further on this point.

When identifying a contradiction, our intuitions suggest that at least one of the contradictory statements should be regarded as false; however, in the majority of cases, when trapped in this type of scenarios, scientists do not have the needed resources (either theoretical, experimental, mathematical, among others) for discovering which of the contradictory statements is false (Cf. Bueno, 2006; Martínez-Ordaz, 2017).

Considering that contradictions are commonly seen as extremely problematic, it would not be rational to accept a contradiction if consistent versions of the theory were available; thus, only when there is no better option to the inconsistent set of propositions, such set is rationally *acceptable* (see Priest, 2002).[7] In addition, as the majority of scientific contexts are of the type in which inconsistencies occur but are considered either exceptional or problematic, when contradictions are tolerated such tolerance is conceived as merely *provisional* (see Batens, 2002), and the length of this tolerant attitude will depend mostly on the time it takes to get rid of the ignorance that justified the tolerance.

In what follows I deepen into the type of ignorance that plays a role in explaining inconsistency toleration as a rational practice.

1.3.2 Factual ignorance and contradictions

There are two rival accounts of what ignorance could be. "On the first view, called the 'Standard View', ignorance is lack or absence of knowledge, whereas on the second view, called the 'New View', ignorance is lack or absence of true belief" (Le Morvan & Peels, 2016: 12) –for the purposes of the chapter, here, I focus on the first type.

Ignorance, understood as the absence of knowledge, could be divided into three subcategories: *absence of factual knowledge* (lacking knowledge of the truth value of specific propositions), *absence of objectual knowledge* (not knowing a particular object) and *absence of procedural knowledge* (not knowing how to do certain tasks).

When philosophers have used ignorance to explain inconsistency toleration in

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[7] For a comprehensive study of the different views on acceptance associated to the toleration of contradictions in the sciences see [Šešelja, 2017: Sec. 3.3].

the sciences, they refer to the fact that scientists ignore the truth values of the conflicting propositions. Let me press further this point:

- First, if committed to one's classical intuitions (such as bivalence), one would accept the following: (i) if p is the case, $\neg p$ cannot be the case and vice versa and (ii) always, either p is the case or $\neg p$ is the case. This gives the impression that, if it were possible to determine the truth value for either p or $\neg p$, one could immediately infer the truth value of the other.[8]
- Later on, when facing contradiction, if scientists were aware of which of the conflicting statements is false, they would have a (sort of) guidance for solving (or at least, beginning to solve) the contradiction in question.
- However, if inconsistency toleration were to occur in the empirical sciences, this would suggest that scientists *ignore* if any of the components of the contradiction must be taken as definitely true (or false).

Before continuing, let's explain the type of ignorance that is at stake for these explanations, namely *factual ignorance.*

Factual ignorance of p consists of ignoring the truth value of p –at least, under certain circumstances. Factual ignorance can be the result of epistemic agent's failure at fulfilling any of the basic conditions for factual knowledge.[9] For the purposes of this chapter, I focus on the factual ignorance that could result from the non-satisfaction of the *alethic condition* —and I focus on the cases that result from the agent's (at least, temporarily) incapability to determine the proposition's truth value.

I consider the alethic condition to be understood in terms of, when satisfied with respect to p, assigning a unique truth value to the proposition p. If this

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[8] In addition, even if not committed to bivalence, to know if a particular proposition is false can allow scientists to look for an explanation of why such a proposition can be used in a specific contexts; for instance, if the proposition in question is taken to be a fiction, a component of a highly idealized model, among others.

[9] Namely:

(i) a *doxastic* condition: S believes that p;

(ii) an *alethic* condition: p is true;

(iii) a *justificatory* condition: S believes that p with justification;

(iv) a *Gettier-proofing* condition: S's justification for believing that p must withstand Gettier-type counterexamples. (Le Morvan & Peels, 2016:18).

condition is not fulfilled, the agent fails at deciding if p is true or not, and the same happens for the case of $\neg p$. If the agent cannot assign values neither to p nor to $\neg p$, the agent would be unable to satisfactorily reject or accept any of the two propositions in dispute. Therefore, if scientists are factual-ly ignorant due to the non-satisfaction of the alethic condition, they might be pushed to either retain both statements meanwhile they decide the truth values of the elements of the contradiction or reject both statements a priori.

One of the worries that the reader might entertain regarding this view on ignorance is the following: if characterizing ignorance as the complement of knowledge, the philosophical understanding of ignorance that philosophers of science could achieve will be strongly (and problematically) tied to a specific conception of knowledge −which, very likely, would be still extremely contentious among philosophers.

With this in mind and only for the cases associated to the use of inconsistent information the empirical sciences, I suggest to see this type of factual ignorance from a (maybe) less philosophically contentious perspective and focus more the connections between ignorance and truth values. I suggest to understand factual ignorance as the *temporary undecidability*[10] of the truth value of a proposition p by an epistemic agent S at a specific time T_1.

An important remark: recognizing oneself being ignorant of, for instance, the truth values of p and $\neg p$, is only the first step for the development of scientists' response to the presence of contradictions in science. This is, despite the fact of being aware of their ignorance, scientists "do not simply respond to ignorance by leaving a mere blank. We have a natural a perfectly reasonable inclination to fill in those gaps in the easiest, most natural, and sometimes, even most attractive way" (Rescher, 2009: 2). As a matter of fact, "the ignorant agent rarely (if ever) approaches the lack of information as a missing content, but she often permeates her cognition with possibilities and hypotheses that build a framework around that black space in order to justify or explain it" (Arfini, 2019: 28), and these ways of providing frameworks that surround the blanks are what give rise to the many different ways in which scientists can tolerate contradictions in the sciences.[11]

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[10] 'Undecidability' understood not necessarily as it is perceived in the literature of logic and philosophy of mathematics –as for the cases of inconsistent science the truth values in question are, in the long run, likely to be determined (if interested in the connections between this conception of undecidability for statements from the empirical sciences see [Gutiérrez-Ramírez, 2015: Chap. 1. In Spanish].

[11] While such different ways to deal with contradictions are not the main objective here, they

While more could be said here, I hope it is clear that the notion of factual ignorance has played an important role when explaining why scientists are, sometimes, rationally inclined to tolerate contradictions in the sciences. In the following section, I present two case studies that illustrate inconsistency toleration in the empirical sciences and I scrutinize them to see if factual ignorance actually takes place in both cases and if it is explanatory of why the contradictions were tolerated at the time.

1.4 Historical Evidence

This section is devoted to provide historical evidence in favor of the thesis *Ign*→*IT*. In order to do so, I present two case studies that illustrate inconsistency toleration in the empirical sciences: the first one is the contradiction between the Permanentist theory and the Continental Drift theory, and the second one is the anomaly in the measuring of the solar neutrinos' flux. I consider these cases to be great exemplars of how, regardless if scientists had doxastic preferences towards any of the elements of the contradiction, ignorance played a notorious role in explaining why scientists were willing to tolerate the contradictions at the moment.

1.4.1 Preliminaries: Coherence

Here I aim at addressing the value of intertheoretic coherence in the sciences; this will benefit enormously the understanding of the first case study as illustrative of inconsistency toleration.

Scientific theories tend naturally to form clusters guided by particular scientific problematic to be solved; these clusters could be *coherent* only if *consistency*, *compatibility* and *reinforcement between theories* are achieved first (see Elsamahi, 2005). In order to fulfill these requirements, it is not needed that all theories in a cluster attend the same issue or belong to the same discipline. "Although it may appear from the first look that theories are arranged in clusters according to the topics they address, a closer look at actual clusters of theories shows that members of a cluster do not merely deal with similar topics" (Elsamahi, 2005: 335).

A cluster of theories is *consistent* if an only if it is impossible to form a contradiction (at least, in the intersection of the theories in question). (Empirical)

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are tackled in Chap. 2, Chap. 4. and Chap. 5.

theories in a cluster are *mutually compatible* if they are mutually consistent and they 'talk' (at least partially) about the same empirical domain; this, of course, strengths the motivation for the union of the theories. Finally, two theories in a cluster *reinforce* each other if either "T provides a "rationale" for (a part of) T1" (Laudan, 1977: 54); or if, at least, one supports the basic assumptions of the other, or explains mechanisms of the second theory, or clarifies the concepts of the first theory and their applications (Cf. Elsamahi, 2005).

In sum, consistency assures the safety of the union by helping to avoid the risk of explosion. Complementary enhances the production of scientific novelties that could not be entailed by each theory alone. And reinforcement guarantees the relevance of the union, if a set of theories talk (at least partially) about the same empirical domain then, the motivation for the union gets strengthen immediately. Coherence between theories allows for the constant opening of certain domains that are usually closed to independent theories.

Therefore, coherence between theories is not only worth of pursuit but a salient feature of scientific rationality (as it promotes the satisfaction of epistemic virtues such as scope, fruitfulness, among others). In the following subsection, I introduce a case study that illustrates the presence of contradictions between theories and explain why the inconsistency was tolerated at the time.

1.4.2 Permanetism vs Continental Drift theory

By the early 20th century, in the Earth sciences (specifically in geology), there were two rival research programs that aimed at explaining some salient features of the Earth. Such programs were the *Permanentist theory* and the emergent *Continental Drift theory*.

On the one hand, the Permanentist theory assumed that, after an original contraction of the materials of the continents and the oceanic floor, the oceans and continents have always remained the same (Pérez-Malváez, Bueno & Morrone, 2003: 3, my translation). One of the most notorious supporters of the permanentist view was Maurice Ewing (1959). Ewing's proposal sustained that, although currents of convection existed, these were restricted to the mantle. Likewise, he explained that the oceanic bases and the continents were fixed and that there was a sort of ascending currents through which the cracks in the valleys and the shallow earthquakes were explained (Frankel, 1988). Ewing's theory provided explanations for:

1. the lack of sediment at the peak of the mountains,

2. the presence of shallow earthquakes,
3. the positive magnetic anomaly, and
4. the geological fracture of the Great Rift Valley.

On the other hand, the Continental Drift theory, Wegener's proposal (1912), was a theory according to which "all the continents had once been united, had broken apart and had drifted through the ocean floor to their current locations" (Šešelja & Weber, 2011: 148). For this theory of Continental Drift, the Earth was formed by layers of different compositions and densities, the most superficial being the one that included the continents, which were once united in a supercontinent called Pangea. This theory provided an account for:

1. the similarities between the contours of the coasts of eastern South America and western Africa, as well as those of the coasts of North America and of Europe;
2. the disjunctive distribution of the species (current and past),
3. the presence of glacial deposits (beds) in South Africa, Argentina, Brazil, India and Australia (central and eastern) during the Permian and Carboniferous periods, and
4. the formation of new mountain ranges (Cf. Frankel, 1988: 271 -72).

Despite the broad explanatory power that nowadays we recognize in the Continental Drift theory, at the moment, things were different. The strongest objection that this theory had to face was the one made by Harold Jeffreys. Jeffreys argued that "the continental crust was strong enough to support the mountain Everest, and the oceanic crust strong enough to keep deep pits." (Pérez-Malváez, Bueno & Morrone, 2003: 11, my translation). Thus, both soils were extremely rigid and an excessively large force would be needed to enable the possibility of what was sustained to have happened with the continents across the oceans. In addition, Jeffreys argued that "The forces postulated by Wegener to move the continents on the ocean floor were most of the time, too weak. In addition, no force known was large enough to cause such a migration of the continents. And even if there was one, the continents would not be able to resist such a trip" (Frankel, 1988: 272-73).

At the moment, it was known that if any of these theories were to be true, they must have been compatible with, at least, the the recently discovered large presence of Radium (*Ra*) in the Earth (and the implications that this had on the predictions of the heat that the *Ra*'s disintegration would continuously cause in the inside of the Earth). In addition, if any of these theories was true, it had to be compatible with the phenomenon of *isostasy* (which "is the idea that the lighter crust must be floating on the denser underlying mantle (...) Isostatic equilibrium is an ideal state where the crust and mantle would settle into in absence of disturbing forces" (Pan, 2007)). Finally, to be true these theories were expected to either be compatible with both Jeffreys' objection and the assumption that the ocean floor is too rigid for the continents to move through (which was a basic assumption of physics and oceanography of the time), or to explain why these accepted assumptions were mistaken.

Not surprisingly, Ewing's theory was compatible with Jeffreys' objection, but Wegener's theory was extremely troubled by Jeffreys' point on the rigidity of the ocean floor. But, while geologists at the time decided which theory was (more likely) to be true, it seemed that they tolerated a contradiction between theories, as they used one to explain certain phenomena, and used the other to explain the phenomena that the first could not account for. The contradiction was dissolved in the following years when Jeffreys' objection was discovered to be false (see Frankel, 1988).

1.4.3 Explaining the case study

Regarding the tension between Wegener's and Ewing's theories, given that both theories met a minimum criterion of empirical adequacy, that each of them ordered and grouped phenomena that the rival did not encompass, and also that they offered both new problems and strategies to resolve highly relevant conflicts in their discipline (fruitfulness); we can say that both were, at the moment, functional empirical theories. In addition, it would not be hard for the reader to spot the nature of the conflict between these theories, they were not only rivals but they were mutually contradictory. Yet, this should not be problematic at all, as the presence of rival theories has been systematically considered as a salient feature of good scientific practice, for them to be mutually inconsistent might be solely accessory.

The problem was that, because of the nature of the object of study (it is the Earth itself and it's past), it was extremely difficult for the scientists to actually confront and test any of the conflicting assumptions. In this sense, they were helpless when having to figure out which of the two sets of propositions, either Ewing's theory or Wegener's theory, was false. As a matter of fact, both theories seemed equally powerful overall, and scientists did not have any serious reason for preferring one over the other, distinct from their mere intuitions and personal preferences (Cf. Pérez-Malváez, Bueno & Morrone, 2003). To add more pressure to this scenario, scientists recognized that both theories explained disjointed sets of relevant empirical phenomena appealing to mutually incompatible basic assumptions. As a response to this extremely complicated setting, for a time, geologist had to rely on the two conflicting theories, one for describing and explaining some of the features of the Earth and on the other for describing and explaining some other features of the Earth.

In this sense, the temporary retention of the mutually inconsistent theories could be understood as an exemplar of inconsistency toleration. However, if this was a legitimate case of inconsistency toleration, an important question that comes to us immediately is: was the decision to temporarily retain the conflicting theories a rational decision? This question is particularly relevant, especially in light of the epistemic significance that (according to Sec. 1.4.1) intertheoretic coherence has. All things considered, it seemed that scientists were in a scenario in which they could either reject both theories (due to the mutual inconsistency), reject one of them (the selection being done without a clear rational basis), or tolerate the contradiction and keep both explanatory approaches only *pro tem*. According to this picture, the two first options could have caused a clear explanatory loss, which seemed even more severe than the one possibly caused by temporary neglecting intertheoretic coherence as regulatory guidance. Therefore, to retain both theories *pro tem* seems to have been the most rational option.

If this intertheoretic contradiction was actually tolerated, and if what was said here is along the right lines, it is natural to expect that ignorance played an important part in the explanation of the rationality behind this practice. If so, what did the scientists actually ignore? It seems that, in this case, two mutually contradictory sets of propositions were in dispute: one containing the propositions of Ewing's theory and another containing the propositions of Wegener's theory. In addition, it was quite transparent for the involved scientists that, if one of the sets was discovered to be false, the other would be extremely likely to be true (see Pérez-Malváez, Bueno & Morrone, 2003). They did not question that the majority of evidence in favor of Ewing's theory was conditionalized by the truth value of Jeffreys' objection: if the objection was true, then Ewing's theory would be extremely likely to be true as well. In contrast, if the objection were to be discovered to be false, then Ewing's theory would be likely to be false as well. In addition, the involved scientists were certain that the truth value of the propositions involved in Wegener's theory depended largely also on the value of Jeffrey's objection, if false, Wegener's theory should be regarded as true. All this reveals that scientists at the time knew that the truth value of the Jeffreys' statement, once determined would help them to decide the values of the statements associated with both theories.

The explanation that I have just provided is extremely compatible with the practice of inconsistency toleration as motivated by the factual ignorance. It is very likely that the ignorance of the truth value of Jeffreys' objection was caused by the complexity of the subject of study, as at the time, it was impossible to design any test that could help geologists to decide the value of Jeffreys' statement. In the long run, it was shown that Jeffreys' objection was mistaken and only then the debate was decided in favor of Wegener's theory. But for the time that scientists ignored if the objection was true or not, the only option that scientists had at their disposal was to tolerate the contradiction.

In the following paragraphs, I introduce another case of inconsistency toleration in order to see if the traditional explanations that link inconsistency toleration to ignorance, can be uniformly applied (for, at least, cases of contradictions in the empirical sciences).

1.4.4 The anomaly in the measuring of the solar neutrinos' flux

For many years, neutrinos were theoretical entities that help to explain and predict nuclear reactions -which later would be known as "β-decay". In 1960, physicists finally believed that it was possible to design an experiment to prove the existence of neutrinos –this, through the measuring of the flux of solar neutrinos.[12] In order to do so, physicists combined information from different disciplines –which included radiochemistry, nuclear physics, astrophysics, and neutrino physics (Cf. Pinch, 1986: 47). With that information put together, physicists were able to come up with what was needed in order to test the existence of solar neutrinos. On the one hand, John Bahcall designed a mathematical model that helped to predict the flux of solar neutrinos, this model was named 'Standard Solar Model' (henceforth, *SSM*) (see Bahcall, 2003: 78).[13] On the other hand, Ray Davis was

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[12] Solar Neutrinos are subatomic particles that are generated from solar fusion; it was believed, that this type of particles did have neither electric charge nor mass.

[13] 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

expected to design an experiment to tests the predictions of the *SSM*. The experiment made use of the combination of knowledge from radiochemistry, neutrino physics, nuclear physics, among other areas of physics, as well as of complex apparatus and instruments -as a super cooled underground tank and a Geiger counter.

When, in 1967, Davis did the experiment, the results showed that the *SSM*'s predictions were 2.5 times larger than the results reported by Davis (see Bahcall, 2003: 79). Davis blamed Bahcall's calculations, and Bahcall attributed the conflict to the experiment. Both research groups spend the year of 1968 reviewing and correcting the *SSM* and the setup for the experiment. After almost a year, they ran the experiment once again, but the results were not yet satisfactorily. The difference between the predictions and the observational results was still large enough to dismiss a margin of the error situation, making the observational outcome impossible to be considered as evidence in favor of the *SSM*. The problem remained unsolved 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.

1.4.5 Explaining the case study

Since it was very difficult for the scientific community to point out where the inconsistency originated –they did not agree for a long time which part of which theory had to be modified–, it was impossible for them to satisfactorily isolate the problematic part(s) of the theory. But this did not mean that they stopped using the SSM nor relying on some of the basic assumptions of the theories involved in the design of the experiment. 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 (Cf. Bahcall & Davis Jr., 1976; Bahcall, 1981, 2000; Pinch, 1986; Franklin,

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of gas and that has a spherical shape, and that also possess the luminosity, the radio, the age and the composition of the Sun. 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 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. (Martínez-Ordaz, 2017: 133.)

2003). So, I consider this to be enough to say that scientists kept using the conflicting assumptions despite the discovery of the inconsistency, which could only be understood as if they were tolerating the inconsistency.[14]

This case, contrary to the previous one, does not seem to be so straight forward to explain. While there was a common agreement among the involved physicists on the fact that ignorance played an important role when explaining why it took so long to resolve the contradiction (Pinch, 1986), it is not clear that the involved scientists actually possessed factual ignorance. First, the major problem was that, at the moment, they ignored many of the inferential relations that the SSM allowed for as well as many of the inferential relations that connected the theories that underlie the experimental design; nonetheless, it was not clear that they had no epistemic access to the truth value of the propositions that they considered of interest.

As a matter of fact, it seemed that the physicists did not understand fully the experimental setup, and that this had the consequence of them being unable to identify the problematic propositions that could be causing the conflict between the SSM's predictions and the observational results. For instance, they knew that when chlorine, Cl^{37}, and argon, Ar^{37}, interacted they were going to allow the production of neutrinos, however, they were not sure of the reason of why this was going to happen –as the development of nuclear physics and nuclear chemistry was still very poor. Even more problematic, the assumption about the cross sections of Cl^{37} and Ar^{37} was shared by the SSM, thus, the lack of understanding of the elements of the experiment was also affecting the understanding of, at least, part of the SSM's relations. Finally, in contrast with the previous case study, here the scientists did not know which of their assumptions were conditionalized by which other assumptions, making even harder to find a way out from this scenario. This lack of epistemic access was different from the one that was described for the previous case study; especially because here, it was almost impossible for the scientists to identify which were actually the conflicting statements –while the contradiction appeared between the predictions of the theory and the observational reports, it was thought that the actual conflict was likely to be located between the involved theories.

With all this in mind, in the next section I describe in more detail this phenomenon and argue that the type of ignorance that took place here was not a simple case of factual ignorance, but something deeper and closely related to the

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[14] For a detailed analysis of this particular case study as an exemplar of inconsistency toleration see [Martínez-Ordaz, 2017].

epistemic access to the structure of the theories that scientists at the moment were working with.

1.5 Ignorance of theoretical structure

In this section, I explain the ignorance that took place in the anomalous measuring of the solar neutrinos' flux. In order to do so, I briefly introduce some conceptual specifications on the structure of scientific theories and holistic properties of bodies of scientific knowledge. Furthermore, I characterize *ignorance of theoretical structure* and explain why is this type of ignorance is what motivates the inconsistency toleration as a rational practice in the neutrinos case study.

1.5.1 Theoretical structure and holism

Scientific theories are often considered to be clusters of information which are initially incomplete but that, in the long run, tend to incorporate new data in order to improve the picture of the world that they provide. But, 'the world' itself is an infinitely large source of information, and one "of the most obvious sources of ignorance is the sheer volume of available factual information. There is so much out there to be known that any given individual cannot ever begin to make more than an insignificant fraction of it" (Rescher, 2009: 4). In light of this, scientific theories are epistemic vehicles that help scientists to filter, order and relate the varied information that they get about the world in order to provide accurate descriptions, predictions, and explanations of the domain that they are talking about.[15]

While scientific theories aim at uniting varied types of data, they do it in a very special way, they follow and preserve particular inferential patterns, patterns that are thought to be linked to predictive and explanatory success. Scientific theories are arranged in such a way in which they illustrate the different scientific disciplines' methodological commitments, and what scientists

> [R]eally study are not any objects and their properties, but certain general inference relations or *inference patterns* (...) What exactly does speaking of 'inference relations' here involve; in particular, what are the *relata*: mere

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[15] I am fully aware of the fact that there is an ongoing philosophical debate about the status of the different characterizations of scientific theories (see [Halvorson, 2016] for a comprehensive revision of the different views on scientific theories), however, I think this will suffice for the purposes of the chapter.

> sentences (so that we are back to some kind of formalism?), propositions (leading us beyond formalism after all?), etc.? (Reck & Price, 2000: 347-48. My emphasis).

This is, one of the main tasks of scientific theories, according to this view, is to preserve and stress-specific inference relations between propositions –and it is expected that such inference relations are what warrants the success of the theory in different contexts. For the purposes of this chapter, here I understand that the inference relations between propositions constitute one of the most important elements of the structure of scientific theories.[16]

In general, take a structure to be a set of operations and relations between collections of objects such that, in a certain way, has the epistemic role of facilitating the study of the objects contained by the collections. For the case of the empirical sciences, structures cannot be "construed as 'pure' logico-mathematical structure (see French, 2007); it was always intended to be understood as theoretically informed structure" (French, 2012: 7), this is, structures that were constantly informed by the (empirical) commitments associated to the domain of application of the theories in question.

In light of the above, for the case of empirical sciences, one could see empirical theories as formulated based on the following theoretical model: $\boldsymbol{T}$=<$\boldsymbol{D}$, $\boldsymbol{R}_i^n$> "where $\boldsymbol{D}$ is a particular domain (a set of objects to which the theory is supposed to apply) and $\boldsymbol{R}_i$ is a family of n-place relations holding between the elements of $\boldsymbol{D}$" (Bueno, 1997: 588). While the domain could be selected and individuated depending on the methodological preference of the research program in which the theory is being used and vary from time to time; the set of relations, $\boldsymbol{R}_i$, work in a very different way: first, they are what helps to order, classify, and evaluate the objects in the domain (and the propositions through which they are described). Second, they close under specific logical consequence relations the objects of $\boldsymbol{D}$, allowing and forbidding certain interactions between them. And third, as they regulate the behavior of $\boldsymbol{D}$, they will not necessarily change if $\boldsymbol{D}$ increases or decreases.

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[16] I believe that the identification of reliable inferential patterns constitutes an extremely important scientific task. To identify inferential (logical) strategies for the preservation of the success within our best theories is closely related to the preservation of (epistemic) reliability. That said, I consider important to state that, this does not imply that scientific theories *are* structures, but that structures constitute a useful "'mode of representation' and it is important to reiterate that, on my view, adopting it does not entail that either theories or the structures they put forward as 'out there' in the world should be regarded as inherently set-theoretic in any way" (French, 2012: 5).

It should not be hard to see how inference relations determine largely the type of results that scientists can get in their day to day use of theories. For instance, there are historical cases in which the chosen set of inference relations forbade the unrestricted use of conjunction as a reliable inference (Cf. Brown & Priest 2004, 2015), or cases where *reductio ad absurdum*-type of proofs were regarded as not reliable (Cf. Meheus, 2002). Henceforth, I refer to the inferential structure of empirical scientific theories as *theoretical structure*.

To pay attention to the theoretical structure of our scientific theories can allow us to identify entailment-type of relations between the elements of such theories; this, in a sense, is a feature that is not always obtained when assigning a truth value to a particular proposition. To understand (part of) the inferential structure of a theory can help scientists to achieve a sort of epistemic warrant on certain inferences and to block other inferences in specific contexts.

Considering both the extremely large amount of information that scientists acquire and process daily, and the complexity of scientific theories, when dealing with with contradictions in the empirical sciences it is extremely complicated, or even contextually impossible, to recognize the part of the theory that should be blamed for the contradiction.

This could be partially explained by the Duhem-Quine thesis: Our theories are molded by different types of statements and very diverse and complex inferential relations between those statements. That considered, sometimes, when we identify an assumption that contradicts another one or a prediction that is incompatible with another report, the problem does not lie only in the propositions known to be in explicit conflict; it could be bound to many more and different segments of the theory that sometimes are not easy to identify and that are also justified by appeal to statements that we fully trust (see Duhem, 1991: 185). And this is not a problem that arises exclusively when evaluating a theory alone. As,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 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 the holistic properties of empirical theories, it will have to be said that sometimes the holistic properties of the theory do 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 (see Martínez-Ordaz, 2017: 128-29).

Summing up, when dealing with contradictions in the sciences, holism might represent one of the most important challenges for the isolation and the resolution of the incompatibility. However, I consider holism to play an interesting role at the level of the acquisition of knowledge regarding the theoretical structure of our theories. Let me say more about this in the next section.

1.5.2 Knowledge and ignorance of theoretical structure

Can we fully know the theoretical structure of a theory? It seems that due to our cognitive limitations and due to the complexity of the large majority of our best empirical theories, this might be impossible. However, this does not mean that we cannot know *some* important parts of the theoretical structure of our scientific theories. As a matter of fact, to know certain parts of the theoretical structure of her theories gives the scientist the capability of knowing which inferences are correct in a particular context and also why such inferences have certain specific consequences.

When scientists master specific inferential patterns within a particular domain, what they gain is a way to structure and follow successfully certain inferences in their day-to-day practice; this is, not only that they can use inferential rules in an effective way but also that they can explain under which circumstances and why certain inferential rules are reliable in a domain of application of their theory.

A question that should be addressed at this point is *what type of knowledge is the knowledge of theoretical structures in the empirical sciences.* In what follows I argue that knowledge of theoretical structure is a limit case of factual knowledge, which is interestingly also close to procedural knowledge.

Knowledge of theoretical structure is not a simple case of factual knowledge, as to say that someone knows certain structure does not mean only that that person knows the truth value of specific propositions. Indeed, to know which inferences are allowed within a theory can help scientists to determine the truth value of particular propositions of the theory, but it can also provide, for example, important hints on the logical principles that rule that segment of the theory. So, while knowledge of a fragment of a theoretical structure allows agents to achieve knowledge of the truth values of specific propositions that are located in such a fragment, knowledge of theoretical structure seems to be something deeper.

A clear way to emphasize the differences between a simple case of factual knowledge and factual knowledge associated to a theoretical structure would be to describe the way in which these two kinds of knowledge can fail. On the one hand, the impossibility of determining the truth values of certain propositions in the empirical sciences, could be simply associated, for instance, to a lack of experimental resources; for example, in the case of Jeffreys' objection (in the contradiction between the Permanentist theory and the Continental Drift theory) what scientists were missing was a way to test the actual rigidity of the ocean floor, but this was not really caused by the structure of any of the theories involved, but only by a lack of experimental development. On the other hand, sometimes, the incapability of determining the truth value of certain proposition within a theory is closely caused by a lack of epistemic access to the structure of the theory itself, this could occur if scientists have not figured out how certain parts of the theory hang together and which are the adequate inferences that could be ran in that segment of the theory.

The fact that knowledge of theoretical structure is extremely close to the agents' ability to making certain inferences within a part of a theory, makes this kind of knowledge to be particularly close to procedural knowledge. Procedural knowledge about inferential maneuvers might be a result of knowledge of theoretical structure, but is not fully it. When scientists come to know the inferential patterns that are proper of their theories, they discover an extremely abstract entity and they develop the capability of 'reading' the structure on many different objects and scenarios; but they never lose sight of the real aim: to determine if certain propositions are true within the theory.[17]

If what has been said in this section is along the right lines, I propose now to characterize *ignorance of theoretical structure.*

***Ignorance of theoretical structure*:** absence of knowledge regarding

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[17] In addition, it seems that knowledge of theoretical structure is not of the type of objectual knowledge either. While there are many still open debates regarding the nature of inferential (mathematical and logical) structure, it is more or less clear that structures are formal entities of a different kind of our regular objects of objectual knowledge.

> the (relevant) inferential connections that scientific theories allow for. When ignoring (the relevant parts of) the theoretical structure of a theory, scientists are not capable of grasping abstract causal connections between the propositions of their theory, they can neither identify the logical consequences of the propositions that they are working with nor can explain under which conditions the truth value of such propositions will be false.
>
> A possible cause of this kind of ignorance might be the holistic character of bodies of scientific knowledge. If the degree of internal or external holism, is extremely high, scientists would not able to satisfactorily identify privileged inferential relations that allow them to test the truth value of the involved premises, among other things.

An important question still remains: *can we explain the rationality behind the inconsistency toleration in the case of the anomalous measuring of solar neutrinos' flux by appealing to ignorance of theoretical structure.*

The case study presented in Sec. 1.4.3, illustrates that 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. (. . .)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. (...) The main problem 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 (see Martínez-Ordaz, 2017: 137-39).

All this considered, it seems that the scientists at the time ignored how all the different theoretical assumptions that shaped both the Standard Solar Model and the design of the experiment interacted to one another in an inferential manner. Given so, when the anomaly was discovered, they had two options: on the one hand, they could reject all the theoretical assumptions involved in the design of the experiment as well as the ones related to the development of the SSM. On the other hand, they could entertain the conflicting theories while trying to grasp a better understanding of the structure of the, at least, common elements between them.

As the involved theories were independently used in other disciplines, it seemed irrational to lose confidence in them only because neutrino physicists had not yet achieved full understanding of how such theories related to one another, As a matter of fact, once scientists realized that they ignored the theoretical structure of the SSM and of the theories behind the experiments, they were inclined to tolerate the contradiction *pro tem* –until the community surpassed their ignorance of the relevant inference relations between the conflicting pats of the involved theories. Once knowledge of certain parts of the structured of the theories was achieved, physicists realized that the understanding of neutrinos required to take as false the hypotheses about neutrinos as massless and neutrinos being of just one type, this is, to understand how these theories related mutually and internally, allowed scientists to adequately determine the truth values of certain propositions.

1.6 Final remarks

In this chapter I have submitted the following:

> **Thesis *Ign→IT*:** When scientists find a contradiction in their theories, if they recognize to be ignorant regarding either the truth values of the conflicting propositions or segments of their theory's *theoretical structure*, they can be rationally inclined to tolerate such a contradiction.

In order to do so, in Sec. 1.2, I have characterized *inconsistency toleration* as the ability to preserve sensible reasoning despite doing it with inconsistent information, and I have argued that the philosophical explanations of the rationality behind inconsistency toleration make constant reference to ignorance –specifically factual ignorance.

In Sec. 1.3, I explored different ways to understand and explain *factual ignorance* and I concluded that, according to a large majority of philosophers, the ignorance that plays a prominent role in the majority of cases of inconsistency toleration is indeed *factual ignorance* –due to the non-satisfaction of the alethic condition of knowledge. I characterized such ignorance as the (temporary) undecidability of the truth value of a proposition p by an epistemic agent S.

In Sec. 1.4, I presented two case studies that illustrated inconsistency toleration in the empirical sciences. I argued that ignorance (and the acknowledge of it) played an important role when justifying the scientists' tolerant attitude as rational. In addition, I argued that, when the first case study was an exemplar of *(temporary) undecidability of truth values*, the second case illustrated a deeper kind of ignorance.

Then, in Sec. 1.5, I explored which type of ignorance could be explanatory of the second case and argued that in that case what was being ignored was the (inferential) structure of the theory; I called this type of ignorance *ignorance of theoretical structure* as described it as a limit case of factual ignorance.

In the next chapter, I return to the anomaly in the measuring of the solar neutrinos' flux to see if the type of ignorance that was present in that historical episode had any repercussions in the way scientists dealt with the contradiction. As a matter of fact, the next chapter consists in an attempt to argue in favor of tree different types of contradictions between theory and observation in the empirical sciences. There, I discuss not only the anomalous measuring of the solar neutrino's flux, but also the anomaly in the perihelion of Mercury and the constant use of two mutually contradictory models of the atomic nucleus.

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2 The many contradictions between theory and observation

2.1 Introduction

This chapter consists in providing both historical evidence in favor of the presence of inconsistency toleration in the empirical sciences, as well as a typology of contradictions between theory an observation.

An extremely important question in the philosophy of science is whether science could be inconsistent and non-explosive at the same time; this is, *can science be inconsistency tolerant*. On the one hand, those who answer 'no' typically point to certain well-entrenched classical intuitions about the relation between contradictions and logical triviality. According to such a view, if a scientific theory is inconsistent, then, the theory is, at its best, strictly 'false' (Davey, 2014), and, at its worst, dangerously 'uninformative'(Hempel, 2000: 79; Popper, 2002: 72).[1] Therefore, scientists should take seriously only logically consistent theories, and reject every inconsistent theory that comes to their sight. This standpoint, the *Traditional view*, submits the thesis that inconsistency toleration is either an impossible task or an irrational maneuver.

On the other hand, those who answer 'yes' typically appeal to the fact that, according to some non-classical logics, contradictions do not always entail triviality.[2] For such a view, while science should never be trivial, it can be, and has been, *inconsistency tolerant* (Batens, 2002; Meheus, 2002; Priest, 2002; Urbaniak, 2012; Friend & Martínez-Ordaz, 2018). Call this the *Paraconsistent view*

In Chap. 1, I provided some strong arguments, pace the Traditional view, in favor of inconsistency toleration. In addition, I presented two case studies that illustrated the toleration of contradictions in the empirical sciences and I argued

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[1] See Sec. 1.2.1 of Chap. 1.

[2] See Sec. 1.2.2 of Chap. 1.

that in both cases, ignorance was explanatory of *why* scientists were rationally inclined to tolerate such contradictions. In addition, I argued that the ignorance that took place in these two cases was of different kinds. While the first case, the contradiction between the Permanentist theory and Continental Drift theory, illustrated a simple case of *factual ignorance*,[3] the second case, the anomaly in the measuring of the solar neutrinos' flux, showed a deeper and more problematic kind of factual ignorance, namely, *ignorance of theoretical structure*.[4] Concerning this particular case, even if the scientists would have known the truth value of one of the components of the contradiction, it would have not been of any significant use, as they ignored how the elements of the contradiction related to other elements in the same theory. In this chapter, I address again this second case study and I explore the possibility that this kind of ignorance had affected the way in which scientists tolerated the contradiction, if they ever did so.

In this chapter, I focus on analyzing (exclusively) contradictions between theory and observation, often called "anomalies". The reason why I chose anomalies to be the main object of my study here is that they are not only remarkably frequent in the scientific practice, but they are also considered to be, most of the time, harmless. This is why I believe that a correct philosophical analysis of inconsistency toleration in the empirical sciences that focuses in this type of contradictions will, in the long run, shed light on optimal ways to understand the general phenomenon of inconsistency toleration in scientific reasoning.

The present chapter is devoted to, first, argue in favor of the plausibility of inconsistency toleration in the empirical sciences and, later on, to provide a typology of contradictions between theory and observation –which, I consider, could allow for a novel fine-grained study of inconsistencies in empirical sciences.

In what follows, I address the question *are the empirical sciences "inconsistency tolerant"*. As a response to it, I submit that inconsistency toleration is not only possible but that we have sufficient historical evidence to conclude that it has actually taken place in science. Call this the ***thesis*** $\ll$***IT***$\gg$. In addition, I contend that, in the empirical realm, there are, at least, three different types of contradictions between theory and observation, call this the *Many Contradictions between Theory and Observation-thesis* (henceforth, ***thesis*** $MC_{(T\text{-}O)}$).

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[3] This case study, I argue, shows that the pertinent scientific community ignored the truth values of the elements involved in the contradiction -if they had known the value of any of those elements, it seems very likely that they would have not tolerated the contradiction.

[4] This type of ignorance, I argued, consists in the lack of knowledge regarding the (relevant) inferential connections that specific scientific theories allow for when solving particular scientific problems.

The plan for the chapter goes as follows. In Sec. 2.2, I briefly introduce some preliminary concepts, such as *empirical theory*, *inconsistent empirical theory* and *contradiction between theory and observation*. Later on, in Sec. 2.3, I present a specially strong argument, from the Traditional view, against the possibility of inconsistency toleration in the sciences. In Sec. 2.4, I provide a philosophical response to such an argument and in Sec. 2.5, I support my response with historical evidence. In Sec. 2.6, I argue that some of the inconsistencies between theory and observation look quite different from each other, and I provide a guide for the recognition of contradictions between theory and observation. In Sec. 2.7, I argue that contradictions between theory and observation could be of, at least, three distinct types and draw some conclusions on the possibility of inconsistency toleration in empirical sciences and a way to characterize contradictions that involve (experimental) observation. Sec. 2.8 is devoted to present some conclusions.

2.2 Preliminaries

This section is devoted to characterizing some of the most important elements in the philosophical debates on inconsistent science, so as to make it easier to address the phenomenon of inconsistency toleration. Such elements include: criteria for theory identification in the empirical sciences, *frameworks*, *empirical theory*, *inconsistent empirical theory* and, *inconsistencies between theory and observation*.

2.2.1 Frameworks

In the empirical sciences, the different disciplines and research domains are never completely independent of each other (see Laudan, 1977: 53; Elsamahi, 2005: 335). As a matter of fact, considering that certain scientific problems are complex enough that they cannot be clearly solved by one theory or model alone, more often than we expect, scientists combine different theories (from different disciplines) and different models (from different theories) for solving specific problems. As scientists tend to group different theories and models in order to extend their scope enough to explain a target phenomenon, the clusters of information that they allow to form by these combinations are expected to be what I call *scientific frameworks* (henceforth, for simplicity, *frameworks*). A framework consists in a combination of (originally) different sets of information that relate to each other in such a way that the subset that contains all the fundamental assumptions of each of the theories or models that are combined is, at the very least, non-trivial, *logically consistent*, *mutually compatible* and (weakly) *complementary.*[5] Finally, the elements of a particular framework are subject to "new" inferential rules –not necessarily previously shared by all the "originally" different sets of information.

2.2.2 Empirical theory

The presence of contradictions in the sciences has been generally regarded as a result of scientists constantly working with incomplete information in their disciplines (Cf. Bueno, 1997, 2017; Meheus, 2002; Šešelja, 2017). Despite the fact that the information at their disposal is often defective (partial, conflicting, or inconsistent), scientists are still able to arrange this information in such a away that they can provide theoretical approaches to studied domains. These arrangements often have the shape of scientific theories. In addition, inconsistency toleration does not consist on tolerating an isolated contradiction; as a matter of fact, the toleration often happens within a scientific theory in which both parts of the contradiction play different roles for describing, narrowing predictions, measuring, or explaining specific phenomena. For these two reasons it is important to pay special attention on how to identify and locate different types of contradictions within empirical theories; to do so, of course, it is necessary to provide a characterization of empirical theories.

In accordance with the above, here, I assume that scientific theories can be seen as clusters of information which are initially incomplete but that, in the long run, can incorporate new information in order to be completed –even if this final stage is never achieved.

In particular, in what follows, I commit to the so-called *Partial Structures approach* to scientific theories ($PPEE$). This particular view on scientific theories has proved to be extremely handy when explaining and modeling inconsistency in the empirical sciences (See Bueno, 1997; Bueno, French & Ladyman, 2002; da Costa & French, 2003; Bueno & French, 2011).[6]

The basic idea behind *PPEE* is that science is essentially an open endeavor and because of that, systematically, scientists work with partial, conflicting and inconsistent information. *PPEE* tries to account for the (at least, temporarily) defective theories that result from scientists working with defective (in particular,

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[5] As characterized in Sec. 1.4.1 of the previous chapter.

[6] As *PPEE* has shown to be extremely successful when addressing the way in which scientists deal with the presence of inconsistent information in the sciences, Chap. 4 is devoted to discuss this approach in more detail.

partial) information in their day to day practice, submitting that such theories are partially true with respect to a specific domain.

Following the *PPEE* intuitions, in what follows I briefly characterize empirical theories.

Let *scientific theories* to be formulated based on the following set−theoretic construct: $T =< \boldsymbol{D}, \boldsymbol{R}_i^n >$ "where $\boldsymbol{D}$ is a particular domain (a set of objects to which the theory is supposed to apply) and $\boldsymbol{R}_i$ 3 is a family of n-place relations holding between the elements of $\boldsymbol{D}$" (Bueno, 1997: 588). T consists of a set substructures (*partial structures*), $< A, A', ...A^n >$, of the form $A =< D, R_k >_{k \in K}$. Each R is a *partial relation*,[7] and an ordered triple $< R_1, R_2, R_3 >$ where R_1, R_2 and R_3 are mutually disjoint sets such that:

- R_1 is the set of n-tuples that (we take to) belong to R,
- R_2 is the set of n-tuples that (we take) do not belong to R,
- R_3 is the set of n-tuples for which it is not defined whether belong or not to R. (Bueno & French, 2011: 858-59).

I chose this particular approach to scientific theories because (as it was discussed in the previous chapter), the rational tolerance of contradictions in the sciences is often accompanied by the scientists' factual ignorance about, at least, the conflicting propositions. The *PPEE* leaves room for the ways in which the partiality of information and ignorance interact in the scientific endeavor without putting in danger scientific rationality; as a matter of fact, the R_3 component allows us to address this issue by hosting the elements that have not yet been determined to be models of the theory.

Now, for a theory, T, to be an empirical theory, it should contain certain partial structures that relate the theoretical elements of T with the corresponding empirical domain. Such partial structures are to be called *empirical structures*. A' is an empirical structure *iff*:

- $A' =< D', R_i^{n\prime} >_{i \in I}$.
- $D' \subseteq \boldsymbol{D}$ and $R_i' = R_i^{n\prime} \cap D^{n\prime}$ for some $i \in I$

Any partial structure is said to be *A−normal* if it extends the partial relations R_i^n in A to normal total ones (its n-place relations are defined for all n-tuples of

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[7] A partial relation R_i over D is a relation that is not necessarily defined for all n-tuples of elements of D (see da Costa & French 1990: 255).

elements of its domain) (see Bueno, 1997: 592). This is, a structure B is said to be an $A-$*normal* structure if:

- B's domain is D,
- the relations in B extend the partial relations of A, and
- if c is a constant in the language considered, then in both A and B it is interpreted by the same element (Bueno, 1997: 592).

Finally, a sentence s is *quasi-true* in A according to B if:

- A is a partial structure
- B is an $A-$*normal* structure,
- s is true in B (in conformity with Tarski's definition of truth).[8]

"So, roughly speaking, a sentence s is pragmatically true in a partial structure A if there is some $A-$*normal* (total) structure B in which s is true." (Bueno, 1997: 592).

In the next subsections I say more about how the notion of quasi-truth has an impact in how we identify cases of inconsistency toleration when dealing with contradictions between theory and observation.

But before moving to empirical inconsistent theories, I would like to stress once again the reason why I decided to characterize scientific theories according to the *PPEE*: First of all, inconsistency toleration often happens within a theory and its applications, therefore, to provide a way to punctually characterize empirical theories is extremely relevant for the study of inconsistency toleration in the empirical sciences. Second, the *PPEE* to scientific theories can model both the presence and use of incomplete and partial information in the sciences, and the ignorance associated with such type of information − this ignorance, if what was said in the previous chapter is along the right lines, plays an extremely important role when explaining under which circumstances are scientists rationally inclined to tolerate a contradiction. Finally, the simplest versions of *PPEE* provide a guidance for modeling empirical theories but do not take any stand on what theories *really are* nor on what they should be. As a matter of fact, the *PPEE*−characterization of empirical theories that I have provided here, is mostly free of philosophical commitments (for instance, about the status of the different theories of truth or any other type or realistic commitments).

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[8] If s is not pragmatically (or quasi-) true in A according to B, we say that s is pragmatically (or quasi-) false (in A according to B).

2.2.3 Inconsistent empirical theory

An *inconsistent empirical theory* is a theory that contains both a sentence s (taken to be quasi-true) as well as a sentence $\neg s$ (also taken to be quasi-true).

> "it should be noted that quasi-truth is strictly weaker than truth, in the sense that if a sentence is true, then it is quasi-true, but the converse doesn't hold in general. Moreover, if a sentence is quasi-false, then it is false, but the converse doesn't hold in general either. (For further details, see Bueno [2000], and da Costa and French [2003].) Furthermore, it is also possible that a sentence s is quasi-true and its negation, $\neg s$, is quasi-true as well." (Bueno & French, 2011: 860)

An inconsistent empirical theory can, despite the presence of a contradiction, be non-trivial, this will depend on either the theory or the scientists' reasoning to be non explosive and, therefore, inconsistency tolerant.

Any empirical scientific theory is susceptible to be inconsistent in, at least, three different ways: 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 (Cf. Kuhn, 1977). This is,

> if we distinguish between observation and theory (what cannot be observed), then three different types of contradiction are particularly noteworthy for our purposes: between theory and observation, between theory and theory, and internal to the theory itself (Cf. Priest, 2002: 144).

Therefore, one can recognize, at least, three *types of contradictions in the empirical sciences*, namely, a theory T' could be:

> **Internally inconsistent** if a pair of partially true sentences, s and $\neg s$, is found in the structure A' of T', and such A' contains explicitly only the theoretical (non-empirical) elements of T'.
>
> **Inconsistent with observation** (this is, T' would be partially empirically inadequate) if two empirical structures of T', A' and A'', contain a pair of partially true sentences, s and $\neg s$.

lastly, two theories, T' and T'', would be

> **Mutually inconsistent** if, there is a partial-function that maps elements from one to the other, and a substructure of T' contains the partially true sentence s and a substructure of T'' contains the quasi-true sentence $\neg s$.

From these types of contradictions, the contradictions between theory and observation are the most common in scientific practice. For this reason, scientists have often regarded as the less harmful of the three of them. Therefore, it is sensible to suspect that the mechanisms that scientists have developed for tolerating this type of contradictions are sophisticated enough to, in the long run, help philosophers to shed light on optimal ways to understand the general phenomenon of inconsistency toleration in scientific reasoning. In what follows, I focus on discussing in more detail this type of contradictions.

2.2.4 Anomalies

A *contradiction between theory and observation*, also known as *anomly*, takes place when an empirical theory T has s as an observational consequence (prediction, description, etc.); however, a reliable experiment reports $\neg s$. And both, s and $\neg s$ are taken as quasi-true.

This is,

(i) Given an empirical theory $T =< \boldsymbol{D}, \boldsymbol{R}_i^n >$;

(ii) A_{pred} and A_{exp} are empirical substructures of T [9]

(iii) $A_{pred} \models s$ and $s \in T$;

(iv) $A_{exp} \models \neg s$ and $\neg s \in T$.

As I mentioned before, contradictions between theory and observation are generally called *anomalies.*

As empirical sciences legitimize, through their methodologies, the role of observation as fundamental for the construction, choice and application of scientific theories, if wanted 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).

Contradictions between theory and observation are generally called "*anomalies*". An anomaly is a contradiction that occurs when (i) there is an empirical theory T that has s as an observational consequence, and (ii) an reliable experiment reports $\neg s$.

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[9] For which, the former is constructed including the predictions of the theory with respect to $\boldsymbol{D}$ and the latter including the experimental observations about $\boldsymbol{D}$ in T.

When compared with internal contradictions (contradictions within a theory), anomalies stand out for both the naturalness and the frequency in which they are tolerated. As a matter of fact, philosophers and scientists would agree that, when identifying a contradiction of this type neither the theory in question nor the observational reports need, irremediably, to be abandoned. In this respect, it has been argued that, in the majority of cases, both the theory and the reports can be accepted *pro tem* while a new theory or better instruments are designed in order to dissolve the problem. Some examples of this type of contradictions are the anomaly in the precession of Mercury's perihelion and Prout's hypothesis (Laudan, 1977).

Many more things can be said as regards to inconsistent empirical theories, but for the purposes of our discussion, this will suffice.

2.3 Against inconsistency toleration

This section is devoted to introducing one of the strongest arguments from the Traditional view against the possibility of inconsistency toleration in the empirical sciences. In particular, it has been argued that when

> [F]aced with a theory that is known to be inconsistent, 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)

According to this view, whenever a contradiction is identified, scientists 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 to denying inconsistency toleration in science.

From the outset, it should be highlighted that Davey's main concern is not to deny that, sometimes, contradictions appear during the development of some scientific theories. Rather, he aims at showing that whenever a contradiction arises in a theory in use, scientists are able to neatly trust only the consistent part of the theory and, in a sense, isolate the inconsistent part. Therefore, as scientists only work with consistent theories, inconsistency toleration never takes place in the empirical sciences (nor ever is needed). All this said, let me to look in detail at Davey's general argument against inconsistency toleration.

Davey's main argument can be summarized through the following six points.

- Empirical theories are expected to provide scientists with reliable information about the external world, information that can help them to measure, predict, anticipate, and modify some aspects of particular empirical domains (Hempel, 2000). For these theories to allow us to do so, they have to offer explanations and also guide us getting about the studied domain.[10] As a matter of fact, giving accurate explanations (and predictions) is the main goal of a scientific theory. Without its predictive or explanatory power, any 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.

- If the predictions of a specific empirical theory are fulfilled and its explanations actually help to us understand the empirical domain the theory aims at explaining, then belief in the theory is *justified* (Davey, 2014: 3012). Therefore, the *bona-fide* consequences of the theory are the only thing 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.

- All of the sentences that form an empirical theory are needed and used for explaining or predicting some relevant phenomenon. Thus, 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”(Davey, 2014: 3013). [11]

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[10] For the sake of the argument, I do 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, allows one to consider a greater number of functional theories.

[11] This gives the impression that it is impossible to provide proper scientific explanations when employing propositions that the scientists do not take to be true. However, this clearly conflicts with the scientists’ constant use of highly idealized models, fictions, among others. As a matter of fact, scientists usually employ clear falsehoods (such as fictions, abstractions, idealizations) in order to obtain accurate measurements, descriptions, predictions and explanations regarding their object of study, these practices show that not only can scientists rely on false information, but that they actually do in their day-to-day practice. I say more

- 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). 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 one assumes the possibility of formalizing empirical theories suitably, and if one admits the constraints of classical logic, then a set of beliefs expressed as a collection of sentences will explode immediately once a contradiction is formed. The explosive character of a particular theory means that if this theory contains a contradiction, then it is possible to infer from it any proposition; regarding scientific theories, this could be translated into the lack of criteria for selecting and distinguishing between good inferential products and wrong inferential products. 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 is quite obvious that this inconsistent theory should be immediately rejected.

- Due to the negative connotation of inconsistencies, in practice, scientists always find a way 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 contradiction, 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, I call this *inconsistency isolation condition.* Something important to be considered is that, for Davey, this condition is satisfied if and only if, it is possible for scientists to identify satisfactorily a part of the theory as the 'source' of the contradiction and also if it is possible for them to isolate this part in such a way that the contradiction can somehow be dispensed with or avoided.[12]

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about this in Sec. 2.4.

[12] I want to thank an anonymous referee for asking me to point this and for helping me to give

- If discovering a contradiction in one of 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. Therefore, no inconsistency is tolerated in science.

One can reply to this argument in at least two different ways: either one can object to Davey's classical commitments, or one can object to the third and fifth premises of his argument (regarding *reliability* and the *possibility of separating the consistent part of the theory from the inconsistency-causing part*). Here I have chosen the second way, namely, to show that, even if accepting Davey's classical commitments, his argument is not robust enough to satisfactorily rule out inconsistency toleration in science.

2.4 In favor of inconsistency toleration

My central commitment here is to respond to Davey's argument against the possibility of inconsistency toleration in the empirical sciences. I expect that my response does not only show that Davey overlooks important elements of scientific knowledge, but that, even if the basic assumptions of his argument were correct, inconsistency toleration would still be possible. In particular, I expect the conclusion of this section to be that inconsistency actually takes place in the empirical sciences (thesis $\ll IT \gg$). In order to do so, first, I introduce an argument against Davey's naïve description of scientific theories; later on, I argue that, even with a more sophisticated notion of 'scientific theory', the inconsistency isolation condition cannot be fulfilled because it ignores from the outset the holistic properties of standard empirical theories.

2.4.1 Theories and justified beliefs

Davey understands empirical theories to be (exclusively) sets of statements about specific empirical domains, which could be the object of justified belief depending on the predictive and explanatory success of the statements given the whole theory. However, Davey overlooks extremely important aspects of scientific knowledge. Let me press this point further.

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a better phrasing of my ideas on this point.

- First, the statements involved in actual empirical theories are diverse. The propositions that contained by scientific theories can have the shape of general laws, auxiliary statements, empirical constraints, etc.; and if one ignores this, one is neglecting the way in which actual theories are (Kuhn, 1970).

- In the scientific practice, scientists tend to use "false elements" in order to obtain accurate predictions, to identify more simple inferential patters, among others. However, the use of this knowingly false information is not irrational (as it was argued in Sec. 1.2.2). For instance, in order to obtain accurate predictions in a minimal amount of steps, sometimes scientists treat the Sun and Earth as if they were the only members of an isolated physical system, something that we all know is false but also a simplification of important heuristic value for the ordinary scientific practice (Putnam, 1981). Therefore, when leaving aside this type of distinctions, one is neglecting one of the main characteristics of the practices associated to the use of scientific theories.

- In addition, if one does 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 scientists achieve justification for their beliefs.[13]

- In cases where false statements are used and where the obtained predictions are empirically adequate, if one trusts Davey, scientists would be required to rely on *all* the sentences that played any role for reaching the predictions in question, including the false assumptions.[14] And more importantly, if stated that reliability comes only from corroboration (as Davey requires), it is well known that it is impossible, at least for finite agents such as scientists, to corroborate all the logical consequences of empirical theories (Putnam, 1981). This would make impossible for scientists to be justified

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[13] For instance, scientists are justified to believe some assumptions but not because they regard them to be candidates for the (partial) but because these assumptions are heuristically extremely useful. Some scientific models are good examples of this when being successful most of the time but also known to be not-true. If scientists believe in such models, they cannot believe them in the sense to be true; however, due to the success that they bring to the scientific practice, certain beliefs towards them are justified. Thanks to Peter Vickers for the pointers.

[14] This, of course, does not mean that scientists rank all the sentences equally, nor that all of them are literally believed.

> in believing in any empirical theory complex enough to describe a relevant empirical domain.

Thus, if one wants Davey's argument (as well as my reply) to hold for actual scientific theories, one should recognize at least that, sometimes due to the diversity of the elements of empirical theories, (epistemic) justification does not come directly from the predictive or explanatory success of parts of the theory; and more importantly, one should recognize that, sometimes, scientists are justified to believe some assumptions because they trust their pragmatic benefits but not because their regard them as true.

2.4.2 Holism and the withdrawal of *inconsistency isolation*

It is 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 a contradiction is noticed. 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 diverse relationships 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 contradiction).

Nevertheless, considering the way holism have 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 rather clearer: once a contradiction, whether with observation or rival theories, is identified, the price to be paid has to be minimal because to isolate an individual part of one theory will not affect directly the other theory involved. 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 contradiction, which – in accordance with Davey's view –seems to work just fine.[15]

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

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[15] In Sec. 2.5, I offer an example of how Davey's inconsistency isolation condition works while facing an contradiction 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 contradiction has been 'removed'. So, while talking about inconsistency-detection, appealing to the holism that is 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 contradiction would have to be tolerated at least temporarily.[16]

However, Davey might reply to our characterization of the *inconsistency isolation condition* by saying that he is talking about something less strong than that: that when finding a theory T to be inconsistent in certain context, we stop trusting its consequences regarding that particular framework and we understand that T is not functional in that exact context, we (might) reject the particular

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[16] A similar stand point is presented in [Bueno, 2006], and some particular objections are introduced in [Vickers, 2013: Chap. 8].

inference(s) in that exact context as unreliable, while maintaining that the theory is reliable in other contexts. Now, T 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 T 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, if wanted to talk 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 presence of holism in 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 three 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.

2.5 Historical Evidence

In Sec. 2.4, I argued in favor of the possibility of inconsistency toleration in the sciences (***thesis*≪*IT*≫**). In what follows, I support such a claim by presenting three case studies that are exemplars of inconsistency toleration when facing contradictions between theory and observation in the empirical sciences.

2.5.1 The *Anomalous* Behavior of Mercury

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.

2.5.2 Explaining the case study

Were scientists ignorant in this case? Even though several astronomers offered many auxiliary hypotheses to resolve the problem (such as the presence of another planet, Vulcan, that interfered with Mercury's orbit, among others), the theory could never explain the anomalous orbit of Mercury. As a matter of fact, the problem was solved only when providing an alternative theory, namely, Einstein's theory of General Relativity. However, the exhaustive search for an explanation of the anomaly and the amount of candidates (compatible with the background theory) that were provided by the scientists at the time, show that astronomers were not necessarily ignorant about the structure of the Newtonian theory –as they were able to explain under which circumstances, being the background theory true, could the precession be regarded as *normal*. This is, scientists at the time understood their theory well enough to identify it's relevant logical consequences that could have helped to solve the anomaly, if the Newtonian theory was true; however, which they actually ignored was which of the assumptions of the theory were false, and thus, why the theoretical description of the empirical domain was mistaken.

Was this a contradiction? 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' perihelion), it's plausible to assume the observational consequence (prediction) of the theory T is inconsistent with the observational reports. So, in this case that we have a functional yet still observationally inconsistent theory.

Was explosion "avoided"? The way in which scientists escape from logical triviality in this particular case could be explained through the use of a *dividing-tool*. The explanation goes as follows. 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 shows that the scientific community was not willing to tolerate inconsistency in any sense. In addition, it has been stated that the presence of new auxiliary hypothesis offered at the moment reveals that the scientific community had located the inconsistent 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).

Finally, when addressing this case study philosophers of science usually appeal to a sort of inconsistency isolation methodology. They often argue that scientists made a sort of fragmentation of the theory by separating the reliable part of the theory from the unreliable part. The alleged division is guided by different degrees of epistemic commitments. According to Davey (2014), scientist at the time were, clearly, *confident* about the truth of the theory but *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 and avoid putting the contradictory information together. So, no contradictions were explicitly formed, and that's why explosion was avoided.

2.5.3 The *anomalous* magic numbers

In a nutshell, the case study goes as follows: the Liquid Drop Model and the Shell Model contain incompatible basic principles regarding the structure of the nucleus of an atom; it is only when nuclear physicists combine some of the predictions of both models that they gain accuracy in their predictions and measurements of binding energies for all the chemical elements of the periodic table and in their predictions and explanations of other nuclear processes such as fission. This case study illustrates a scenario in which each model can accurately predict only a segment of the elements in the periodic table and only part of a general phenomenon, but in which combining the predictions of both models provides successful descriptions and predictions of more general phenomena.

Some preliminaries: The nucleus of an atom is the small region in which 99.9% of the total mass of the atom is located. The nucleus consists in protons and neutrons bound together. The behavior of the nucleus is explained by appealing to two different forces: the strong nuclear force and the weak nuclear force. The strong nuclear force is what binds nucleons (protons and neutrons) into atomic nuclei, while the weak force is responsible for the decay of neutrons to protons. The binding energy of a nucleus is what in large part determines the stability of the nucleus. Any atomic nucleus (of any chemical element) will exhibit binding between protons and neutrons and decay of neutrons and protons. Finally, our current nuclear physics has provided us with, at least, 31 nuclear models that allow us to, at least, describe, predict and measure this type of behavior of atomic nuclei. (Cf. Cook 2006, Morrison 2015: Chap. 5).[17]

On the one hand, the Liquid Drop Model (*LDM*) is one of the most successful nuclear models. It was formulated under the assumption that the nucleus of an atom exhibits classical behavior (protons and neutrons strongly interact with an internal repulsive force proportional to the number of nucleons). On the other hand, the Shell Model (*SM*) is a nuclear model according to which a shell represents the energy level in which particles of the same energy exist, and so, the elementary particles are located in different shells of the nucleus. According to the *SM* the nucleus itself exhibits quantum-mechanical behavior (Heyde, 1994: 58); that is, for this model "nucleons are assumed to be point particles free to orbit within the nucleus, due to the net attractive force that acts between them and produces a net potential well drawing all the nucleons toward the center rather than toward other nucleons" (Morrison, 2015: 185).

To measure binding energies of different nuclei, physicists have always preferred *LDM*, as it is extremely simple and highly accurate. However, while this model is efficiently used for predicting binding energies and fission of many elements, *LDM* faces serious difficulties when addressing the behavior of atoms of Helium (He), Oxygen (O), Calcium (Ca), Nickel (Ni) and Lead (Pb). Such elements' nuclei are bound more tightly together than predicted by the *LDM* depending on the number of nucleons that they possess. This is the so-called 'magic numbers' phenomenon. Yet, *SM* can predict binding energies of nuclei with magic numbers (and, oddly, only nuclei of magic numbers). So, if physicists want to measure

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[17] The diversity of models itself is not problematic; especially if "each model has its particular successes, and together they are sometimes taken as complementary insofar as each contributes to an overall explanation of the experimental data" (Morrison 2015: 179). However, the case study that I am presenting here illustrates how the basic assumptions required by one model contradict those required by another model.

binding energies of all elements' nuclei they have to, sometimes, see the nucleus as classical and, some other times, as a quantum object. As could be obvious to the reader, to assume that the atom is describable as a classical entity as well as a quantum object is at its best, problematic, and at its worst, inconsistent.

It is well known that nuclear physicists do not take both models as candidates for the partial truth, they only use them (and combine them) in order to get accurate predictions and measurements, but they do not believe that both models put together describe realistically the empirical domain they 'talk about'. Nonetheless, this case study demands an explanation about how an inconsistent combination of information –interpreted realistically or not- could entail accurate predictions and how scientists could avoid triviality at the same time. Cases like this one require then an analysis in terms of inferential procedures that are useful (or needed) for the avoidance of triviality while tolerating inconsistencies, i.e., an explanation in terms of logic.[18]

2.5.4 Explaining the case study

Were scientists ignorant in this case? While it might be clear to the reader that scientists know quite well under which circumstances each of the models can be false (especially when using them together), what originally happened in this case was that, at first, they ignored the truth values of some of the propositions of the *LDM* when dealing with atoms of Helium, Oxygen, Calcium, Nickel or Lead. Once they were aware of the falsity of some of the *LDM* consequences regarding atoms of these elements, they lacked an explanation for this. Later on, when they developed an understanding of the phenomenon of *magic numbers* and they realized that, in order to reach accurate predictions, they had to combine two incompatible models (*LDM* and *SM*), it became clear that they lacked of knowledge of how to provide a consistent uniform model that could be used for predicting fission for all atoms from the periodic table.

Was it a contradiction what was tolerated? The fact that they had to combine mutually contradictory assumptions (from incompatible models) left scientists in such a position in which if they wanted to reason classically or constructively, and avoid triviality when predicting fission for all the elements from the periodic table, they either have to get rid of some basic assumptions of specific models (by deciding that they are in fact idle, for instance), or they have to find a way to

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[18] For a more comprehensive discussion of this case study see [Friend & Martínez-Ordaz, 2018: 73–76].

connect the consequences of both models without allowing explosive reasoning.

Additionally, it is well known that nuclear physicists do not take both models as candidates for the partial truth, they only use them (and combine them) in order to get accurate predictions and measurements, but they do not believe that both models put together describe realistically the empirical domain they 'talk about'. Nonetheless, this case study demands an explanation about how an inconsistent combination of information –interpreted realistically or not- could entail accurate predictions and how scientists could avoid triviality at the same time.

One of the most interesting aspects of this case study is that, if one focuses on each of the models alone it is not obvious that they exemplify a contradiction between theory and observation; each scenario alone can still allow for the possibility of the problem being an explanatory or predictive gap (like the Kuperian lacunae (Kuipers, 1999, 2000)) more than a contradiction. This is, the fact that the LDM cannot predict adequately the binding energies of certain elements (such as Helium, Oxygen, Calcium, Nickel and Lead) could be understood as a symptom of the scientists' lack of knowledge of the properties of such elements. Nonetheless, when contrasting the effectiveness of LDM and SM regarding both the calculation of binding energies and the prediction of fission, it is clear that LDM miscalculates the binding energies of elements like Helium and Oxygen, and that this is not a case of ignorance about the properties of such elements but a case in which a theoretical model, T, entails a prediction, s, that is contradicted by experimental reports, $\neg s$.

2.5.5 The Solar neutrino anomaly

As I have previously argued, 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 $\frac{1}{2}$ 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 the scientific community 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 of 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, I 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 charge less particles which only interact via the weak interaction, an experimenter cannot in any 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. As a result of the interaction ($Cl^{37} + \nu \rightarrow Ar^{37} + e^{-}$), 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: 41-43)

However, not all the *clicks* that were reported by the Geiger counter were understood as final observational outcomes, some of these clicks were assumed to be generated by other sources. In order to identify the ‘correct’ measurement of the solar neutrinos’ 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.

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

> [G]ave the number of counts he had detected; the number of background counts; the number of neutrino-induced events, $\sum \psi\sigma$, (SNUs); and the boron-eight neutrino flux, ${\psi_B}^8$(...) he compared Bahcall’s latest predicted value of this flux, (${\psi_B}^8 = 1.4\ (1 \pm 0.6) \times 10^7$ neutrinos cm^{-2} sec^{-1}) with his own observed value, ${\psi_B}^8 \leqslant 0.5 \times 10^7$ 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: 121-122)

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.[19] These were indeed the modifications that in the long run helped to solve the anomaly in 2001.[20]

2.5.6 Explaining the case study

As it was argued in Sec. 1.4.5, I consider this episode to be an exemplar of ignorance of theoretical structure. That considered, here I explore if such peculiar type of ignorance had any effect in the way scientists dealt with the contradiction, I contend that it did indeed. As a matter of fact, I consider that through the study of this case it is easy to spot a particular limitation of the *fragmentative approaches* (such as the one submitted by Davey (2014) that was discussed in Sec. 2.3) and such a difficulty is neatly connected to the source of the ignorance that motivated the toleration in the first place.

First, allow me to explain once more what *ignorance of theoretical structure*

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[19] 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). For this discovery, Kajita and McDonald were awarded with the 2015 Nobel Prize in Physics.

[20] For a more comprehensive reconstruction of the problem, see [Bahcall, 2003] and [Franklin, 2003].

consists of. First, this type of ignorance is absence of knowledge regarding the relevant inferential connections that a scientific theory (in this case, the SSM), allows for. Second, if ignoring the relevant parts of the theoretical structure of a theory, scientists are not capable of grasping causal connections between the propositions of their theory. Third, if ignorant of the theoretical structure of a particular theory, scientists cannot identify the logical consequences of the propositions that they are working with nor can they explain under which conditions the truth value of such propositions will be false. Finally, a possible cause of this type of ignorance is be the holistic character of bodies of scientific knowledge –if the degree of internal or external holism, is extremely high, scientists would not able to satisfactorily identify privileged inferential relations that allow them to test the truth value of the involved premises, among other things.

Since it was very difficult for the scientific community to point out where the inconsistency originated –they did not agree for a 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; i.e., provide a clear and satisfactory division for avoiding explosion. 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 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 from reemerging).

Therefore, at least for this case study, the ignorance that motivated the rational toleration was caused by the holistic nature of the theories involved in the anomaly (the theory that underlies the design of the experiment and the SSM); and those holistic properties (and the associated ignorance) were what prevented scientists from fragmenting adequately the theory –as for years, they seemed unable to provide neither a solution for the anomaly nor a partition of the theories in which strong consistency was preserved.

Did the scientists *tolerate* the contradiction? Even though the problem lasted for thirty years, and scientists were not sure about which part of the theory was the responsible one for the emergence of the anomaly, they were not empty-handed. 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 (Cf. Bahcall and Davis Jr., 1976; Bahcall, 1981, 2000; Pinch, 1986; Franklin, 2003). I consider this to be sufficient to argue that scientists kept using the SSM as a reliable theory despite the discovery of the inconsistency, and despite the impossibility of dividing the theory into clearly consistent fragments. I believe all this could only be understood as if they were tolerating the inconsistency.

I hope that these three case studies work as evidence in favor *thesis* $\ll IT \gg$; this is, *pace* Davey and the Traditional view, inconsistency toleration actually takes place in the empirical sciences. In the next section, I explain how these cases also entail a methodological conclusion, namely that, in the empirical sciences, one could identify

2.6 Anomalies vs. Logical Contradictions

Considering the significant differences between the three cases of inconsistency toleration that I presented in the previous section, I have reached the conclusion that there are, at least, three types of contradictions between theory and observation. In what follows, I call this conclusion the *Many Contradictions between Theory and Observation-thesis* (henceforth, ***thesis*** $MC_{(T^{-}O)}$).

I consider that to provide a way to identify and analyze contradictions in the empirical sciences can lead philosophers and logicians of science to achieve a better understanding of the general phenomenon of inconsistency toleration in the sciences. Therefore, this section is devoted to characterize these three different contradictions. In order to do so, I proceed in two steps: I first introduce the distinction between anomalies and logical contradictions (Cf. Laudan, 1977, Chap. 2); later on, I argue in favor of identifying, at least, three types of contradictions between theory and observation.

2.6.1 Anomalies and logical contradictions

The unification of inconsistencies between theory and observation under the one and only label of *anomaly* has been a mistake (see Sec. 2.2.4). As a matter of fact, here I contend that it is of prominent importance to distinguish *anomalies* (as Kuiperian−lagunae) from *logical contradictions.*

Anomalies consist of the presence of a statement (generally some kind of observational outcome), s, such that when combined with a particular theory, T, and with a ceteris-paribus clause the statement becomes a potential falsifying statement for the theory (see Lakatos, 1977: 40). Under this definition, the three case studies presented before are anomalies. However, anomalies are of two different types: *lacunae* (Cf. Kuipers, 1999, 2000), also associated to *abductive novelties* (Aliseda-Llera, 2006: 47), and *logical contradictions* (Cf. Laudan, 1977). Allow me to characterize them in more detail.

Given a body of empirical knowledge (theory), T, which applies to a particular domain, D; $T =< \boldsymbol{D}; \boldsymbol{R}_i^n >$ –where R_i is a family of n-place relations holding between the elements of D. In addition, take A_{pred} and A_{exp} to be empirical substructures of T and s to be an observational prediction of T (A_{pred}). That said,

Lacunae: are either explanatory or predictive gaps of the following form:

1. $A_{pred} \not\models s$
2. $A_{exp} \not\models \neg s$

$\therefore$ $\neg s$ is *novel* with respect to T

$\neg s$ is novel in the sense that it is not explained or predicted by T; however, it is consistent with T.[21]

Logical contradictions: consist of contradictions between a prediction, s, that T entails and an observation, $\neg s$. This is:

1. $A_{pred} \models s$
2. $A_{exp} \models \neg s$

$\therefore$ $\neg s$ is *anomalous* with respect to T

s is anomalous in the sense that it is not explained or predicted by T's A_{pred}.[22]

Given this understanding of *lacunae* and *logical contradictions*, it should be easy for the reader to see that the three anomalies that were introduced in Sec. 2.5 are of the latter type. However, even if the three case studies are all logical contradictions, they look more different than alike from each other:

The ***anomalous*** **behavior of Mercury:** This case illustrates a neat contradiction between theory and observation. An empirical theory, T, predicts s; however, s is not observed. After considering different explanations and calibrating the acceptable margin of error, scientists cannot do anything but recognize the observational result as the negation of s, this is, $\neg s$.

The ***anomalous*** **magic numbers:** This case study illustrates a contradiction between theory and observation which only becomes visible when

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[21] Under the name of *abductive novelties*, a similar phenomenon was characterized in [Aliseda-Llera, 2006:47].

[22] Under the name of *abductive anomalies*, a similar phenomenon was characterized in [Aliseda-Llera, 2006: 47].

> comparing the results of two different theoretical frameworks that work with the same information but have incompatible results.
>
> The **Solar neutrino *anomaly*:** While this case study illustrates a contradiction between theory and observation, it also shows that tolerate a contradiction does not imply to have a recipe for the isolation of the contradiction. This is, scientists can hold a tolerant attitude towards a contradiction even if they ignore the actual cause of the inconsistency.

This said, if one assumes 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 is, in both cases the predictions of the theory are contradicted by the observational results. Yet, there are some considerable differences between the two of them (Cf. Martínez-Ordaz, 2017: 138-140). In the following subsection, I argue that sometimes the non-satisfaction of an observational independence criterion could play an important role when studying inconsistencies between theory and observation.

2.6.2 Some methodological morals

Here I provide a methodological guide for the recognition of contradictions between theory and observation in the empirical sciences. In order to do so, I evaluate how the particularities of each of the historical exemplars that I have presented in this chapter can interact and shed light in the differences between types of contradictions between theory and observation.

First of all, as it was argued in the previous subsection, and illustrated by the case studies of the perihelion of Mercury and the Magic Numbers, contradictions between theory and observation (always) fulfill the following criteria:

(1) Given an empirical theory T, there is an observational consequence of T, s.

(2) Experimental reports show that instead of s being the case, r is the case.

(3) (1) and (2) obtained, r cannot be explained by appealing to an *acceptable* margin of error, therefore the anomaly is acknowledged as a contradiction.

But, *are the above sufficient conditions for characterizing logical contradictions (between theory and observation)*?

If contrasting these conditions with the case studies presented previously in this chapter, the reader will notice that the first case study presented in Sec. 2.5, the anomaly in the precession of the perihelion of Mercury, satisfies all these three conditions. In addition, for that particular case, the satisfaction of (1)-(3) seems to be sufficient for the anomaly to be a logical contradiction.

However, not all anomalies behave equally. For instance, in contrast with the case study on the perihelion of Mercury, even if the anomaly of the magic numbers satisfies criteria (1)-(3), this is not enough for justifying the claim of a logical contradiction. Scientists could address the anomalous behavior of some nuclei (the ones with magic numbers) only focusing on the inaccurate predictions that LDM provides, when doing so, physicists face the dilemma of either accepting that the behavior of certain nuclei is unpredictable given the current understanding of atomic nuclei in general, or accepting that (some of) the predictions of one of their most successful model contradict with experimental reports.

As the large majority of scientists would consider empirical sciences as an endeavor of constantly dealing with incomplete information about the world (see Bueno, 1999), it seems natural to explain out the anomaly by accepting that we have not yet enough information, in the framework of LDM , about atomic nuclei and describing the problem close to an abductive novelty -to do so prevents seeing this as a contradiction. Nonetheless, when providing an additional framework, the framework of SM, it becomes clear that physicists actually possess enough empirical and theoretical information for predicting accurately the behavior of atomic nuclei with magic numbers, only not with the help of the framework of LDM, in light of this, overall, the anomaly should be seen as a contradiction between theory (LDM) and observation.

In light of the above, I regard of prominent importance to update the criteria previously introduced.

Consider the following: If r were not explainable by appealing to a relevant margin of error, this does not guaranty that r contradicts s. As a matter of fact, in this scenario, r could be the result of an explanatory-predictive gap. If the anomaly were to be caused by lack of sufficient information, it would not be surprising that scientists were to provide mistaken predictions.

With this in mind, it seems sensible to include a forth criterion that helps to refine the way in which we distinguish between anomalies of the type of Kuiperian lacunae and anomalies that are logical contradictions. Such criterion should require that, when identifying an anomaly, it will be needed to provide additional evidence that all the relevant information at the scientists disposal was taken into account, but that even with that information being considered, the prediction and the report contradicted each other.This is,

> (4) One can claim to have identified a contradiction between theory and observation if and only if: (1), (2), (3) are satisfied and there is a different framework which shows that, all things considered, the information at the scientists' disposal is enough to provide accurate predictions about x but nonetheless, the theory provides a prediction that is not satisfied -and the experimental reports show that the difference between the prediction and the observation is larger than a relevant margin of error.

So far, the two case studies (the anomalies of the perihelion of Mercury and the anomalous Magic Numbers) are compatible with the traditional fragmetative approaches for dealing with contradictions (such as the approach provided by Davey in Sec. 2.3). In both cases scientists know when to either change the methodology or to stop trusting the predictions of the theory in order to avoid forming contradictions (and thus, in order to avoid logical explosion).

A characteristic feature of inconsistencies between theory-observation is that they involve *exclusively* a prediction of a given theory and a specific observational report -such as that contradicts the prediction in question. However, from what we have seen in the third case study (the anomaly in the measuring of the solar neutrinos flux), this is not always as neat as expected. If a contradiction between theory and observation involves exclusively a prediction and an observational report, the following criterion is satisfied:

Call this the

> **Observational Independence Criterion**: The set of propositions that underlie the design of instruments and methods used to evaluate the observational consequences of T, ideally, are achieved totally independently of the propositions belonging to the theory in question (Martínez-Ordaz, 2017: 140).

This criterion 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 (T), or the relation between an assumption of T and another one that is used for the designing of the experiment, or the relation between an assumption of T 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 of 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.

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 theoretical 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 close relationships between theoretical and experimental elements prevented the observational independence criterion from 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 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 close relations between theories is what entails the impossibility of the observation being fully independent of the theory in question. However, this is a common characteristic of scientific theories, 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 episodes.

This particular exemplar from the history of science seems to challenge the fragmentative approaches to inconsistent science. Yes, scientists were able to segment the theory in many different ways, but none of such ways was successful, none of those partitions behaved as Davey (2014) and others have recommended, none of such partitions was able to fulfill the isolation condition. What the case study from neutrino physics shows is that, the mechanism that underlies the toleration of the inconsistency was more related to the constant change of partitions (due to the high degrees of ignorance and holism present in the case) than to the isolation of the *ill* part of the theory.

2.7 A typology of contradictions

If the considerations that I have advanced in the previous section are correct and if we want to analyze something more than toy examples, 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 three different subtypes of contradictions between theory and observation could be identified:

> **Inconsistency T-O (Indp)**: Given an empirical theory T that has s as an observational consequence, if an experiment is made, $\neg s$ is reported; also, there is an empty intersection between the subsets of the relevant assumptions of T and the relevant assumptions of the theory behind the design of the instruments and the design of the experiment.
>
> **Inconsistency T-O (AddFramework)**: Given an empirical theory T that has s as an observational consequence, if an experiment is made, r is reported. But there is another theoretical framework, *T'* which provides additional evidence of the legitimacy of interpreting r as $\neg s$.[23]
>
> **Inconsistency T-O(Aux):** Given an empirical theory T that has s as an observational consequence, if an experiment is made, $\neg s$ is reported; yet, there is an important overlap between the relevant assumptions of T 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).

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[23] Special thanks to Tessa Ligtenberg for her many valuable comments on this particular type of inconsistency.

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 the three case studies that I analyzed here 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.

2.8 Final remarks

In this chapter I have submitted the following:

> ***Thesis* ≪*IT*≫**: Inconsistency toleration is not only possible but we have sufficient historical evidence to conclude that it has actually taken place in science.
>
> ***Thesis MC*** $_{(T\text{-}O)}$**:** In the empirical sciences, there are, at least, three different types of contradictions between theory and observation; namely:
>
> - contradictions that satisfy a high degree of observational independence (*Inconsistency T-O (Indp)*),
> - contradictions that require the use of an additional theoretical framework to be identifiable (*Inconsistency T-O (AddFramework)*) and,
> - contradictions that illustrate a low degree of observational independence between the tested theory and a relevant auxiliary theory (*Inconsistency T-O(Aux)*)

In order to argue in favor of these two theses, in Sec. 2.2, I discussed some preliminary notions, such as *empirical theory*, *inconsistent empirical theory* and *contradiction between theory and observation*. Sec. 2.3, was devoted to introduce an argument against the inconsistency toleration in the sciences. In Sec. 2.4, I provided a philosophical response to such an argument and in Sec. 2.5, I supported my response with historical evidence. In this section, I introduced three case studies that illustrate the toleration of contradictions between theory and observation, such cases were the anomaly in the perihelion of Mercury, the anomaly in the meassuring of solar neutrinos' flux and the anomalous magic numbers. In Sec. 2.6, I offered a guide for the identification of contradictions between theory and observation in the empirical sciences. In Sec. 2.7, I provided a typology of contradictions between theory and observation.

The relevance of this chapter has been to offer both historical evidence of inconsistency toleration, as well as a set of methodological criteria for analyzing and classifying such evidence. In the next chapter, I focus on discussing how these case studies can support philosophical theses about inconsistency toleration and enhance philosophers' understanding of the phenomenon of the toleration of contradictions in the sciences.

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3 Biased reconstructions of inconsistency toleration

3.1 Introduction

This chapter aims at contributing to the methodology of history and philosophy of science. Here, I deal with a serious objection to the way in which historical evidence has been used to support philosophical theses about contradiction in science, namely:

> if one looks more carefully at the relevant history o science, most of these cases are *not* really inconsistent in any significant sense after all. *And to reconstruct such cases as inconsistent sets of propositions is a highly dubious move. Often when this is done the motivation seems to be to find an application for a paraconsistent logic, and not to say something interesting or important about how science works, or even could work* (...) There are just too many philosophers failing to read up on the relevant history, partially because is an unfortunate culture of publishing philosophy of science which makes no attempt to engage with the real scientific or historical facts. (Vickers, 2013: 252)

If correct, this objection jeopardizes the value of the philosophical theses defended by the Paraconsistent view when supplemented with historical evidence, and in particular, weakens the value of the theses presented here in Chap. 1 and Chap. 2, as, so far, their strength depends on the historical evidence that supports them.

If the historical case studies that have been provided to support paraconsistent theses about science are biased, one would not be able to benefit from them in any significant way as they would lack of both an independent "empirical" support and methodological rigor.

As a response to this objection, here, I contend that, even if the historical reconstructions that have been presented in favor of inconsistency toleration were biased, they could still play an important role for achieving philosophical understanding of the tolerance of contradictions in the empirical sciences –in particular, of contradictions between theory and observation.[1]

The main concern of this chapter is to address the question of *what could be the value of historically inaccurate reconstructions, when philosophically biased, for the philosophy of science*. Two main answers have been provided to such a question: On the one hand, in [Currie & Walsh, 2019] it has been argued that, due to their nature, historical explanations, either from history or philosophy, always require to have "'principles of selection' that guide in identifying the relevant and irrelevant aspects of the target episode: they tell us what to foreground and what to background (...) and provide contrasts and comparisons, that is, guidance as to which parts of the explanatory text to include and which to exclude" (2019: 10f). This considered, what could be initially, and mistakenly, regarded as 'philosophically biased', might be an important result of a (responsible) use of historiographical methodologies. Meaning that historically incomplete reconstructions could enable clearer historical explanations of episodes from the history of science.

On the other hand, the second answer to the question, first presented in [Martínez-Ordaz & Estrada-González, 2018], says that historically inaccurate reconstructions, when inaccurate due to a philosophical bias, can play a highly important epistemic role for the development of philosophy of science, namely, to enhance our understanding of philosophical theses about science. While this looks like an interesting and novel response, they did not tell how such an understanding is achievable nor why it is valuable. In this chapter, I aim at tackling such problems.

In order to do so, I defend that philosophically biased historical reconstructions can be seen as exemplars of the philosophical theses that they were 'designed' to support. In addition, I defend that, as exemplars, this type of reconstructions can promote philosophical understanding of the theses that they were designed to back up. Finally, I sustain that, for many cases, the value of historically inaccurate reconstructions could be epistemic, namely to provide an understanding

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[1] While this enterprise could be seen as a modest synonym of accepting that the battle for the historical accuracy in this debate is lost already, at least for the Paraconsistent view; nothing could be further from the truth. As a matter of fact, this chapter might be one of the most ambitious of the whole dissertation, as it consists on convincing the supporter of the Traditional view that, even if she does not accept the plausibility of inconsistency toleration in the sciences, there is still a valuable epistemic outcome that she could obtain from the study of cases of alleged inconsistent scientific reasoning.

of our philosophical theses.

The plan for the chapter is the following: In Sec. 3.2, I briefly present the Dilemma of Case Studies. Here I argue that the common responses to such a dilemma are not strong enough for ruling out the danger that philosophically biased historical reconstructions pose against the philosophical endeavor itself. In Sec. 3.3, I explain how exemplification can, under certain conditions, provide understanding, which is a valuable epistemic achievement. In Sec. 3.4, I defend that philosophically biased reconstructions could greatly benefit philosophy of science by enhancing philosophical understanding; in order to do so, I refer to the literature on scientific understanding and explain how these reconstructions, if taken as exemplars of philosophical theses, could be of important philosophical use. Finally, Sec. Sec. 3.5, is devoted to making some final remarks.

3.2 Historical evidence and philosophical theses: Some problems

If the historical record is altered in order to tell a story that exclusively supports a particular philosophical view, such a story is biased and should not be regarded as reliable evidence in favor of such a view. Here, in Sec. 3.2.1, I present a case study that illustrates how problematic is when one scientific episode is reconstructed in different and rival ways. In Sec. 3.2.2, I present one of the most important challenges that historically informed philosophy of science faces, namely, the *Dilemma of Case Studies* and explain why this dilemma constitutes a serious problem for the philosophical endeavor.

3.2.1 One episode, different reconstructions

There is a shared view in the philosophy of science which contends that any philosophical approach to science has to be entwined with our finest historical knowledge about science (Cf. Hanson, 1961; Kuhn, 1962; Laudan, 1977; Lakatos, 1978). However, in recent years, it has been argued that to reconcile history and philosophy of science is most of the time a hopeless task (Cf. Pitt, 2001; Schickore, 2011). Either philosophers end up finding themselves unable to obtain legitimate philosophical information from particular historical data, or historical data is tampered to fit a philosophical point –this is, philosophy develops from using *philosophically biased historical reconstructions.*

A philosophically biased historical reconstruction is a reconstruction that results from "cutting" historical evidence in such a way that it clearly fits a desired specific philosophical thesis. When the same historical episode, allegedly, supports two different (and rival) philosophical theses, there is the common intuition that at least one of the stories involved has been distorted to fit a philosophical point. Let me introduce an example of this.

In *The Analyst* (1734), Berkeley argued that the (Newtonian) Early Calculus had operated on an inconsistent basis. According to Berkeley, in *Principia* (1687), Newton characterized infinitesimals inconsistently; this is, Newtonian infinitesimals were entities that, at some points in a particular proof, were equal to zero and, at some other points, they were different than zero. Berkeley argued that this was a clear case of flawed mathematical practice.

Pace Berkeley, in recent years, it has been argued that historical evidence supports the claim that the use of the Newtonian Calculus was nothing but a good mathematical practice. Yet, this historical evidence has helped also to characterize such a practice as consistent as well as inconsistent. On the one hand, Brown and Priest (2004) used this historical episode to illustrate a case of 'good' inconsistent mathematics. Making this into a case of paraconsistent reasoning and satisfactory inconsistency toleration in the formal sciences (Brown & Priest, 2004: 379). They did not pay much attention to Berkeley's original claim nor to the evidence that Berkeley provided for his accusation, in contrast, they neglected the Newtonian theory of fluxions and focused on the contemporary analyses of the conceptual development of the calculus and the associated mathematical practices.

On the other hand, other philosophers have defended that the Newtonian Calculus was a safe case of consistent mathematics (See for instance Edwards, 1979; Vickers, 2013). The supporters of this view have claimed that if one pays attention to Newton's own words (in both, Principia and the introduction of *De Quadratura*), "Newton says, exposition in terms of indivisibles or infinitesimals is simply a convenient shorthand (but not a substitute) for rigorous mathematical proof in terms of ultimate ratios (limits)" (Edwards, 1979: 226). Therefore, Newtonian infinitesimals were never considered to be inconsistent and users of the calculus could "actually keep all of the propositions, and continue to reason with classical logic, and still trust many or most of one's inferences" (Vickers, 2013: 241). This is, there was no need of changing to another underlying logic that could tolerate contradictions. Thus, the associated mathematical practice was clearly good and consistent at the same time.

The diversity of philosophical theses related to this particular case is not in itself methodologically challenging. Actually, there is a common agreement on the fact that as (historical) events are extremely complex, it is nothing but methodologically acceptable to adopt different methodological (and philosophical) points of view in order to obtain a more detailed picture of the general episode. This is, history of science is often done "by developing a more or less explicit tool kit of frameworks, that is, a set of approaches that, judiciously applied, aid the historian and philosopher in navigating the complex, contingent episodes that concern them" (Currie & Walsh, 2019: 16). That considered, it would not be anomalous to have different reconstructions of the same episode if they shed light on different aspects of the historical event, especially if they are, in the long run, susceptible to be complementary to one another.

However, for the case of the different reconstructions of Newtonian infinitesimals, it is not so clear that the diversity of reconstructions is of the sort just described above. As a matter of fact, it seems that at least some of these reconstructions are rivals,[2,3] making it impossible to consider them to be mutually complementary. For this case, each of the different reconstructions that I have listed above aims at addressing the possibility of inconsistency toleration in Newtonian mathematical practice (paying special attention to the work of Newton), and proponents of each of them assume to have selected *all* the relevant historical information that is needed for satisfactorily explaining such a phenomenon. Nonetheless, in this case, the set of "relevant" information varies from reconstruction to reconstruction, making each of them neglect elements from the history of science that some of the others consider indispensable. This gives the impression that at least one of these historical reconstructions is overlooking relevant historical information, and thus, at least one of them is *historical inaccurate*.

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[2] I consider that "there is rivalry between two reconstructions if: (a) they emphasize different elements while aiming to recuperate the same historical episode, (b) they seem to not be mutually compatible and (c) it is expected to choose no more than one of them as the correct one for describing such historical episode" (Martínez-Ordaz & Estrada-González, 2018: footnote 3).

[3] The reader might infer from this that philosophical rivalry (in the sense that is defined in the previous footnote) is enough to prove historically inaccuracy of at least one of the parties of the philosophical divergence. However, while this could be, in some cases, a reliable indicator of historically inaccuracy, rivalry is not a necessary condition for the identification of this type of defect.

3.2.2 The Dilemma of Case Studies

Case studies like the one presented in the previous section have motivated the intuition that the union between history and philosophy of science might not be as fruitful as it was once thought to be. One of the main arguments to support this view is the Dilemma of Case Studies (henceforth DCS). It was first introduced by J. Pitt (2001) and it goes as follows:

1. On the one hand, if philosophers have a case that is clearly not-philosophically biased, then it is also not philosophically relevant, because it is unclear what philosophical lessons to learn or how to generalize from it.
2. On the other hand, if philosophers have a case that is philosophically relevant, it is likely that it is also philosophically biased, because it may have been selected or tampered with to fit the point.[4]

The threat that the DCS poses against the philosophical activity is the following: across time, philosophers have produced a large number of historically inaccurate reconstructions that seem to have been created to expressively support their particular philosophical theses, call these *philosophically biased historical reconstructions*. This has suggested that philosophers cannot legitimately produce nor support general philosophical claims guided by the history of science; and thus, it is not clear that, at least, the more general philosophical insights can tell us anything significant about the scientific activity.

Because of the severity of DCS, to answer to the dilemma has become an issue of high importance for philosophers of science. So far, three main types of responses have been provided:

> **Skepticism:** The combination of the philosophy and history of science is mostly historically and philosophically uninformative (Cf. Pitt, 2001). Therefore, the best way to avoid DCS is to stop pursuing an ideal integration between the two disciplines.
>
> **Deflationism:** The relation between both disciplines could still be beneficial —at least for the philosophical party— if philosophers start holding more modest expectations about the philosophical use of historical evidence (Cf. Burian, 2001; Kinzel, 2015). Instead of looking for support such as

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[4] I want to thank Katherina Kinzel for asking me to point this and for helping me to give a better phrasing of my ideas on this point.

guidance, confirmation and, especially, adjudication,[5] we should be looking for more modest types of evidential support, for instance, recalcitrance and novelty (See Kinzel, 2015).[6] Such evidential roles that historical data can play are still informative enough for allowing for limited generalizations about the local standards of scientific inquiry (See Burian, 2001: 399, 400).

Optimism : Historiography of science is in general methodologically theory-laden and this is not problematic in itself (Cf. Currie & Walsh, 2019). As a matter of fact, the use of such theory-ladenness is what makes possible to provide historical explanations of specific and fine-grained aspects of the scientific practice. Methodological theory-ladenness, under the shape of frameworks,[7] is necessary for any historically informed research.

While the Skeptic and the Deflationist standpoints are prescriptive about the relation that should hold between history and philosophy of science, the Optimists' approach has a more explanatory nature, it explicates why philosophers and historians could not have worked framework-freely and still succeeded at portraying historical events.

Unfortunately, the Optimist approach is still on agreement with the views of Skeptics and Deflationists on the fact that there will be reconstructions that

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[5] **Guidance:** The analysis of various historical case studies should guide the philosopher to identify or modify her philosophical stands (See Pitt, 2001).

Confirmation: Historical case studies could confirm a philosophical view "in the sense of raising its credibility and probability, (this) is usually considered the central evidential function of case studies in the philosophy of science." (Kinzel, 2015: 53).

Adjudication: Historical case studies could be used "to confirm one philosophical doctrine while falsifying a rival position" (Idem).

[6] "**Novelty.** One function of historical case studies is that they provide us with new, previously unknown and perhaps surprising information. New information about the precise historical dynamics of an episode of scientific change, new insights into the structure of a scientific debate, new knowledge about the reasons and causes that motivated a certain scientific decision, and so on. Providing us with new knowledge is perhaps the weakest sense in which case studies can be evidential.

Recalcitrance. A somewhat stronger claim is that engaging in case study research can force us to revise our beliefs. The hermeneutical process of historical reconstruction described by Schickore is such that initial assumptions are revised and modified in the process of historical reconstruction (Schickore, 2011, p. 472)." (Kinzel 2015, 53).

[7] "Frameworks are ways of dividing up and unifying various historical episodes—they are recipes for shifting from chronologies to histories. They have, then, a functional role in historical inquiry: back grounding and foregrounding. Different frameworks foreground and background different aspects of a historical episode" (Currie & Walsh, 2019: 7).

will not be acceptable under any possible conditions, namely the resulting reconstructions of selecting "information from among the set of relevant information in an arbitrary or random way" (Currie and Walsh, 2019: 11) or in an extremely philosophically biased manner.[8]

This still leaves philosophers of science with an open question: *can philosophers of science benefit (in a significant way) from philosophically biased historical reconstructions*? In the following sections, I contend that this particular type of reconstructions can still help philosophers to satisfy a vital epistemic goal, namely, to facilitate philosophical understanding of philosophical theses by satisfactorily exemplifying them.

3.3 Understanding and Selectivity

In the previous section, I claimed that it was of vital importance to explain why philosophers have employed philosophically biased historical reconstructions for a long time without damaging the discipline to death. To effectively do so, it seems to me that first, it is necessary to say something about the processes that are needed for generating such type of reconstructions, in particular, *selection* and *exemplification*.

How are selection and exemplification important for this discussion? Allegedly, philosophically biased historical reconstructions are the result of philosophers first choosing a philosophical thesis, later, identifying the most important components of such a thesis, and finally, manipulating the historical evidence in such a way that it exemplifies neatly the previously chosen thesis. In what follows, I assume that philosophically biased historical reconstructions are nothing but intended

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[8] Explicitly they claim:

> Our discussion demonstrates a way of navigating this apparent impasse. It does not follow from framework ecumenism that we are allowed to do violence to the past—just as *we are not licensed to say whatever we wish philosophically*. Different frameworks, geared toward different purposes, license differing distortions, emphases, and focuses (e.g., Walsh and Currie 2015a). Making these explicit, and understanding their different applications, would switch Sorrell's impasse into a potentially productive interchange between these differing frameworks. (Currie & Walsh, 2019: 17, my emphasis)

exemplars of specific philosophical theses, and explore the possibility of exemplars to be vehicles of the epistemic good 'understanding'.

3.3.1 Understanding and exemplifying

In recent years, understanding has been meticulously analyzed and characterized as a serious philosophical problem. Many things have been said about the type of intellectual achievement that it constitutes, and many different ways to attain and assess scientific understanding have been proposed (See Ammon, 2016; Baumberger & Brun, 2016; Elgin, 1996, 2004, 2009; Wilkenfeld, 2016). In particular, it has been said that the value of understanding seems to surpass that of knowledge, concretely, that

> [A]chieving understanding seems an additional step forward, and we would not take this step if it did not have some additional value. Furthermore, knowledge may easily be acquired through the testimony of experts; understanding, by contrast, seems more demanding and requires that an epistemic agent herself puts together several pieces of information, grasps connections, can reason about causes, and this too suggests an added value. (Baumberger, Beisbart and Brun, 2017: 3)

While there is a recent agreement about the philosophical importance of studying the phenomenon of understanding, there is no unanimous view on how to characterize it. For instance, some philosophers have claimed that there are different types of understanding: objectual, propositional, and interrogative (Cf. Baumberger, 2011; Carter & Gordon, 2014), and explanatory (Cf. Baumberger, Beisbart & Brun, 2017). Depending on the particularities of the type of understanding that is being studied, different philosophical approaches have been developed. In what follows, I focus exclusively on the approach developed by Elgin (2009, 2017) and on the importance of exemplification for that approach.

First, "an understanding, on this conception, is an epistemic commitment to a comprehensive, systematically connected body of information that is grounded in fact, is duly responsive to evidence, and enables non-trivial inference, argument and perhaps action pertaining to the phenomena the information is about" (Elgin, 2017: 82). In addition, for Elgin, exemplification fulfills (at least) two important roles when talking about understanding. The first one is enabling the generation and the strengthening of a variety of epistemically valuable connections across different domains (Idem 78) and thus, enhancing the interpreter's understanding of specific phenomena. The second one consists in indicating that understanding has been achieved: the capability of providing an example "displays an understanding of the subject. It is not just an instance, it is a telling instance" (Elgin 2017, 77). But why, from an Elgin-type of view, exemplifying is strongly linked to understanding? In order to answer this question, it is important to explain what type of activity is to provide an example.

Given that exemplification is a selective activity:

> When an item serves as a sample or example, it exemplifies: it functions as a symbol that makes reference to some of the properties, patterns, or relations it instantiates (Goodman 1968, Elgin 1996). Let us call anything that exemplifies an *exemplar*, and all of an item's properties, as well as all of the patterns and relations it figures in its features. (...) A property then is just that which members of an extension share. Patterns and relations receive analogously tolerant treatment. Thus exemplified features may be dynamic or static, monadic or relational, and may be at any level of generality or abstraction. (...) Because exemplification requires instantiation, only something that actually possesses a property can exemplify that property (Elgin, 2017: 76).

Thus, exemplification is selective in the sense that it requires the identification of particular features of the object/phenomenon that is being studied and to identify similar features in another, apparently, very different object. It is important to highlight that exemplars often can simultaneously exemplify multiple features. Nevertheless, they cannot exemplify all the features of a particular studied object. From an Elgin-point of view, exemplification ¯for purposes related to understanding¯ consists in showing that "a single item can, in the right context, exemplify any and many of its features, enabling the interpreter to forge a variety of epistemically valuable connections across a variety of domains" (ibid, 78).

When trying to understand a phenomenon X through exemplification it is necessary to select a particular group of features of X that one believes are highly relevant for explaining X (and while doing so, one will be explicitly dismissing some features as not relevant or idle for the understanding of X). Thus, when exemplifying, a common and basic requirement is to remove distractors (or idle features). However, "before we can remove the impurities or other irrelevant factors, we need to engage in some analysis: we need to conceptualize the item in question as made from components ¯ those we seek to exemplify, and those we do well to set aside. The analysis is often straightforward. Our prior understanding of the domain frequently enables us to identify the relevant components" (ibid, 81). In the end, one must be able to identify the same features in the chosen exemplar and argue in favor of the connection between X and the exemplar.

Finally, an Elgin-type of approach to understanding will suggest that

> Once we recognize that (...) exemplars marginalize features that are referentially insignificant, we can exploit this capacity through factor analysis. We can construe the phenomenon of interest as factorable into components, distinguish between relevant and irrelevant ones, and then sideline the irrelevant ones. (...) Maybe we need to introduce correction factors to accommodate the simplifying assumptions we made in our exemplars; maybe not. But if we recognize that the representation serves to illuminate the phenomena by exemplifying features it shares with them, and that it makes no commitment to the realism of unexemplified features, we can see how such exemplars embody, advance and convey an understanding of the world (ibid: 91)

The process that is described by Elgin in the above quotation seems to reveal another important connection between exemplification and understanding. Sometimes when providing an exemplar of a particular phenomenon X, we discover that what we considered to be the salient features of X are not enough (or not adequate) for recognizing the exemplified phenomenon in the 'actual' world and that we need to introduce, accommodate or dismiss particular objects or relations that we initially had chosen as components of our exemplar of X. Call this *afterlighting*.

As the reader may have anticipated, afterlighting (as an evaluative process) plays a significant epistemic role for our understanding of a particular phenomenon, it promotes a kind of belief revision and with it, it helps to refine the degrees of understanding that epistemic agents can achieve with respect of particular phenomena. Afterlighting is a process that requires epistemic agents to examine exemplars of a specific object of study, one that sometimes has as output the discovery of new features of the studied phenomenon. Confronting alleged exemplars with the object of study, and facing failure might shed light also on which were the causes of such failure. Finally, I consider afterlighting to be the most important epistemic outcome of exemplification because, in the best scenarios, it helps to narrow the description and to construct the explanation of the object of study –the assembly of explanation not being a direct output of the other results of exemplification.

In sum, exemplification helps us to see how the basic components of a particular phenomenon are combined and put together, and more important it highlights some particularities of the phenomenon in such a way that one is able to provide novel explanations of it. As I understand Elgin's proposal, exemplification can promote understanding through, at least, three different processes:

> **Recognition:** Identifying salient features of a particular object of study and generating an exemplar of such object guided only by the features previously selected.
>
> **Provision:** Presenting an exemplar of a particular object and explaining how the salient features of such an object are present in the provided exemplar.
>
> **Afterlightining:** Evaluating the soundness of the exemplar provided as well as the relevance of the elements previously identified as salient.

In the following section, I relate this approach to understanding the epistemic outputs of using historical reconstructions in philosophical activity.

3.4 Historical and Philosophical Exemplification

A response to the Dilemma of Case Studies consists of endorsing more modest evidential-support aspirations (Kinzel, 2015). However, even the ones who subscribe to this modest view have emphasized that the more historically accurate reconstructions are, the more they will be of use for philosophical purposes. This leaves us with an important question: *Can philosophers of science benefit (in a significant way) from philosophically biased and historically inaccurate historical reconstructions*?

In this section, I respond positively to such question and I explain how, despite their historiographical faultiness, philosophically biased historical reconstructions can provide us with understanding. In order to do so, I appeal to the literature on scientific understanding through exemplification, and argue that philosophically biased historical reconstructions can be seen as exemplars of philosophical theses, and thus vehicles of understanding.

3.4.1 Philosophically Biased Historical Reconstructions as Toy Examples

Introducing Elgin's proposal on scientific understanding had the intention of elucidating, though her approach, a way in which philosophers of science could benefit from philosophically biased and historical reconstructions. In order to achieve that purpose, this sub-section is devoted to characterizing philosophically biased historical reconstructions as exemplars of philosophical theses.

Philosophical theses about science, for simplicity, could be understood as general explanations about the underlying mechanisms of scientific activity. The generality of such explanations is gradual, while the main quantifier of some philosophical theses could be universal, the quantifiers of some others philosophical theses could be existential, and this does not pose any trouble against the characterization that I propose here.

Historical Reconstructions of particular scientific episodes are historiographical explanations of how to put certain historical data together, they assume a particular methodology that allows historians to distinguish between idle and relevant information when telling the history of such episode.

Philosophically Biased Historical Reconstructions (henceforth PBHRs) are historical reconstructions in which some parts of the 'actual' story have been ignored or dismissed in favor of the philosophical assumptions that have chosen (Cf. Martínez-Ordaz & Estrada-González, 2018). Thus, the elements present in the reconstruction are not relevant but they are pertinent. An important remark is that, for a reconstruction to be philosophically biased does not imply (nor forbid) that such reconstruction could be misleading or defective; it only says of the reconstruction that is philosophically theory-laden. Finally, not all historically inaccurate reconstructions are philosophically biased, some of them just ignore historically relevant elements and fail at illustrating any philosophical thesis (for a case of historically inaccurate reconstructions that do not portrait any philosophical thesis, see Martínez-Ordaz & Estrada-González, 2018: 276-278).

From the above, an important question arises: Are PBHRs *mere 'toy examples'*? The response to this question depends on what we label as 'toy examples'. Consider the following (non-exhaustive) list of possible types of toy examples:

> **Extra-simplistic toy example:** Is an item that portraits the instantiation of a general sentence. It only concerns the substitution of free variables of the general sentence by singular terms. For instance, given the proposition "All As are Bs", an extra simplistic toy example of such proposition can be constructed by providing a model in which there are two sets of theories A, B such that $A \subseteq B$, for instance, one substitution would be *all gravitational theories are physical theories.*

While this type of substitutions is often used in (formal) philosophy, it is not the kind most commonly used by philosophers of science when generating PBHRs. So, let us consider another type of toy examples.

Minimally-contextual toy example: Is an exemplar for which a general sentence is satisfied under certain conditions and such conditions are minimally expressed as part of the exemplar itself. For instance, given the proposition "A is preferable to B in context C", a minimally-contextual toy example of such proposition can be constructed by providing a model in which both 'As' and 'Bs' are linked by a preference-type relation and the conditions that warrant such substitution are expressed, for instance, *during the 17th century, mathematicians preferred internal consistency of mathematical theories over procedural simplicity unless they were using the early calculus.*

Even if these toy examples are more frequent in the literature of the philosophy of science, PBHRs are not exclusively of this type. So, let us consider a third kind.

Distractors-free toy example: Consists of an exemplar for which a general sentence is satisfied in a specific context. For this type, such context is extensively expressed but any element of the context that could conflict with the general sentence is removed.

This particular type of toy examples seems to be closer to the standard PBHRs that we find in the literature of the philosophy of science. PBHRs are often presented as large sets of historical information that have been selected to fulfill two main tasks: to illustrate a philosophical thesis and to leave aside any historical bit of information that could *overshadow*[9] the satisfaction of the thesis; and this coincides with the characterization of distractors-free toy examples. With this in mind, one can say that PBHRs are toy examples of, at least, the two latter types.

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[9] This could be understood in at least two senses:

- On the one hand, 'overshadowing' could mean *to distract from the argument*; for example, in the way in which 'interesting' but irrelevant information about the historical case might distract. This, as argued in [Currie & Walsh, 2019], might be a natural result of the activity of choosing a particular methodological framework to explain the episode. And, in this sense, there would be nothing methodologically abnormal taking place in such a practice.
- On the other hand, this could mean *to discredit the argument or to undermine the case for the thesis*; in that case, the intuition would be that this constitutes an important methodological defect, as neither the historian nor the historically minded philosopher is allowed to do violence to the past (Cf. Currie & Walsh, 2019).

In this section, I explain why even if historically inaccurate reconstructions were to be of the later type, they would still be epistemically useful for the philosophical endeavor.

Finally, this view on historical exemplars is in much agreement with other philosophical perspectives from the history and philosophy of science. For instance, according to Hasok Chang:

> [T]he episode is a concrete instantiation of the general concepts (the characters, the setting, the type of events to be expected, etc.), and each episode also contributes to the articulation of the general concepts. To be sure, this analogy is very imperfect, but it does express something relevant about the relation between concrete historical episodes and abstract philosophical conceptions. (Chang, 2011: 111)

Chang's view goes in a similar direction that what I have proposed here: historical episodes exemplify and develop the abstract concepts of our philosophical theses. However, neither Chang nor other philosophers have explained how this can be accomplished. In light of this, the reflections on scientific understanding and historical episodes seem very helpful in this context.

3.4.2 *Good* Philosophically Biased Historical Reconstructions and Reinforcement

As I have discussed in Sec. 3.2, the Dilemma of Case Studies puts philosophers in a very difficult position: if philosophers have produced themselves the historical evidence for expressively supporting their philosophical theses, then it would not be clear how history of science has ever informed philosophy of science in any significant way (Pitt 2001). However, I believe that when discussing the DCS, something important is been overlooked: some philosophically biased historical reconstructions could be epistemically valuable if they promote (either philosophical or scientific) understanding.

Taking into account the nature of PBHRs, it is not difficult to consider that this particular type of reconstructions aims at being exemplars of specific philosophical theses. Nonetheless, as not all toy examples are epistemically useful, and as some exemplars are better than others (Elgin, 2017), I believe not all PBHRs can promote understanding to the same degree. In what follows, I focus only on the PBHRs that instantiate satisfactorily all the relevant elements of specific philosophical theses, and that, because of that, work as exemplars of them. Call these reconstructions *Good Philosophically Biased Historical Reconstructions* (henceforth Good-PBHRs).

Good-PBHRs can be only *good* with respect to specific philosophical theses. Good-PBHR X is a PBHR that is good in virtue of both the way in which it instantiates the salient elements of a philosophical thesis X and the way in which it dismisses the relevant distractors (the ones that could overshadow the fulfillment of X). In that sense, Good-PBHRs work as well-behaved exemplars of philosophical theses. And if Elgin is along the right lines, exemplars actually play a role in achieving understanding, and thus, Good-PBHRs can enhance our understanding of, at least, our own philosophical theses. But again, the intuition about how not all exemplars are equally successful demands a way to evaluate and measure the connection between philosophical theses and Good-PBHRs. In order to provide a way for doing so, let me appeal to a peculiar type of evidential support that was first introduced in [Martínez-Ordaz & Estrada-González, 2018], namely *Historiographical Reinforcement.*

Historiographical Reinforcement consists of an evidential-type of the relation between historical evidence and philosophical theses. In order to evaluate historiographical reinforcement, it is necessary to assume that a given historical reconstruction could be philosophically virtuous with respect to a particular philosophical thesis X if it reinforces X in any of the following grades:

> **Strong Reinforcement :** This level of reinforcement is achieved when, given a philosophical thesis (T) and a specific, relevant historical reconstruction (H'), H' provides a rationale for (a significant part of) T.
>
> **Weak Reinforcement:** This level of reinforcement is achieved if, given a philosophical thesis (T) a specific relevant historical reconstruction (H'), H' supports the basic assumptions of T, contributes to a better understanding of T, illustrates mechanisms relevant for the understanding of T, or clarifies some of the concepts of the theory and their applications. (Martínez-Ordaz & Estrada-González, 2018: 267)

In addition, as not all intended exemplars are good exemplars, for the case of historiographical reinforcement a limit negative case is considered: Given a historical reconstruction, it fails at reinforcing a specific philosophical thesis if it satisfies the following criterion,

> **No Reinforcement:** The absolute lack of reinforcement occurs when, given a philosophical thesis (T) and a specific, relevant historical reconstruction (H'), H' does not instantiate any elements of T, nor does it contribute to a better understanding of the philosophical thesis in question (idem).[10]

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[10] Related notions of reinforcement could be found in Larry Laudan (1977) and Mohamed Elsamahi (2005).

Historiographical reinforcement behaves differently than other evidential relations. On the one hand, the higher limit case, strong reinforcement, seems to warrant scientific understanding: there is a philosophical thesis that explains part of the scientific realm and we have found a good exemplar of such explanatory success. So far, it seems that the underlying mechanisms of such understanding are of the type of Recognition and Provision. On the other hand, weak reinforcement provides an alternative route for philosophical understanding: one intended to introduce an exemplar of a philosophical thesis, but the data was biased and thus, the possibility of providing evidential support has been weakened. Nonetheless, through a mechanism of the type of Afterlightining, one can still discover something new about one's philosophical commitments.

3.4.3 Against weak reinforcement

Concerning weak reinforcement, the reader might wonder if, in cases where we risk damaging the historical record, *philosophical 'fictional' case studies* (invented purely to play the role of exemplars to philosophical theses) would not do just as well. This question seems to lead us to a peculiar dilemma: On the one hand, if the response is affirmative, philosophical theses could be weakly reinforced by using solely false information –which seems extremely counterintuitive. On the other hand, if there is a demand for historical accuracy, it is not so clear how the reinforcement-approach to reconstructions does not collapse in the traditional standpoint –the same view that gave rise to the DCS. So far, while I do not consider the use of full-blooded fictions to be equally fruitful than the use of historically informed reconstructions (even if not fully accurate), I believe that even these reconstructions can yield *some* philosophical understanding. Let me press further on this point.

Regarding the first horn of the dilemma and the counter intuitiveness of achieving (philosophical) understanding via falsities. First, it is important to notice that *philosophical fictional case studies*, if satisfactorily linked to specific philosophical theses, they would behave as, at least, extra-simplistic toy examples. In addition, if the connection between X and the exemplar of X is exemplificatively-adequate, namely, that the exemplar exhibits the selected properties of X, as well as the highlighted patterns and relations it figures in its features (Elgin, 2017: 76), an exemplification of X could yield to the understanding of X. That said, if the fiction adequately exemplifies (part of) a specific philosophical thesis, it could yield to the understanding of such a thesis.

However, the opponent might still reply that, even if the connection between

X and its exemplar is what looks more distinctive of this type of approach, it is not clear that a sort of factic condition (a requirement of the exemplar to be 'real') should not be also demanded for gaining understanding via exemplification –especially considering that the satisfaction of this sort of condition is often assumed when endorsing Elgin's point of view. To respond to this objection, one can argue that, even if the exemplar is fictional, and in a sense, false, this false character of the exemplar does not necessarily block the possibility of achieving understanding via philosophical fictional case studies.

In the corresponding literature, it has been argued that *False Theories can still Yield Genuine Understanding* (De Regt & Gijsbers, 2017). This is, for a given set of propositions, even if the veridicality condition (also called 'factic condition') is not satisfied, this would not necessarily prevent philosophers from gaining an understanding of such a set of propositions. According to De Regt & Gijsbers (2017), what is needed for understanding is only the satisfaction of an 'effectiveness condition' (where, for this case, 'effectiveness' could be understood as the tendency to produce useful philosophical outcomes of certain kinds). So, even if the philosophical fictional case studies were clearly false, if they could still produce relevant philosophical products, they would still be able to lead us to achieve some understanding of the theory they aim at illustrating.

The limitations that could come with gaining understanding via fictions might include that the type of understanding that is gained is, only, *modal understanding*. "One has some modal understanding of some phenomena if and only if one knows how to navigate some of the possibility space associated with the phenomena" (Le Bihan, 2017: 112). In the case of fictions about science, to achieve modal understanding of the philosophical theses that a particular fiction is designed to exemplify, would be to determine the set of possible worlds that correspond to the generic structural features assumed by the philosophical view that such a thesis substantiate (this is, if the thesis were to be true, which type of scientific practices would it describe, how would these practices relate to one another, among others).

All this considered, as a response to the first horn of the dilemma, I think that the solely fictional character of this type of case studies does not prevent philosophers from gaining a philosophical understanding of their theses. However, it seems to me that the falsity involved in full-blooded fictions might block the possibility of achieving an understanding of fragments of the history of science.[11]

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[11] One might wonder if extreme inaccuracy (or even falsity) isn't necessarily a problem, and fictional case studies can still lead to understanding, then is there any reason to pay attention

Regarding the second horn of the dilemma –about the closeness between the reinforcement-approach and the traditional standpoint. I consider the answer to be straight forward: the proposal that I have presented in this chapter assumes both the desirability of historical accuracy when providing historical case studies in the philosophy of science, as well as the possibility of historically inaccurate reconstructions to be valuable for the philosophical endeavor. According to the reinforcement-approach, the former corresponds to the pursuit of strong reinforcement of philosophical theses (via historical case studies), and the latter corresponds to the acquisition of weak reinforcement for philosophical theses (in particular, the achievement of philosophical understanding). When historical accuracy is discovered to be implausible to be completely satisfied, the philosopher of science can adopt any of the methodological views sketched in Sec. 3.2.2 and be able to explain why the philosophical study of science can still be carried out successfully even if historical accuracy is not fully attainable –in such a way, the DCS weakens dramatically. In addition, the reinforcement-approach would allow philosophers to explain why *some* historically inaccurate reconstructions (when being philosophically biased) can still have an important epistemic value in philosophical research.

The combination of these responses shows that the allegedly troubled scenario consists of a false dilemma and that the approach that I submitted in this chapter is a midpoint between the possibility of understanding philosophical theses from a false basis and the strong commitment to historical accuracy when evaluating the use of historical evidence in the philosophical practice. So, while the reinforcement approach is compatible with the Traditional view, it is innovatively explanatory of why one can benefit from some traditionally unwelcome type of historical reconstructions.

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to the history at all? I think there is. First of all, I consider philosophy of science to be an investigation about science, and in particular a chase for explanations of scientific phenomena. In light of that, what inaccurate reconstructions provide in the majority of cases is only understanding about our philosophical thesis. But even if one can gain *some* scientific understanding (understanding about science) via the analysis of inaccurate reconstructions, it is still debatable if that will necessarily imply that one gains explanation as well (Cf. Elgin, 2004, 2009, 2017; Baumberger, 2011; Carter & Gordon, 2014; Ammon, 2017; Baumberger & Brun, 2017; Baumberger, Beisbart & Brun, 2017). So in that sense, to have historically accurate reconstructions is still very much desirable as they could be of prominent use for achieving explanation.

3.4.4 Understanding the exemplar

For the particular case of the Newtonian Early Calculus (first introduced in Sec. 2.1), it is easily to recognize that, so far, no agreement has been reached on the fact of how to accurately portrait such a historical episode (regarding solely to the possibility of inconsistency toleration). This is, it is not clear to philosophers if any of the many reconstructions that I presented could reinforce strongly one of the many philosophical theses involved. However, it is not difficult to see is that all of them reinforced weakly their philosophical theses. To only mention one way in which this has been done, let me refer first to the Brown and Priest (2004) reconstruction.

In [Brown & Priest, 2004], the authors aim at providing a formal mechanism that could do both explain and allow for representing the inferential strategies that human reasoners tend to follow when dealing with inconsistent or conflicting information. This mechanism is named 'Paraconsistent Reasoning Strategy *Chunk and Permeate*'. One of the benefits of their proposal is that it allows for change-of-logic maneuvers (scientists could change the logic that underlies their reasoning depending on the inferential tasks that they have to fulfill) and that it is not strongly logic-dependent (different logics could be explanatory of the different inferences that scientists do and this is not problematic).

Brown and Priest's proposal is mostly inspired by the foundations of non-aggregative logics and epistemic fragmentation; nonetheless, as their approach consists of a hybrid formal resource, it is not clear how to go from the formal properties of the strategy to its application to philosophy of science. What the case of Newtonian Early Calculus helps to see is the bridge between the formal properties of the Brown and Priest-strategy and the way to select relevant information about scientific reasoning from the historical record.

This, of course, does not mean that they have proved that the reasoning that Newtonian mathematicians followed was anything but paraconsistent. As a matter of fact, according to Vickers (2013), Brown and Priest's reconstruction was historically flawed as well as more driven by a philosophical trend (the attention to paraconsistent logics applied to science) than historically informed:

> [T]he simple story we are met with so often in the literature, of the early calculus as a set of inconsistent propositions plus a logic, is plain wrong. Brown and Priest (2004) are typical of a subsection of the philosophy of science that assumes the early calculus can be reconstructed by making use of a paraconsistent logic. To motivate the application of a particular paraconsistent logic they dub 'chunk and permeate' (. . .) Clearly this

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> blurs the important distinctions between the algorithms of the calculus, the story one tells oneself whilst making a derivation, and the attempted justifications of the moves made within the algorithms (. . .) However, Brown and Priest are simply following a theme in philosophy of science which is completely entrenched. (Vickers, 2013: 186-90)

In that sense, if Vickers objections are along the right lines, the reconstruction provided by Brown and Priest might be a philosophically biased historical reconstruction (PBHR). Nonetheless, as I had previously argued here, this would not necessarily mean that the reconstruction should be absolutely abandoned. Actually, I think this case illustrates perfectly how one can achieve weak reinforcement to the use of a PBHR. Let me press further on this point.

First, the corresponding literature has shown that, via the analysis of the Newtonian case study presented in [Brown & Priest, 2004], not only authors but readers have grasped a better understanding of what would be to use *Chunk and Permeate* to formalize and explain inconsistency tolerant reasoning in the sciences. The many following applications of the paraconsistent strategy accompanied by reflections on the Newtonian case study support this idea (See for example Sweeney, 2014; Brown & Priest, 2015; Brown, 2016, 2017; Friend & Martínez-Ordaz, 2018).

In addition, in the following years, Sweeney (2014) provided an, allegedly, more historically accurate reconstruction of the same episode, and he kept using Chunk and Permeate to explain the behavior of Newtonian infinitesimals. While doing so, Sweeney improved Brown and Priest's historiographical methodology but not the philosophical thesis behind their proposal. As a matter of fact, the philosophical proposal might have not changed much since it was presented in 2004, the thing that has varied across related chapters is the examples that are provided to illustrate the functioning of *Chunk and Permeate*.

That said, it is possible that the Newtonian historical reconstruction has helped the authors themselves and the readers to understand better Brown & Priest's formal proposal, regardless how much historical understanding of the case they were able to obtain –yet, it also seems that the fact that their reconstruction was not fully fictional, enhanced the more historically informed project later undertaken by Sweeney (2014). I am confident that all this should be phrased in terms of achieving *weak reinforcement*.

3.5 Final remarks

Here I contented that, even if the defenders of the Paraconsistent view have misused the historical record to support their philosophical claims, some of the philosophically biased reconstructions that they have provided in favor of their view might have an extremely high epistemic value, namely, to promote our understanding of their philosophical theses. This, combined with other epistemic outcomes (achieved, for instance, via afterlightining), should convince the supporter of the Traditional view that she could benefit substantially from the study of the historical reconstructions of alleged inconsistency toleration in the sciences.

In Sec. 3.3, I appealed to the literature in scientific understanding according to which understanding is achievable via exemplification, and argue that if a certain type of historically inaccurate reconstructions of scientific episodes could be taken as exemplars of philosophical theses, they could enhance our understanding of, at least, such theses. I proposed that exemplification could promote understanding through, at least, three different processes:

Recognition : identifying salient features of a particular object of study and generating an exemplar of such object guided only by the features previously selected.

Provision : presenting an exemplar of a particular object and explaining how the salient features of such an object are present in the provided exemplar.

Afterlightining: evaluating the soundness of the provided exemplar as well as the relevance of the elements previously identified as salient.

Considering the above, in Sec. 3.4, I argued that historical reconstructions, even if not historically accurate, can play another equally important role: to enhance our understanding of philosophical theses about science via exemplification (of specific theses). This is, they could be taken as exemplars of philosophical theses.

Finally, I presented different ways in which the relation between historical data and philosophical theses could be characterized:

Strong Reinforcement : achieved when a historical reconstruction provides a rationale for (a significant part of) a philosophical thesis.

Weak Reinforcement : achieved when a specific relevant historical reconstruction contributes to a better understanding of a particular philosophical thesis.

> **No Reinforcement :** Achieved when given a philosophical thesis and a specific, relevant historical reconstruction, the latter does not instantiate any elements of the former (and thus, does not behave as an exemplar of the thesis).

All this considered, even if the historical case studies provided by the Paraconsistent view were historically inaccurate they could still play an extremely important epistemic role for the philosophical endeavor, namely, to reinforce philosophical theses about contradiction in science by enhancing the understanding of such theses.

For these reasons, the study of historical episodes that allegedly illustrate inconsistency toleration in the sciences would be philosophically revealing even if, in the long run, one discovers that the reconstructions of such episodes were philosophically biased. This supports the idea that the case studies provided in previous chapters are in themselves valuable, for both the Paraconsistent and the Traditional views, as they promote the philosophical understanding of the mechanisms that could underlie the inconsistency toleration in the sciences, as well as they allow for philosophical discoveries via afterlightening.

In the next two chapters, I provide formal reconstructions of historical cases that illustrate the toleration of contradictions between theory and observation; such reconstructions aim at providing explanations of the different ways in which scientists might have tolerated a specific contradiction. Considering what has been discussed in this chapter, I expect these analyses to, at least, enhance our philosophical understanding of both certain formal tools and philosophical theses associated to the use of such tools.

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4 Reconstructing inconsistent science using *Partial Structures*

4.1 Introduction

The following two chapters of this dissertation deal with possible explanations of the way in which scientists can preserve sensible reasoning when working with inconsistent information. In particular, this chapter provides an application of the *Partial Structures approach* (*PPEE*) to a case of inconsistency toleration in the empirical sciences, namely, the anomaly in the measuring of the solar neutrinos' flux. While doing so, I aim at offering an explanation of how scientists can tolerate contradictions between theory and observation without facing logical triviality.

PPEE constitutes a particular type of semantic approach to scientific theories; and it has proved to be extremely handy when used to explain and model the ubiquitous use of defective (incomplete, partial, conflicting and inconsistent) information in the sciences (see Bueno, 1997; Bueno, French & Ladyman, 2002; da Costa & French, 2003; Bueno & French, 2011; Ladyman & French, 1999, 2002). The main motivation behind this approach has been to provide formal analyses of the way in which, despite the fact that scientific information is often incomplete, scientists can still rationally rely on it, work with it and constantly update their sets of information with new discoveries.

In general terms, according to this view, given a particular domain $\boldsymbol{D}$,"all the relevant information we obtain, up to a certain moment, may in general be formulated through a family of partial n-place relations $(\boldsymbol{R}_i^n)_{i \in I}$, holding between the objects of $\boldsymbol{D}$; these relations are *partial* in the sense that for some n-tuples it might not be defined whether or not they satisfy $\boldsymbol{R}_i^n$)" (Bueno, 1997: 591). Therefore, partial structures are formal resources that play an extremely important methodological role for the philosophy of science, namely, allowing us to see how scientific bodies of knowledge, which are initially incomplete, can grow by incorporating new information in order to be refined and "completed" in the long run.

Here I aim at integrating what has been done in the previous three chapters of this dissertation by using partial structures. The main idea is to show the way in which *PPEE* can allow philosophers of science to see how factual ignorance, avoidance of logical explosion and a complex case of contradictions between theory and observation hang all together. In addition, the result of such analysis would be a *PPEE*-type reconstruction of a specific historical case, which, even if it were philosophically biased in any significant way, would still allow philosophers of science to understand, first, the way in which partial structures can be used to explain cases of inconsistency toleration, and, second, the way in which an inconsistent theory can be kept in use without facing logical triviality.

The outline of the chapter is as follows: First, in Sec. 4.2, I discuss different types of research programs for the study of inconsistency toleration in the sciences and argue that a purely historiographical analyses of cases of inconsistent science cannot suffice to provide philosophical understanding of the phenomenon. In Sec. 4.3, I discuss purely logical programs and, argue that by themselves, they cannot suffice either; so I present the need for integrative research programs on inconsistency toleration. Here, I also argue in favor of the use partial structures for the study of cases of inconsistency toleration in the empirical sciences. In Sec. 4.4, I introduce *PPEE* in more detail, drawing special attention to the way in which partial structures can account for the empirical adequacy of incomplete scientific theories. Sec. 4.5 I explain the anomaly in the measurement of the solar neutrinos' flux in terms of partial structures. In Sec. 4.6, I draw some conclusions on the scope of application of *PPEE* regarding complex cases of contradictions between theory and observation in the empirical sciences.

4.2 Against historical analyses of contradictions in science

In this section I discuss some concerns about the scope of historical analyses of cases of inconsistency toleration. In order to do so, Sec. 4.2.1. is devoted to discuss purely historical approaches to cases of inconsistency toleration, here I also introduce the anomaly in the perihelion of Mercury as a case study of alleged inconsistency toleration. In Sec. 4.2.2, I explain how the supporters of purely historical approaches to inconsistency in science have reconstructed such a case study. Finally, in Sec. 4.2.3, I problematize the results of this type of approaches to the study of contradictions in the sciences.

4.2.1 The (hi)story of Mercury's anomaly

Inconsistency toleration in the sciences consists on identifying specific inferential mechanisms that allow scientists to work with an inconsistent set of information and still preserve sensible reasoning.[1]

Considering the complexity of the phenomenon of inconsistency toleration, philosophers of science have developed three different types of research programs for its study:

- **Historical programs:** this type of programs have a deeply descriptive approach to contradiction in science, "which concerns the question whether inconsistencies commonly appear in science, and whether scientists sometimes accept and reason from inconsistencies" (Šešelja, 2017: 2).
- **Logical programs :** these programs have a more "normative perspective, which concerns the questions whether we can rationally reason from an inconsistent set of premises without ending up in a logical explosion, and if so, how" (idem)
- **Methodological programs:** this type of programs have "a normative perspective, which concerns the role of the standard of consistency in evaluations of scientific theories" (ibid).

While the Paraconsistent view has been largely motivated by the emergence of paraconsistent logics and has focused mostly on logical research programs; large part of the supporters of the Traditional view have assumed that, as the toleration of contradictions in science is only apparent, to explain cases of alleged

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[1] An important remark: Inconsistency toleration does not necessarily involve any of the following: (a) the final solution of the contradiction, nor (b) a real contradiction 'in action'.

One the one hand, when facing a contradiction, scientists could be trying to solve it and fail at doing so, and we can still call this 'inconsistency toleration'. Additionally, they also could be using an inconsistent set of propositions without focusing on the contradiction but, if they are aware of its presence and can still prevent triviality, we can keep calling it 'inconsistency toleration'.

On the other hand, if, in a particular time (t1), a scientist identifies a contradiction in a particular set of propositions, regards it as not dangerous and remains capable of having sensible reasoning; and later, in a time (t2), she discovers that the original set of propositions did not contain a real contradiction (but only apparent), we can still call the processes that she followed for the avoidance of triviality, in t1, 'inconsistency toleration'.

inconsistent science is not a business of logic but only a matter of providing an adequate historical research that can show how inconsistencies never need to be tolerated.

Here, I tackle the latter view and explain how informal historical reconstructions would not be enough for the adjudication of the debate on the possibility of inconsistency toleration in the sciences. In order to do so, I introduce a way to interpret Kevin Davey's reconstruction on the anomaly in the perihelion of Mercury as a case of inconsistency toleration. I argue that his reconstruction of the case should be seen as an invitation for further and more detailed analyses of the different ways in which scientists tolerate contradictions between theory and observation.

The reason why I chose to focus back again on this case study is because, if one were to accept the existence of logical contradictions between theory and observation, the anomaly in Mercury's perihelion precession must be taken as one of the neatest exemplars of such contradictions (see Priest, 2002; Harper, 2007; Gine, 2008; Martínez-Ordaz, 2017). In this respect, if they were any methodological guides on how to approach this case, these guides would be useful also for other, less neat, cases of contradictions between theory and observation. In addition, I go back to Davey's explanation of this case in order to show that, despite Davey's ambition, his reconstruction only instantiates a way in which scientists can tolerate a contradiction but nothing more contentious than that.

That said, the case study goes as follows. Kepler's laws in conjunction with Newtonian mechanics say that planets revolve around the Sun following elliptical and closed paths −once a planet has traveled across such a path, always would go back to the starting point. The close relationship between Kepler's laws and Newtonian mechanics made it possible for astronomers and physicists of the time to have, what seemed to be, a wide catalog of predictions and explanations for the behaviour of celestial bodies. The theory was perceived as extremely successful in the eyes of the relevant scientific community. However, in 1859, Le Verrier discovered that the orbit of Mercury did not behaved as expected. Once the planet had traveled along its orbit, it did not go back to the starting point.

The problem was that, although all the planets have a precession of their perihelion (that is, a shift), the case of Mercury stood out surprisingly. According to Newton's laws, Mercury's orbit's ellipse should precess by 432 arc-seconds per century, but the observational reports indicated that it precessed at a rate of 474 arc-seconds per century. The difference between the predictions and the reports, was not only large but it was impossible for the physicists at the time to explain the cause of the problem. Nonetheless, they kept confidently working with the theory as it gave extremely accurate predictions on the behaviour of the other celestial bodies.

This historical episode has often been seen as illustrative of a remarkably tolerant attitude towards contradictions between theory and observation. The main reason for doing this is that the anomaly in the perihelion of Mercury lasted for several decades,[2] and despite of being aware of the anomaly, physicists trusted enormously a theory that had proved to be inconsistent with the observational reports about Mercury's behaviour.

4.2.2 Re-reading the history about Mercury

This apparent extreme confidence shown by the scientists made some philosophers of science, such as Kevin Davey (2014), skeptical about the alleged toleration of a contradiction in this specific case. For them it seemed quite odd that despite of knowing the problems of their theory, scientists were not showing also weakened commitments towards it; this made some philosophers of science, mostly supporters of the Traditional view, to look with more detail at the historical record seeking for evidence of weakened commitments towards specific segments of the theory (the ones associated with the prediction of Mercury's orbit). As a result of this enterprise, Davey (2014) argued that if philosophers of science, specially the supporters of the Paraconsistent view, had conducted more sophisticated analyses of this case (and similar ones), they would not have been able to characterize this episode as a case of inconsistency toleration.

Davey claimed that the initial discovery of the inconsistency, made by Le Verrier, caused scientists to stop trusting theory as a whole. As a matter of fact, when the relevant scientific community became aware of the problem, they immediately adopted a skeptical attitude regarding the observational consequences of the theory corresponding exclusively to Mercury's behavior. Therefore, according to Davey, there was no inconsistency toleration in this case, "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" (2014: 3018).

Davey's main point is that, whenever a contradiction arises in the sciences, the doxastic commitments that scientists hold towards their relevant theories decrease immediately. This is done as a rational maneuver to avoid some of the

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[2] The problem was not solved until the appearance of Einstein's theory of relativity at the beginning of the 20th century.

traditional and intuitive problems associated to the presence of contradictions (some of these problems were described in Sec. 1.2). According to Davey, such a weakening has the effect of dissolving the contradiction; namely, scientists can still trust s without being committed to (or even suspecting of) $\neg s$. Davey's methodology describes the way in which scientists allegedly isolate (what they consider to be) the problematic element and when doing so, they are able to block the conjunction of the conflicting propositions. In that sense, this type of isolation methodology prevents scientists to trust in the same degree mutually inconsistent propositions. This is, it blocks the possibility of being committed to an explicit contradiction.

Nonetheless, despite the fact that Davey's main claim is that the Paraconsistent view has not provided enough evidence in favor of the actuality of inconsistency toleration, his understanding of the way in which scientists deal with contradictions does not defer much from what I had previously characterized as *inconsistency toleration* (see Sec. 1.2). Which basically consists on the possibility of keeping sensible reasoning when working from inconsistent information, Davey's isolation methodology is only one way to do so, but it is not proof of anything stronger.

As a matter of fact, for this particular case, if Davey's reconstruction is along the right lines, what scientists did was isolated a small group of consequences from a reliable set of information −the set that contained the empirical information about each of the planets of our solar system. However, when weakening their commitments, they did not lose confidence neither in the whole set nor in its sources; for instance, they did not start questioning the quality of the data that they had about Mars or Venus, they only regarded as non-trustworthy the subset of information that concerned Mercury.

According to Davey, scientists faced the following situation: on the one hand, they had a theory that, when informed with empirical data, provided accurate predictions for all celestial bodies except for one, for Mercury it predicted something of the form s. On the other hand, they had a reliable set of empirical information that contained trustworthy data about celestial bodies, except for the case of Mercury, regarding the observable behavior of the planet, the data set contained a report of the form $\neg s$. Unfortunately for the Traditional view, as far as the scientists did not have an explanation of why the predictions that the theory provided about Mercury were so defective, the isolation that they practiced was not guided in any sense by the theory nor by the data set of empirical information. It was only a way to prevent the contradiction from causing more problems; this is, it was a way to preserve sensible reasoning despite the presence of a contradiction.

4.2.3 How to improve

The fact that Davey's reconstruction is actually confusingly compatible with the Paraconsistent view, in addition to the usual problems associated to the philosophical use of historical evidence (see Chap. 3), might suggest that the study of cases of alleged inconsistency toleration cannot be done by only putting together historical data. The study of cases of inconsistency toleration deserves a more rigorous and meticulous type of methodology.

In addition, as a result of the most recent discussions against the possibility of inconsistent science, it has become clear that any attempt to achieve philosophical understanding of the phenomenon of inconsistency toleration should allow to put all the historical, logical and methodological research programs together in a significant way. Therefore, any satisfactorily approach to inconsistency toleration should allow for a way to understand how it is possible to reason from inconsistent information in science without arriving at arbitrary conclusions, but it should also allow for some insights about the status of consistency in science and finally, it should help us to describe and explain actual cases of inconsistency toleration in science (if any).

The only way in which philosophers would have a chance to grasp the core of the phenomenon of inconsistency toleration is to combine historical research programs with methodological and logical ones. While some methodological concerns could have been naturally part of the approaches preferred by the Traditional view, formal analyses of the alleged inconsistencies were still missing in their research projects. Nonetheless, the alarming similarities between Davey's and the Paraconsistent view's reconstruction of the anomaly in the perihelion of Mercury suggest that, even the supporters of the Traditional view should enrich their methodology by incorporating not only methodological but also formal analyses of the particular cases of alleged inconsistency toleration.

Now, while the use of formal tools could allow philosophers to account for the structure, use and applications of inconsistent empirical theories, in order to do so, such tools may, at the very least, possess elements that allow for

(i) linking a theory to a specific empirical domain,

(ii) illustrating the contradiction between the elements of the theory and the observational reports,

(iii) leaving room for the ubiquitous use of incomplete information, and

(iv) highlighting the way in which sensible reasoning was preserved despite the presence of a contradiction.

In addition, formal tools that are expected to be used to model and explain cases of inconsistency toleration must be sophisticated enough to account for the success of theories. This type of tools should be able to save phenomena of scientific practice such as the inclusion of idealizations in scientific reasoning, the selection of observational consequences worth pursuing through the experimental route, as well as the transmission of reliability (from premises that include propositions false, to satisfactory conclusions) and really underlying the use and application of inconsistent and functional empirical theories.

In the following section, I provide more hints on how to choose such tools.

4.3 Formal analyses of contradictions in science

In the previous section I have claimed that the study of inconsistency toleration in the sciences should combine historical and methodological programs with logical ones. And while I argued in favor of embracing formal approaches for the understanding of the tolerance of contradictions in scientific reasoning, I do not consider logic research programs by themselves to be robust enough for successfully accounting for the phenomenon of inconsistency toleration. With this in mind, in Sec. 4.3.1, I discuss some of the problems that purely logical programs face when explaining cases of alleged inconsistency toleration. Sec. 4.3.2 is devoted to present a peculiar sort of formal resources for the modeling of inconsistency toleration and in Sec. 4.3.3., I explain which are the features that formal tools should possess in order to promote the integration of methodological, historical and logical programs. Finally, in Sec. 4.3.4. I argue that a Partial Structures approach could be a good candidate for explaining and modeling inconsistency toleration in the empirical sciences.

4.3.1 Logical programs and their problems

Logic understood from an epistemological point of view is mainly focused on increasing our understanding of human reasoning through the analyses of certain inferential patterns that agents could actually employ (Corcoran, 1994). Such a view has provoked critical discussions on formal and philosophical level. On the one hand, some philosophers have been strongly skeptical regarding the normative role of formal logic in human reasoning (Margáin, 1976), and some others have accepted that it is not clear if logic could describe and norm always human inferential processes, yet it could still be explicative of some common inferences (Harman, 1984). On the other hand, some other logicians have sustained that certain (type of) logics could ground a theory of human rationality (Carnielli & Coniglio, 2016: Chap.1). The latter approach consists in identifying a paradigmatic element of human rationality and analyzing the inferential patterns that are involved in them (which logical principles play a role in that particular type of reasoning, which are clearly avoided, and so on), the next step is to select a logic or a group of logics that can describe and explain such inferences. Ideally, the result of such analysis will provide us with, at least, a deeper understanding of human rationality (Carnielli & Coniglio, 2016: Chap.1).

Following a similar intuition, some schools of paraconsistent logics have persistently aimed at providing logics that are supposed to describe and norm –actual-human reasoning in inconsistent contexts. Let's call this type of program the *Paraconsistent Logics Approach* (PLAs). The PLAs projects are mostly focused on the analysis of different types of logical consequence that could describe sensible reasoning in inconsistent contexts (regardless if they are associated to scientific practices). As part of this approach one could recognize certain applications of Logic of Paradox (Cf. Priest, 1984, 2006), some branches of the Adaptive Logics project (Cf. Batens, 2002, 2017; Meheus, 2002), some branches of the Logics of Formal Inconsistency (LFIs) project (Cf. Carnielli & Coniglio, 2016: Chap. 8, 9), among others.

While this enterprise has produced many interesting formal results, it also has been accused of overlooking the actual phenomenon of handling inconsistency in scientific reasoning. This, partially because the type of analysis that this view holds requires strong commitments with very specific (some of them even peculiar) logical consequences, which might not be part of human reasoning at all. In addition, so far these projects have not been able to agree in their explanations of which are the inferences that scientist could follow in order to avoid explosion when reasoning with inconsistent information; even more alarming, for the same case studies, different and rival explanations have been provided by the supporters of PLAs projects. And so far it has seem that either there is no core of shared elements that could explain how certain scientists have dealt with certain contradictions at a particular moment, or there are way too many alternative explanations that is no clear that any of those is actually an explanation for the particular cases.

In the majority of instances, the PLA-explanations of cases of inconsistency toleration are reinforced by specific applications of particular paraconsistent logics. And so, it has been argued that, PLAs draw the attention away from the actual premises and arguments offered by scientists by privileging discussions on which particular notion of logical consequence is, for instance, more virtuous (Brown & Priest, 2015). For example, in Meheus (2002) the case of Clausius' derivation of Carnot's theorem is presented as a case of inconsistent scientific reasoning, and it is explained by stating that the logic that satisfactorily models this type of reasoning is an Adaptive Logic, in particular, the adaptive logic ANA. In a similar way, Priest (1987, 2006) analyzes the –physical- phenomenon of motion as a contradictory one and provides an understanding of it that suits the basic structures of some dialetheist logics.

The fact that PLA-explanations tend to privilege very specific logical consequences which further applications to scientific reasoning are not clear yet, makes less surprising that PLA-explanations face some harsh critiques from the history and philosophy of science. For instance, it has been constantly pointed out that the adoption of solely PLAs to historical episodes tends to threaten the understanding of the actual phenomenon (as it was claimed for the case of the Priestian theory of motion, by Boccardi and Macías-Bustos (2017), and by Vickers (2013: 186-90) for some other interesting cases of alleged inconsistency toleration).

4.3.2 The other logical programs

In face of this kind of allegations, a more general type of formal approach to inconsistency toleration has been suggested: general formal tools that do "not focus on identifying or proposing alternative logics that might lurk in the background of scientific reasoning. Instead it focuses on a more directly observable feature of reasoning, viz., how and where different premises are invoked in the course of arguments" (Brown & Priest, 2015: 299). The result is a type of analysis of inconsistency in (scientific) reasoning through the use of some reasoning strategies; let's call this the *Paraconsistent Alternative Approach* (PAA).

Considering that the PAA view makes no assumptions about which is the underlying logic of scientific reasoning, it is considered to be 'minimal' (Brown, 2017) when used to model specific cases from the history of sciences.

The PAA consists of a set of strategies or general procedures that are explanatory of the way in which it is possible to handle contradictions in order to avoid explosion. Such strategies are paraconsistent in the sense that they allow scientists to avoid logical explosion in an optimal way –recognizing that what is ‘optimal’ would depend on the own constrains of each of the cases that are being studied. These strategies suggests ways in which information could be broken apart and transmitted while following some inferential patterns. Even though these strategies often substantiate the general dynamics of certain logics; they are, most of the time, also logic-independent –this is, they are compatible with many and diverse logical consequence relations.

Paraconsistent strategies do not necessarily focus on the structure of the scientific inconsistent theory (or model) itself, but they pay special attention to both, the information that epistemic agents often employ to identify the contradiction and the ways in which agents use such information in scientific problem solving and still avoid triviality. This minimal approach to inconsistent scientific reasoning was first sketched through the Rescher-Manor mechanisms (Rescher & Manor, 1970) and is nowadays incarnated in the strategies that substantiate the dynamics of the so-called Adaptive Logics, reliability strategy and minimal abnormality strategy, among others (Cf. Verdeé, 2009; Straßer, 2014; Batens, 2017), and in *Chunk and Permeate* (Cf. Brown, 2016, 2017; Brown & Priest, 2004, 2015; Friend 2013; Benham et al., 2014; Priest, 2014).

In a similar way, another instance of the PAA could be identified in some of the works of the Brazilian school of paraconsistency; in particular, the ones concerning the Partial Structures Approach (*PPEE*) and the corresponding views on partial truth. As I had mentioned in the introduction of this chapter, the *PPEE* consists on providing a methodology for the study of the ways in which scientific theories contain defective (partial, conflicting, inconsistent) information and still provide accurate predictions and explanations about target phenomena. The success of this approach is not motivated by any fixed commitments towards to a specific consequence relation, but it is actually caused by the fact that the *PPEE*’s underlying structure is in a sense fragmentative with respect to, at least, conflicting sets of information. According to the *PPEE*, when working with a scientific theory, scientists are fully aware of the fact that not all the information that they have would hang together, some chunks of information would take as well known facts, others as well known falsities and some others are taken as deeply ignored elements. Yet nothing of this depends on any specific type of logical consequence relation, but it depends mostly on the ways in which we have seen scientists fill and update their theories.

In the following section, I briefly sketch why *PPEE* would constitute a good candidate for explaining specific cases of scientific inconsistent reasoning.

4.3.3 The challenge for ***PPEE***

Summarizing: any approach to inconsistent science that aims at providing reliable explanations about the possibility of the tolerating contradictions in the sciences should combine historical, methodological and logical programs. When doing such integration, it is important to keep in mind to not shift the object of study from scientific reasoning to philosophical and formal notions of logical consequence; in the aim of doing so, PAAs pose a great way to pursue formal analyses of inconsistency toleration. On the one hand, some PAAs (such as *Chunk and Permeate*)[3] have focused on the inferential general strategies that, in solving problem-type of scenarios, human agents tend follow when working with inconsistent sets of information; this type of PAAs have, in a sense, neglected the ways in which that information is ordered to shape scientific theories. On the other hand, the Partial Structures approach (*PPEE*) has provided formal resources to explain how sets of defective (partial, conflicting, inconsistent) information are arranged to form scientific theories.

So far, I had argued in favor of weakening the logical commitments of logic programs to inconsistent science, and submitted that *PPEE* can do so; however, I have not yet explored if, at least in general terms, the *PPEE* can allow for the integration of the other two type of programs, namely, methodological and historical. Here, I discuss the requirements for integrating historical, methodological and logical programs using *PPEE* −for the case of empirical scientific theories.

The *PPEE* constitutes a type of structuralist approach to scientific theories that, seemingly, has succeeded in accounting for the use of defective information in the sciences. What I consider in the following paragraphs is if, when modeling specific cases of alleged inconsistency toleration in the sciences, *PPEE* can incorporate all the relevant historical and methodological features (present in actual scientific practice) in order to provide accurate formal reconstructions of the scientific use of defective information.

For *PPEE* to be successful at this task, some of the corresponding desiderata that it should satisfy could be phrased as:

- If a specific set of information forms an empirical theory, the tools through which such a theory is scrutinized must include elements sophisticated enough for allowing us to recognize at least propositions that refer to merely formal or abstract entities and propositions about empirical objects.
- Since the propositions that integrate our empirical theories can have different

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[3] The next chapter focuses on discussing this type of PPAs.

shapes −for instance, of general laws, auxiliary hypotheses, initial conditions, empirical assumptions, idealizations by omission, idealizations by abstraction, among others (Cf. Kuhn, 1970). Therefore, it is necessary that the formal tools used to address inconsistent empirical theories are sufficiently refined to allow to distinguish between types of propositions.

- In scientific practice, false elements are sometimes added to theories with the intention of obtaining accurate predictions. Then, any formal tool that seeks to account for the operation of inconsistent and functional empirical theories must allow the formal reconstructions of these theories to be linked to the procedures and results of scientific practice.
- From analyzing the reconstruction of the anomaly in the perihelion of Mercury provided by Davey (2014), we have learned that the toleration of contradictions is also associated to a diversity of doxastic commitments. In that sense, any formal tool that aims to shed light on the ways in which contradictions are tolerated in the sciences should allow for distinguishing between the different types of doxastic commitments that scientists might hold towards, at least, the conflicting propositions.

Therefore, in order to provide a formal analysis of inconsistent empirical theories, it is necessary for formal tools to, at least, being able to meet the desiderata presented above as well as to do it in such a way that is neutral to the particular logic. In the following section I aim at expressing why I believe *PPEE* can do all this.

4.4 Partial Structures

In what follows, I introduce the so-called *partial structures approach* to scientific theories ($PPEE$). This particular view on scientific theories is that it has proved to be extremely successful when explaining and modeling inconsistency in the empirical sciences (See Bueno, 1997, 1999; Bueno, French & Ladyman, 2002; da Costa & French, 2003; Bueno & French, 2011). The plan for this section is the following: Sec. 4.4.1 is devoted to argue that *PPEE* is a good candidate for providing an integrative approach to inconsistency toleration in the empirical sciences, in order to do so, I present the philosophical spirit behind the approach, leaving the technicalities aside for a moment. Sec. 4.4.2, consists in introducing the formal apparatus of *PPEE* when characterizing empirically adequate theories.

4.4.1 The spirit of *PPEE*

Taking in to account the challenges presented just above, here I explain how the general features of *PPEE* are enough to promote an integrative approach to inconsistency in science.

The first two challenges can be summarized in terms of some kind of expressivity. Any formal resource that aims at modeling inconsistency toleration in the sciences should be able to express different different types of scientific statements.

As a response to those challenges: despite of being a type of semantic approach to scientific theories, the *PPEE* proposal of the partial structures presented by da Costa and French (Cf. 2002, 2003) and Bueno (Cf. 1999), abandons some assumptions of traditional model-theoretical and structuralist conceptions in order to obtain more flexible structures that allow for some aspects of scientific practice, starting, for example, by the fact that much of the available scientific information about a given domain is incomplete and that scientific activity is an "open" enterprise. Below we briefly present the basic elements of the *PPEE* proposal and show how they can help formalize the requirements of the preceding section.

The generalities of *PPEE* can be phrased in the following way. Let D be a domain of knowledge and L an appropriate language to formalize scientific statements, for example, a first-order modal language (perhaps with different first-order quantification domains). The *PPEE* proposal is that our knowledge of D is systematized by means of a simple pragmatic structure, a formal theory of partial truth and an adequate logic for scientific reasoning −this, of course, vary from case to case. This openness is enough to say that *PPEE* can easily satisfy the first requirement mentioned in the previous section: the indeterminate character of L, but even if it was a first-order modal language, it allows sufficient flexibility to have linguistic elements sufficiently sophisticated to distinguish between sentences about abstract entities and sentences about empirical objects.

Furthermore, according to *PPEE*, a scientific theory is formulated in the following set−theoretic construct: $T =< \boldsymbol{D}, \boldsymbol{R}_i^n >$, for which $\boldsymbol{D}$ is a determined domain and $\boldsymbol{R}_i$ is a family of n-place relations holding between the elements of $\boldsymbol{D}$ (Bueno, 1997: 588). T contains substructures (*partial structures*) of the form $A =<D, R_k>_{k\in K}$; where each R is a *partial relation*, where one of those is a finite ordered list which contains the elements that are known to belong to R, another is a list that contains the elements that are known to not belong to R, and finally, there is a list that contains the elements for which it remains indeterminate whether they belong to R (Bueno & French, 2011: 858-59).

As it was mentioned in Chap. 2, the fact that partial structures can, since the very beginning, account for the scientific elements that are product of ignorance (namely, the ones that are listed in R_3) should be seen as a great advantage of the approach over other logical programs for inconsistent science. The reason for doing this is that, as it was argued in Chap. 1, ignorance has played an extremely important role for the explanation of historical cases of inconsistency toleration in the empirical sciences; therefore, if the formal tool account for this phenomenon, it seems more likely that it can also provide us with accurate reconstructions of the reasoning or procedures associated to inconsistency toleration.

Regarding, the third challenge: any formal tool that aim at modeling inconsistency toleration should be able to portrait the use of false, inconsistent and partial information in the sciences; in particular, it should show the ways in which defective information is arranged within theories in order to predict, describe or measure certain phenomena.

In this respect, *PPEE* provides a way to explain why partial information is, under certain circumstances, considered reliable in scientific practice. As a matter of fact, *PPEE* was initially motivated by a pragmatic approach truth which was not only compatible but explanatory of the success of these practices −and while the *pragmatic truth* view is quite philosophically laden, what has remained in all the *PPEE* projects is a less contentious understanding of truth, namely *quasi-truth* or *partial truth*.

Quasi-truth is strictly weaker than truth, "in the sense that if a sentence is true, then it is quasi-true, but the converse doesn't hold in general. Moreover, if a sentence is quasi-false, then it is false, but the converse doesn't hold in general either. (For further details, see Bueno [2000], and da Costa and French [2003].) Furthermore, it is also possible that a sentence s is quasi-true and its negation, *s*, is quasi-true as well" (Bueno French, 2011: 860). It should also be noted that the *PPEE* aims also at addressing the implicit processes, techniques and rules that relate empirical theories to specific domains −processes like the use measuring devices and methods, observation techniques, auxiliary theories, paradigmatic examples, etc. And because the elements of specific partial structures are determined by such processes, techniques and rules of which depends on T to be accepted as (at least pragmatically) true about $\boldsymbol{D}$.

With the latter we have sufficient elements to say that the notion of partial structure applied to the formalization of scientific theories at least recognizes in nuce the need to establish a link between the formal reconstructions of theories with the procedures and results of scientific practices, which was the third requirement set forth in the previous section.

As for the doxastic commitments requirement: *PPEE* seems to do quite well with respect to the second challenge, about different commitments towards different type of scientific statements. as partial structures are defined as sets of sentences distinguished by our commitments to them, since they are the sentences accepted as true simpliciter and that are determined by processes, rules and techniques also distinguished by our commitments to them. In addition, the distinction between truth and quasi-truth expresses a certain kind of difference at the level of doxastic commitments. This may not be enough to say that the notion of partial structure allows in fact to separate the types and levels of doxastic commitment, because that probably should be captured as part of the logic that one is going to associate with pragmatic structures, as Costa does (2000: 185ss), but in any case it should be emphasized that this notion of partial structure is not alien to, and does not prevent, the formalization of the types and levels of doxastic commitment that might be required.

In the following section, I proceed to introduce *PPEE* in more detail.

4.4.2 *PPEE* Empirically adequate empirical theory

PPEE is a particular type of semantic approach to scientific theories, which enriches the traditional semantic view leaving room for the handling of defective (incomplete, partial, conflicting and inconsistent) information (see Ladyman & French, 1999, 2002).

The basic idea behind the *PPEE* "consists in acknowledging the (rather obvious) fact that much scientific information at our disposal, in the various possible fields of research, is blatantly incomplete" (Bueno, 1997: 591). *Partial structures* constitute formal tools that help to reconstruct and describe scientific theories that are allegedly inconsistent, incomplete, and imprecise, among others. This approach combines theoretical commitments from the Semantic View on scientific theories with a more pragmatic set of notions such as 'approximate truth' (pragmatic truth, quasi-truth, etc.) (Cf. Bueno, 1997; Bueno, French & Ladyman, 2002; da Costa & French, 2003; Bueno & French, 2011). As this approach has been used constantly to address cases of alleged inconsistent science, I consider it to be optimal for characterizing, at least, *inconsistent* empirical theories.

Let *scientific theories* to be formulated based on the following theoretical model: $T =< \boldsymbol{D}, \boldsymbol{R}_i^n >$ "where $\boldsymbol{D}$ is a particular domain (a set of objects to which the theory is supposed to apply) and $\boldsymbol{R}_i$ is a family of n-place relations holding between the elements of $\boldsymbol{D}$" (Bueno, 1997: 588). T consists of a set substructures (partial structures), $<A$, A', $...A^n>$, of the form $A= <D, R_i>_{i\in I}$ –

for which $D \subseteq \boldsymbol{D}$ and $R_i = \boldsymbol{R}_i \cap D$.

Furthermore,

> [E]ach R_i is a partial relation. A partial relation R_i over D is a relation that is not necessarily defined for all n-tuples of elements of D (see da Costa and French 1990: 255). Each partial relation R can be viewed as an ordered triple $<R_1, R_2, R_3>$, where R_1, R_2 and R_3 are mutually disjoint sets, with $R_1 \cup R2 \cup R3 = D^n$ and such that: R_1 is the set of n-tuples that (we take to) belong to R ; R_2 is the set of n-tuples that (we take) do not belong to R, and R_3 is the set of n-tuples for which it is not defined whether belong or not to R. (Bueno & French, 2011: 858-59).

An *empirical theory* consists of, at least, two sets of empirical (partial) structures: $A = <D, R_k>_{k \in K}$ and $A' = <D', R'_k>_{k \in K}$, one of which is the result of the theory being examined and the other one which refers to the domain of observable objects. For empirical theories, each of these partial relationships allows connecting propositions about observables with propositions that are only logical consequences of the theory (that is, predictions, descriptions or explanations about elements of D). R_1 indicates cases in which an element of D fully satisfies a model of the theory, R_2 indicates that the elements of D linked by this relation are not models of the theory, and, R_3 indicates that it is unknown whether elements of D are models of the theory or not (however, at the same time, these elements seem relevant in the area of study).

If having two partial structures, $A = <D, R_k>_{k \in K}$ and $A' = <D', R'_k>_{k \in K}$, a function f from D to D' will constitute a *partial* isomorphism between A and A' if :

1. f is bijective, and
2. for all x and y in D:
   $R_{k1}xy \leftrightarrow R'_{k1}, R'_{k1}f(x)f(y)$ and $R_{k2}xy \leftrightarrow R'_{k2}f(x)f(y)$

   (see Bueno, 1997: 592-95; Bueno & French, 2011: 859).

"Partial isomorphism allows the 'openness' necessary for claiming that a theory is empirically adequate with regard to the information at our disposal on the empirical level (represented by the R_1, and R_2, components of the partial relations in question)"(Bueno, 1997: 607).

The concept of partial isomorphism allows to see that, if the theory's empirical substructures have a counterpart in the phenomena, as far as its partial relations are concerned, the theory is *empirically adequate.*

Furthermore, let a (partial) function f: $D \rightarrow D'$ to be a partial morphism from A to A' if for every x and every in D, $R_{k1}xy \rightarrow R'_{k1}$,$R'_{k1}f(x)f(y)$ and $R_{k2}xy \rightarrow R'_{k2}f(x)f(y)$.

We can then define a notion of *quasi-truth*: Take B to be a total structure if its

> relations of arity n are defined for all n-tuples of elements of its universe, and if P is a set of accepted sentences, then B is said to be $A-$*normal* if:
>
> (i) the universe of B is D;
>
> (ii) the relations of B extend the corresponding partial relations of A;
>
> (iii) if c is an individual constant, then c is interpreted by the same element in both A and B;
>
> (iv) if $s \in P$, then $B \models s$ (where '$\models$' stands for the logical consequence relation in the Tarskian sense).
>
> Loosely speaking, a total structure is called $A-$*normal* if it has the same similarity type as A, its relations extend the corresponding partial relations of A, and the sentences of P are true, in the Tarskian sense, in B. Then a sentence s is said to be quasi-true in a partial structure A, or in the domain D that A partially reflects, if there is an $A-$*normal* structure B and s is true in the Tarskian sense in B. (Bueno & French, 2011: 859).

As I have mentioned before, quasi-truth is weaker than truth. If a sentence s is true, s is also quasi-true, however, the converse does not need to hold. In addition, if s is quasi false, then it is false; but s being false, does not mean that s is quasi-false. Contradictions in science are usually cases in which two sentences, s and are quasi-true (Bueno & French, 2011: 860).

Finally, a theory T "is p*artially empirically adequate* if for some of its models there is a partial isomorphism holding between all the models of phenomena[4] (conceived as partial structures) and the partial empirical substructures of the

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[4] *Models of phenomena* should be understood (in a broad sense) as appearances.

To see the importance of models of phenomena for determining the empirical adequacy of a specific theory, the following distinction is important:

Models of data: Are obtained fundamentally on the basis of considerations grounded on (1) statistical inferences and (2) particular bits of information that depend on certain theories.

Models of phenomena : Present a relative independence (in their construction) from particular theories, having thus a greater stability than the models of data. (Cf. Bueno, 1997: 600-605) model-that is, if the appearances are partially embeddable in the theory" (Bueno, 1997: 596). Many more can be said about the generalities of the *PPEE* but for the purposes of this chapter this should suffice. In the next section I put *PPEE* into use when providing a formal reconstruction of the anomaly in the measuring of the solar neutrinos' flux.

4.5 Explaining a case study

The aim of this section is to sketch a way in which partial structures can help philosophers to provide formal reconstructions of the phenomenon of inconsistency toleration. As I have mentioned before, I focus on the study of toleration of contradictions between theory and observation as these contradictions have been generally regarded as remarkably frequent and very often non-problematic at all. In order to show how partial structures can help us to explain and understand the reasoning and the scientific practices associated to the use of inconsistent information, here I scrutinize a case study that was first introduced in Chap. 1 and Chap. 2, namely the anomaly in the measuring of the solar neutrinos' flux. The plan for this section is the following: Sec. 4.5.1 is devoted to set the aim of using partial structures to explain this case. In Sec. 4.5.2, I summarize the case study. Sec. 4.5.3 is devoted to present the reconstruction of the case study using the *PPEE*.

4.5.1 The aimed reconstruction

A *rational reconstruction* of a particular scientific episode constitutes an attempt to provide understanding about a scientific episode. Philosophical reconstructions of science are not only historical data, but also a particular way to put such data together in a way that it can provide an explanation of why that particular episode occurred the way it did. In Chap. 3, I had already discussed some problems associated to the philosophical use of history of science yet, here I focus briefly on one of the most important critiques that formal analyses of historical case studies.

While, at least among the supporters of the Paraconsistent view, there is a common agreement on how certain case studies could –in general terms– illustrate inconsistency toleration in science, there is no consensus in the way such cases should be precisely explained. As a matter of fact, the majority of such case studies have been objects of dissimilar rational reconstructions and thus, of dissimilar explanations. So, it seems important to pay special attention to the methodologies at our disposal that can help us to approach historical episodes of non-trivial inconsistent scientific reasoning in an philosophically informative way. In particular, this section is devoted to discuss the scope of the *PPEE*, as a Paraconsistent Alternative Approach (PAA), when used to model and explain the tolerance of contradictions in the empirical sciences.

The question at hand is *what should we expect from using partial structures to model cases of inconsistency toleration*. The obvious answer is that by filling partial structures with historical data, one would be able to provide rational reconstructions of inconsistency toleration in science, this is, to provide explanations of why certain inconsistent theories have been accepted (either weakly or strongly) by certain scientific communities, or explanations of why theories that were shown to be inconsistent with relevant observational reports were not rejected once the scientists were aware of the contradiction.

In particular, an integrative approach to inconsistency toleration −such that combines historical, logical and methodological worries, should provide *realistic reconstructions of inconsistency toleration*. Namely, if *PPEE* is to be taken as a satisfactorily formal methodology for the study of inconsistent science, it should allow for descriptions of the most natural information-transmitting procedures that scientists use (and have used) when dealing with contradiction in their disciplines. This type of reconstructions should be explicative of how scientists were lead to consider as sensible certain uses of their inconsistent theory given a particular context –rather than to be explicative of what causes certain results given by the theory to be (partially) true (see Margáin, 1976). Therefore, realistic reconstructions of inconsistency toleration are rational reconstructions of specific scientific episodes in which it is explain how was it possible for the scientists to avoid logical triviality when working with inconsistent information −leaving aside worries about scientific realism and its connections with contradictions and other defects.

With this in mind, in the following subsections I go back to the anomaly in the measuring of the solar neutrinos' flux and try to use *PPEE* to provide a realistic reconstruction of how the contradiction was tolerated for almost 30 years.

4.5.2 The case study

As the anomaly in the measuring of the solar neutrinos' flux has been already presented in detail in Sec. 1.4 and Sec. 2.5, here I only sketch very briefly some of the salient features of the case.

(Solar) *neutrinos* are subatomic particles that are generated from the solar fusion. Neutrinos were initially considered to have neither electric charge nor mass, and to be of just one type.[5] In 1960, physicists felt confident enough to commit to a research project for the detection and measuring of the solar neutrino flux; however, by that moment, they did not have at hand any theory that could explain and predict the behaviour of subatomic particles in the stars. So, John Bahcall had to design a mathematical model that could make the flux of solar neutrinos not only measurable but observable as well (see Bahcall, 2003: 78). This model received the name of *Standard Solar Model* (SSM). The SSM combined hypothesis from very diverse disciplines from physics; the following image illustrates how assumptions from different disciplines were combined to give rise to the SSM. See figure 1.1.:

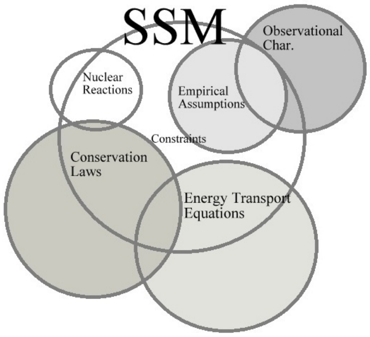

Figure 1.1. The SSM.

When Bahcall provided the first SSM, a research team, led by Ray Davis, designed the experimental set up to test the model's predictions. As there were no other theories, on the behaviour of the stars (and specially on the behavior of solar neutrinos), different from the SSM, the theory that underlie the experiment was a significant segment of the SSM.[6]

Between 1967 and 1968, the experiment was performed twice and in both occasions, the SSM's predictions turned out to be almost 2.5 times larger than the

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[5] In 1930, *neutrinos* were originally introduced by Pauli as hypothetical particles that were needed for accounting for the reactions that later would be known as "β-decay" (Bilenky, 2012).

[6] Some of the commitments that the experiment inherited from the SSM were:it assumed that the information that scientists had at that moment about the collisional cross sections[7] of certain elements such as Ar^{37} and Cl^{37} was correct. In addition, it assumed that the informatics that underlies the data analysis of the Geiger counter reports were reliable.

results reported by the experiment (Bahcall, 2003: 79). The difference between the predictions and the observational results was large enough to not being explainable by appealing to a margin of error, making the observational outcome impossible to be considered even compatible with the SSM's predictions.

While the SSM was initially developed to measure the flux of solar neutrinos, and despite the fact that at that matter it was inconsistent with the observational reports, the model was kept in use by the physicists (specially astrophysicists) because it produced accurate predictions about other properties and behaviours of the stars.[8] This made the study of neutrino production and neutrino detection only two of many more domains of application for the SSM.

Despite the fact that the anomaly in the measuring of the solar neutrinos' flux constituted one of the most important open problems in physics during the 20th Century, given the success of the SSM in other associated disciplines, the model was not rejected when the anomaly was first discovered nor it was 'incarcerated' to be fixed in secrecy. As a matter of fact, the way in which scientists dealt with the anomaly was a combination of using and constantly revising the model as a whole and each of its commitments.

Many auxiliary hypotheses were offered to make the theory and observation consistent, some of ones that were examined include:

- It was initially suggested that the one to blame for the anomaly were the assumptions regarding the cross sections of Ar^{37} and Cl^{37} (which were known with too little precision at the time). This, made that the experiments that followed, explored reactions between other elements.

- 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.

- In different occasions, it was suggested that neutrinos were of different types (*neutrino oscillation*); however, for different reasons (some experimental limita-

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[8] For instance, it could (satisfactorily) estimate the helium abundance in the stars as well as the corresponding mixing length parameter. Once given the correct luminosity and radios of any given star, the SSM could predict accurately the star's age. In addition, it also provided ways to evaluate other (more complex) models of phenomena such as rotation, magnetic fields, turbulence, among others.

tions, and conflicts between the hypothesis and some basic assumptions of the theory), this thesis was dismissed few times before it was finally accepted.

- By 1996, it was also argued that the anomaly was (partially) caused by messy way in which the Coulomb coefficients were calculated for problems of the type of neutrino flux −which was considered to be a 2 (or more) body problem (see Fukasaku & Fujita, 1996).

- By the end of the 1990's, the hypotheses of neutrinos being of different types and having mass were considered as serious candidates for explaining this phenomenon. But it was until 2001 that the phenomenon of neutrino oscillation was accepted as an explanatory working hypothesis.

Finally, in 2015, Takaaki Kajita and Arthur B. McDonald were awarded with the 2015 Nobel Prize in Physics for the discovery of neutrino oscillations, proving that neutrinos are of different types and that they have mass (in order to be able to change identities, neutrinos must have mass).

The following image illustrates the anomaly and its relation with some of the different alternatives that were proposed for solving the contradiction −some of which were, at the end, successful.

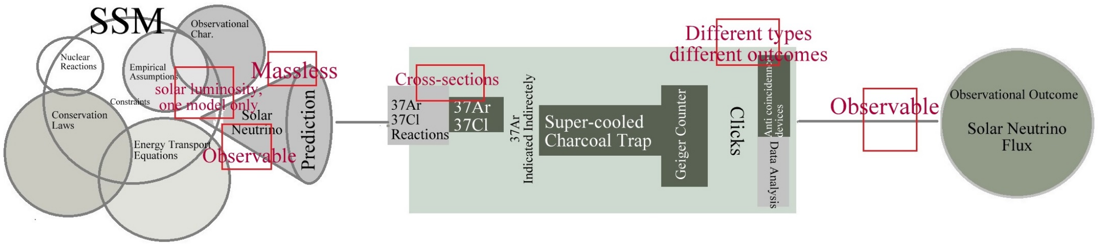

Figure 1.2. The anomaly in the measuring of solar neutrinos' flux.

With all this facts in mind, in the next subsection, I provide a reconstruction of the anomaly in terms of *PPEE*.

4.5.3 A *PPEE* reconstruction

In the previous chapters, I explained this case study in two steps. First, in Sec. 1.5, I argued that scientists were rationally inclined to tolerate the contradiction between *SSM* and the experimental reports, basically, because they were ignorant of the way in which the elements that they imported from radiochemistry, nuclear physics and astrophysics hanged together with what they already had from (the budding) neutrino physics (I called this, *ignorance of the theoretical structure of SSM*).

The second step that I took when explaining this case was to, in Sec. 2.6, state that the ignorance of theoretical structure that scientists were victims of actually had an impact in the way in which they perceived, reconstructed and treated the anomaly. During the time in which they tolerated the anomaly, the ignorance involved as well as the significant overlap between the SSM and the experiment, gave the impression that, due to the holistic properties of the whole body of knowledge (union of SSM and the theory that underlie the design of the experiment), isolation techniques were insufficient to solve the problem; so, scientists worked mostly on ways to disentangle the SSM and the experimental design.

In what follows, I try to account for this case by using the *PPEE*.

First, let's take the SSM to be an *PPEE*-empirical theory.

$$SSM = < \boldsymbol{D}, \boldsymbol{R}_i^n >$$

Where:

- $\boldsymbol{D}$ consists on a set of different compositional properties of the stars.
- $\boldsymbol{R}_i$ is a family of n-place relations holding between the elements of $\boldsymbol{D}$.
- SSM consists of a set substructures (partial structures):

  $<A_{NeuProd}$, $A_{NeuDetc}$, $A_{Heliosmg}$, $A_{RadiatvSp}$ $...A^n>$.[9]
  - Partial structures are of the form: $A = <D, R_i>_{i \in I}$, for which,
  - $D \subseteq \boldsymbol{D}$ and $R_i = \boldsymbol{R}_i \cap D$

  Where, for instance:
- $R_{NeuProd1}$ indicates cases in which an element of $D_{NeuProd}$ fully satisfies a model of the theory with respect of the production of solar neutrinos,
- $R_{NeuProd2}$ indicates that the elements of $D_{NeuProd}$ linked by this relation are not models of the theory,

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[9] This structures are determined by the domain of application of the theory, for instance: while $A_{NeuProd}$ is constructed by taking all elements from the SSM that were needed to predict the production of solar neutrinos, $A_{Heliosmg}$ is build by taking all SSM elements that are needed for the study of wave oscillations in the stars. This substructures can shared elements between them but what does large part of the work is that they also have non-shared elements.

  - and, $R_{NeuProd3}$ indicates that it is unknown whether elements of $D_{NeuProd}$ are models of the theory or not (however, at the same time, these elements seem relevant in the area of study).

Now, the anomaly in the measuring of the solar neutrinos' flux required to pay attention to at least four partial structures, namely: $A_{NeuProd}$, $A_{NeuDetc}$, A_{NeuExp} and A_{Emp}.

- The first, $A_{NeuProd}$, was built by selecting the elements of the SSM associated to the production of neutrinos (information about the process in which four protons are combined to produce two protons, two neutrons, two positrons; information about nuclear reactions and cross sections of elements such as Ar^{37} and Cl^{37}, among others.
- The second, $A_{NeuDetc}$, was built by selecting the elements of the SSM associated to the detection and measurement of neutrinos. These elements include the assumption that neutrinos were massless and of just one type.
- The third substructure, A_{NeuExp}, was built by selecting the SSM associated that could suffice for the design of the experiment. As I had previously discussed, the majority of elements contained in A_{NeuExp} were, problematically, shared by $A_{NeuProd}$ and $A_{NeuDetc}$.
- Finally, the fourth substructure, A_{Emp}, is expected to be an empirical substructure. This one was built by selecting the empirical elements of the SSM (which information about the luminosity and radius of our Sun that constrained the SSM for the domain corresponding to the properties of our Sun).

When the anomaly was discovered, in 1968, it was clear that, it was not possible to provide a partial isomorphism between any of the first three substructures and A_{Emp}.

If one trusts the Traditional view general explanation for cases of alleged inconsistency toleration, it is necessary to say that at the moment when the anomaly was discovered, scientists faced the following dilemma: either to weaken their commitments towards the empirical substructure A_{Emp} or to weaken their commitments towards $A_{NeuProd}$, $A_{NeuDetc}$ and A_{NeuExp}.

The first option was ruled out by the success of the SSM in other areas of research. As a matter of fact, SSM was considered to be empirically adequate with respect to the prediction of the age of the stars, the calculations of luminosity and the presence of heavy metals in the stars, among other empirical elements of the studied domain. It took very little for A_{Emp} to be regarded as an $A-normal$ structure -in connection with other substructures such as $A_{Heliosmg}$, $A_{RadiatvSp}$.

In light of the above, the second option was the most natural to take by the physicists. As a matter of fact, from 1968 to the end of the 1990's, the physicists' doxastic commitments towards some of the (scrutinized) assumptions of SSM were weakened $-$Ssuch assumptions include the information about cross sections of Ar^{37} and Cl^{37} (contained in $A_{NeuProd}$) and the problems linked to the calculation of Coulomb coefficients (contained in $A_{NeuDetc}$). These elements can be naturally captured by the R_3 components, as their truth value (within the theory) was still to be determined.

But, to weaken the doxastic commitments towards these elements was not enough to prevent the theory from being inconsistent. The predictions and the observational reports were still obtainable despite the commitments that scientists held towards the distinct sets of information, this is:

1. $SSM :< A_{NeuProd}, A_{NeuDetc}, A_{NeuExp}, A_{Emp}, ..., A^n >$
2. $A_{NeuProd} \vDash s$
3. $A_{NeuDetc} \vDash s$
4. $A_{NeuExp} \vDash s$
5. $A_{Emp} \vDash \neg s$

This, works as support for two main theses: on the one hand, it shows (as the Traditional view claims) that scientists responded immediately to the presence of the contradiction by weakening their doxastic commitments. On the other hand, it also shows (in favor of the Paraconsistent view) that the weakening of the commitments is not enough for dissolving the contradiction. The theory was still inconsistent regardless what scientists thought about it, and that is the reason why they kept calling this an anomaly and tried desperately to solve it.

Since 1960, the traditional characterization of neutrinos as massless was taken to be more an empirical constraint than a theoretical one; thus, this assumption was shared by all the substructures that addressed the problems associated to neutrinos' presence. However, by the end of the 1990's, when the hypotheses of neutrinos being of different types as having mass were seriously considered, these assumptions (included in the three relevant substructures, $A_{NeuProd}$, $A_{NeuDetc}$ and A_{NeuExp}, and the empirical substructure A_{Emp}) were 'moved into' the R_3 components.

By 2015, another strong change was made, the assumptions of neutrinos being of different types as having mass were seriously considered were discovered to be true; this had the effect of moving the traditional characterization of neutrinos (as massless and of just one type) to the R_2 components. Finally, roughly speaking, the characterization of neutrinos provided by Takaaki Kajita and Arthur B. McDonald in 2015 is now pragmatically true in the partial structures $A_{NeuProd}$, $A_{NeuDetc}$, A_{NeuExp} and A_{Emp}. Doing so, changed the landscape to the following:

1. $SSM :< A_{NeuProd}, A_{NeuDetc}, A_{NeuExp}, A_{Emp}, ..., A^{n} >$
2. $A_{NeuProd} \models \neg s, A_{NeuProd} \not\models s$
3. $A_{NeuDetc} \models \neg s, A_{NeuDetc} \not\models s$
4. $A_{NeuExp} \models \neg s, A_{NeuExp} \not\models s$
5. $A_{Emp} \models \neg s$

What this means is not that the theory is completed, but that the *SSM* is now regarded as empirically adequate with respect of solar neutrinos'.

The reconstruction that I provided in this section has combined all the relevant historical data about the case study including the scientists' weakening of their doxastic commitments as well as the remaining contradiction between the *SSM* and the experimental results. In addition, this reconstruction also highlighted the way in which the theory was updated during the time in which the contradiction remained, and the way in which the conflicting information was placed within the *SSM*.

Finally, even if, in the long run, one discovers that the reconstruction was historically inaccurate there are two things that have being gained through this exercise that will not be lost: on the one hand, the explanation of how *PPEE* work when modeling and explaining the tolerance of contradictions at the theory level (informed by the scientists' doxastic commitments) as well as the way in which, pace the Traditional view, a theory can remain inconsistent despite the weakening of the scientists' doxastic commitments towards it.

4.6 Final Remarks

Here I argued in favor of the integration of historical, logical and methodological approaches to inconsistency toleration, this in order to provide precise explanations and promote the understanding of the historical episodes that, allegedly, illustrate inconsistency toleration in the sciences. Motivated by this plea, I used the *PPEE* for reconstructing a historical episode that was first introduced in Chap. 1 and Chap- 2 as a case of inconsistency toleration.

In order to do so, I preceded as follows. First, in Sec. 4.2, I explained that the Traditional view often considers purely historical analyses of cases of inconsistency toleration to be enough for dismissing the Paraconsistent theses about science. However, I argued, such analyses are not robust enough to help to adjudicate the debate −and, as a matter of fact, sometimes the results of the reconstructions built by purely historical analyses of science coincide with the reconstructions provided by the Paraconsistent view. A, perhaps, undesirable result.

Later on, in Sec. 4.3, I discussed two ways in which historical analyses of inconsistent science can be enriched by incorporating methodological and logical elements. I first introduced what I called the *Paraconsistent Logical Approaches* (PLA) and explained the difficulties these approaches face. Here, i also presented the *Paraconsistent Alternative Approaches* (PAA) which tends to make the reconstructions more historically informed and less committed to particular logical consequence relation (which could be seen as *less philosophically biased*).

With the above in mind, in Sec. 4.4, I discussed the possibility of using *PPEE* to provide historically informed formal reconstructions of inconsistency toleration in the sciences. Sec. 4.5 was devoted to provide a *PPEE* reconstruction of a very complex scientific episode that illustrated the toleration of a contradiction between theory and observation, the episode was the 30-years toleration of the anomaly in the measuring of the solar neutrinos' flux. The resulting reconstruction shed light on the way in which scientists kept safely working with an inconsistent theory for 30 years while dealing with the their ignorance of the theoretical structure of the theory −which was partially responsible for the anomaly.

In sum, this chapter was devoted to use *PPEE* (as an PAA) to scrutinize a case study that illustrated how scientists can work with an inconsistent theory that was empirically adequate. The following chapter is devoted to present a different PAA in order to explain how scientists can combine mutually inconsistent models in order to obtain empirically accurate predictions.

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5 Reconstructing inconsistent science using *Bundle Chunk and Permeate*

5.1 Introduction

In Chap. 2 I introduced three historical exemplars of contradictions between theory and observation, and I argued that this historical evidence supported the intuition that inconsistency toleration actually takes place in the empirical sciences. This chapter provides an application of the paraconsistent reasoning strategy *Chunk and Permeate* to a case of inconsistency toleration in the empirical sciences. This is done in order to provide an alternative explanation (to the one presented in the previous chapter) of how a contradiction between theory and observation can be tolerated without damaging scientific reasoning.

This chapter consists of a joint work with Michéle Friend, and it is devoted to discuss the scope and the limitations of a paraconsistent tool named *Chunk and Permeate* (first introduced by Brown & Priest (2004)) when used to model and explain actual cases of non-trivial inconsistent scientific reasoning. This chapter helps to shed light on the reasoning that scientists carry out when tolerating contradictions in the empirical sciences. In order to do so, Friend and I first describe Chunk and Permeate as a formal tool designed to model and explain the reasoning that underlies inconsistency toleration. Later on, we argue in favor of some needed adaptations for Chunk and Permeate in order to improve its performance when used to model complex cases of scientific reasoning. We extend Chunk and Permeate by adding a visually transparent way of guiding the individuation of chunks and deciding on what information permeates from one chunk to the next. This extension is named *Bundle Chunk and Permeate* and, in this chapter we apply it to one of the case studies introduced previously in Chap. 2, namely, the anomalous magic numbers.

The significance of our work is that we can more clearly discuss some philosophical considerations on the subject of inconsistency toleration in formal and empirical sciences. More specifically, here we focus on three important issues: the role of liar cycles and inconsistent reasoning in the formal sciences, the use of Chunk and Permeate and Bundled Chunk and Permeate for modelling, reconstructing, explaining and providing understanding of inconsistent scientific reasoning, and we discuss the implications of inconsistency toleration in science especially in the light of the research programmes that aim at the unification of science.

In order to address these issues, the chapter is divided in two main parts: the first one is devoted to logic and mathematics. We elaborate on the benefits of using Chunk and Permeate when modelling inconsistent scientific reasoning, and present our method. This part includes Sec. 5.2 to Sec. 5.5. In particular, in Sec. 5.2 we explain why it is that reasoning with inconsistencies is assumed to be problematic from the point of view of the philosophy of logic. In Sec. 5.3 we introduce the Chunk and Permeate strategy and give reasons for looking at bundled Chunk and Permeate. In Sec. 5.4 we introduce bundle diagrams. In Sec. 5.5 we use the bundle diagrams to individuate chunks and determine permeation for liar cycles.

In the second part of the chapter, we discuss science. This part includes Sec. 5.6 to Sec. 5.11. In Sec. 4.6, we make some general remarks about the problem of inconsistency in science. In Sec. 5.7, we introduce some considerations from the philosophy of science about how to individuate and combine mutually inconsistent theories or models. In Sec. 5.8, we provide an example of a globally inconsistent union of models and we apply Bundled Chunk and Permeate, to this example. In Sec. 5.9, we present further insights concerning why inconsistent groups of theories are thought to be a problem for science. In Sec. Sec. 5.10, we draw some philosophical conclusions concerning the unification of science, the nature of consistency and reasoning paraconsistently, and what this means, while using only consistent formal representations of logical reasoning locally.

5.2 Trivialism and Modern Mathematics

We shall be looking at the problems with trivialism and modern mathematics through two lenses, the classical lens and the constructivist lens. We begin with trivialism and why it is a problem.

Under classical, model-theoretic conceptions of semantics, a trivial theory is one where every formula in the language is true; under constructivist or proof-theoretic conceptions a trivial theory is one where any well-formed formula of the language can be derived. A trivial arithmetical theory would have it that $2 + 2 = 4$, but also $2 + 2 = 19$, $2 \times 93 = 6$ and so on. This is not a useful theory of arithmetic for science. In fact, it is a disastrous theory of arithmetic, since it is completely undiscerning between the true theorems or equations and the false ones (as seen from a more traditional consistent and classical conception of arithmetic). There is no false statement, only ungrammatical ones, and ungrammatical statements are, arguably, not counted as statements. Grammatical statements of the theory are all true, all derivable and their negations are all true and derivable. The conceptions of arithmetic error and correction are lost, and arguably (Priest, 2006: Ch.3), meaning is also lost. Trivial theories are to be avoided according to the more common present practice of mathematics.[1]

Most mathematicians claim[2] that they are classical or constructivist reasoners, (Hellman & Bell, 2006: 64 – 70) so they think that if there is a contradiction in their theory, then their theory becomes trivial.[3] In other words, one route

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[1] We thank an anonymous reviewer for reminding us that there are supporters of trivialism, and those who think that we can reason sensibly even in a trivial setting. In fact, this is almost what we show using Chunk and Permeate. 'Almost' means that there is some ambiguity as to what this means. See some remarks in the conclusion for elaboration. Since our concern is with present-day practice and reasoning in science, we maintain that at present there are no trivialists in science.

[2] Some relevant and paraconsistent logicians claim that such mathematicians are actually, as manifested in their reasoning behaviour, relevant or paraconsistent reasoners. This nuance will be addressed in the conclusion.

[3] The logicians and mathematicians who disagree with this, who think that *ex contradictione quodlibet* proofs are *invalid*, are relevant logicians or paraconsistent logicians. The *philosophical* difference is that relevant logicians insist on there being a relevant connection between premises and conclusion, paraconsistent logicians think that we can reason coherently with contradictions, or through a contradiction, and they model such reasoning. Briefly, in a paraconsistent logic, while you can derive an infinite number of formulas, as you can from any formula in any logic with a minimum set of inference rules, you cannot derive very much of interest from a contradiction. It is treated as a logical singularity. From $p \wedge \neg p$, you can derive p, $\neg p$, by $\wedge$-elimination, then by $\wedge$-introduction, you can derive $(p \wedge \neg p) \wedge p$ and so on, with double negation introduction you could derive $\neg\neg p$... The point is that you cannot get to an arbitrary q.

Logically, what distinguishes relevant from other paraconsistent logics is that relevant logicians, as part of the bigger substructural tradition, restrict some structural rules rather than operational ones. Non-relevant paraconsistent logicians change the behavior of the connectives (especially negation) while preserving the full set of structural rules of the language. This guarantees that they stay as close to classical logic as possible (i.e. Priest's *LP*).

This second way of putting the distinction reveals an important bias in this chap-to trivialism starts from classical or constructivist reasoning, you then meet a convincing contradiction that you do not think can be explained away, reason as you would through the *ex contradictione quodlibet* argument and you find yourself in a recognisably trivial theory. By *modus tollens*, if we think we are not in a trivial setting, since this might be thought to be *a priori* impossible, or we think that our theory is not trivial and we are unwilling to give up our classical or constructivist reasoning, then our theory had better not have any contradictions. So, we can avoid trivialism by remaining convinced that whatever looks like a contradiction in our theory must *a priori* not be one. This leap back from the brink of trivialism is rational, in fact, it is quite common in science, and it explains, or excuses, the separated reasoning where the global theory is inconsistent but the local pieces are consistent. When we meet an apparent contradiction in our classical or constructivist setting, we stay short of going through the *ex contradictione quodlibet* reasoning.[4]

Let us turn to modern mathematics. Modern Western mathematicians make proofs.[5] Often, these are only partly formal, so we might not notice a contradiction. Some proofs include sets of premises, lemmas or theorems that *belong to* theories that are inconsistent with each other, in that they use information from different theories, and the theories themselves contradict each other. Some proofs include sets of premises, lemmas or theorems that are inconsistent with each other in the stronger sense that it is possible to derive a contradiction from them. Even worse, few mathematicians seem perturbed by this despite the threat of trivialism. How do we explain the lack of concern?

First note that none[6] of these proofs use an *ex contradictione quodlibet* proof or

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ter and for Chunk and Permeate in general: it appeals to a specific kind of logician/mathematician/scientist. Martínez-Ordaz would say that this particular kind of reasoner is one who admits that classical logic is along the right lines and is a good starting point and possibly thinks that formal representations of relevant reasoning sacrifice too much or change the reasoning too much. Chunk and Permeate then appeals to: classical, constructive and some (non-relevant) paraconsistent reasoners (those who think that inconsistency toleration is alright but we should nevertheless reason as consistently as possible).

[4] Later, we shall see that this is exactly what Abramsky recommends.

[5] Not all mathematicians at all times finished their work with proofs. In the past, before the twentieth century in Europe, and in the colonies of the European countries, it became widespread in the institution of mathematics that results and ideas had to be proved. This is not the case in every mathematical culture, and it has not always been the case in European-based cultures. This is despite the fact that when detailed proofs were given, the proofs in Euclid set the standard for rigour of proof.

[6] The 'none' is meant as a challenge. The authors know of none that has been published, but of course some might have slipped into the published cannon.

sub-proof, since this would bring disaster. In order to explain this, we might speculate that they are using a paraconsistent logic, or are reasoning paraconsistently, unbeknownst to them.[7] This is not an idle thought, since some paraconsistent logicians make this claim. If we agree with it, then it makes sense to use a paraconsistent logic to reconstruct the reasoning. But this would be disingenuous towards the claims, beliefs and practices of present day working mathematicians, since few of them claim to be, or believe that they are, reasoning paraconsistently, and they are qualified to make that judgement, at least *prima facie*.

We introduce Chunk and Permeate as a reconstruction of reasoning in the presence of contradiction that respects the claims, beliefs and practices of present day working mathematicians.

5.3 Chunk and Permeate and General Remarks on Extending it

Let us highlight the original aim of Brown and Priest (2004, 2015) in developing Chunk and Permeate (henceforth *C&P*). It was to reconcile the fact that sometimes mathematicians reason on the basis of inconsistency with the fact that they deny that this is possible or makes any sense. While it may seem sensible to those used to paraconsistent reasoning to argue that the inference procedures of such mathematicians should be represented by a paraconsistent formal logic, it is not always clear that the underlying logic is represented by any of the standard formal representations of paraconsistent reasoning —or that it can be formally represented at all (Brown & Priest, 2004: 379).[8]

The *C&P* strategy consists in dividing a given a proof with inconsistent premises into consistent subsets, and to only allowing some information to permeate from

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[7] The difference is this: if they are using a paraconsistent logic, then they have recourse to a formal representation of the reasoning in the proof. If they are 'reasoning paraconsistently' then this is a looser notion. They are reasoning in such a way as to entertain and recognise contradictions but avoid trivialism. Here is the rub: which formal theory best represents their reasoning is usually ambiguous. Their reasoning is represented by a class of formal theories. They are reasoning in the spirit of paraconsistent reasoning in the sense of exercising damage control on the inconsistency.

[8] Even though, in principle, one could also have relevant or paraconsistent reasoning within a chunk, we ignore this possibility here out of the respect for the prevailing claims beliefs and practices of working mathematicians. See Priest (2015) for an example of paraconsistent logics within chunks.

one chunk to the next.[9] It is assumed that within each chunk we have perfectly 'acceptable' (i. e. consistent) reasoning that can be represented using a classical or constructive formal logic.

Sharing Brown and Priest's original intention, our purpose is to formally depict only classical or constructive reasoning (within chunks) in cases where the premises are inconsistent with each other. This restricts the more general method of *C&P* because there is nothing *a priori* forbidding us from using a paraconsistent logic within a chunk or letting formal representations of paraconsistent conceptions to permeate from one chunk to another. We set aside such possibilities here because we are holding ourselves to the more *wide-spread* current standards in mathematical proofs.

Following the *C&P* strategy, we distinguish between two different types of chunk: source chunks and target chunks. The former are the input chunks, the ones that contain the original information that is often mixed in mathematical reasoning, while the latter are the output chunks, the ones that contain the desired results of the proofs that are being modelled (Brown & Priest, 2004). Between chunks, we only allow to permeate the information we need to reach the conclusion of the chunk. We begin with the source chunks and end with the target chunk.

We should mention that a proof reconstructed with *C&P* loses cut-elimination, in the sense that premises are not always available in any chunk in the reconstruction, (Brown 2016b). Premises have to be present in a chunk to be consulted in a chunk-sub-proof. Classical and constructive reasoning places no such restriction on the use of premises. For this reason, *C&P* proofs are non-classical and non-constructive. However, we need not be alarmed. There are many formal systems of proof where cut-elimination is absent; but more important, the loss of cut-elimination *almost passes unnoticed* in each *particular C&P* proof. For, we might prove cut-elimination in a chunk, or use cut-elimination within a chunk. It is only in the overall strategy of the proof that we lose cut

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[9] An interesting question is whether we can use the chunk and permeate strategy on an *ex contradictione quodlibet* proof. Of course we can, in two different ways: one is to preserve classical validity, so the proof just is a demonstration that anything (written correctly in the formal language) can be derived from inconsistent premises. So the whole proof is one chunk. The second way is to separate the negated *reductio* inference from the double negation elimination, thus 'preserving' consistency within each chunk. A negated reductio inference is one where we conclude the negation of the hypothesis as opposed to the opposite of the hypothesis. If we hypothesise 'q', and this leads to a contradiction, then we conclude the negation (and opposite) '$\neg q$'. If we hypothesise '$\neg q$', we would conclude the negation, (and not the opposite) '$\neg\neg q$'.

elimination. Put another way, under the $C\&P$ strategy, given some premises, especially inconsistent ones, we do not countenance the closure of all inferences from the premises since this would be the trivial theory in that language. Since the mathematicians themselves do not consciously avail themselves of the trivial theory, we think it is legitimate to model their practice using $C\&P$. Making note of this just makes explicit some of the philosophical subtleties involved in reasoning in ways that are closer to reasoning 'paraconsistenly' while not having a particular formal representation of paraconsistent reasoning in mind.

We extend $C\&P$ to model scientific *understanding* and problem solving, not just reasoning and making arguments, and we give a more rigorous *characterization* of chunks.[10] In past reconstructions, choosing the chunks was largely a matter of feel, with hints taken from the original proof. The more rigorous characterization we propose here is meted out in terms of bundle diagrams, but it could also be done more rigorously in terms of cohomology theory and sheaf theory (Abramsky et. al., 2015) or in terms of a *pivotal consequence relation*.[11]

The notion of a pivotal consequence relation is used to maximize sets of assumptions or axioms or rules of inference, up to cut elimination. This would be a way of distinguishing chunks from each other. These extensions have not yet been worked out for $C\&P$ explicitly.[12] However, all of these more precise, rigorous, systematic and formal approaches to defining chunks and the permeability relation might *suffer* from being too precise because they would also have to be adapted to general understanding as opposed to reasoning or deducing, and worse, they might be applicable only in certain sorts of proof – those that can be expressed in the respective formal languages. The pivotal consequence relation concept coupled with maximal sets of assumptions up to cut-elimination is limited to cases that we can express in propositions and in terms of clear and explicit rules of inference. Extending the $C\&P$ strategy using cohomology theory or sheaf theory

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[10] It would be nice to make these *maximal*, but to prove that they are might not be possible. Similarly, to give a method for checking for maximal chunks might not be possible. There might be two $C\&P$ reconstructions that have the same number, or size, of chunks.

[11] See the work of (Makinson, 2003, 2005) for the introduction of this concept and Piazza and Pulcini (2016) for the notion of finding the maximal set of assumptions that could then be used, again, to extend the $C\&P$ strategy by using the maximal set of assumptions to define a chunk.

[12] It would make a nice future project to look into the possibility of more rigorously defining the chunks in this way. Moreover, there promises to be some clean ways of working out what information permeates using the definition of complementary sequent and complementary system. See Piazza and Pulcini (2016) for details. We thank Pulcini for the suggestion in private correspondence.

might also be too precise for the purposes of reconstructing some of the reasoning in *science*, although Abramsky et. al. do this for quantum mechanics, but without $C\&P$. While this might work for highly mathematical areas of physics, it would be too precise in cases where we find it difficult to fit the concepts of science to the concepts and language of cohomology theory or sheaf theory. The scientific concepts might not be ready (yet) to be represented in this way. On the other hand, if the scientific theory is amenable to such representation, then it might be quite revealing to work through the $C\&P$ exercise. Generally, the more logical, formal or mathematical a science is, the more amenable it is to a more rigorous extension of $C\&P$.

The bundles that we introduce in the next section are quite flexible and can be thought of in several very different ways. They are suited to representing scientists' more general understanding and reasoning than representing proofs. They are more flexible, but when combined with $C\&P$ give fairly rigorous guidance for individuating chunks. Thus, Bundled $C\&P$ takes us a step beyond the existing guidelines to individuate chunks by 'trying to follow the original intentions of the author of the proof'. They take us a step towards more rigorous directions. In particular: cohomology theory, sheaf theory, and pivotal consequence relation approaches to individuating chunks. We believe that the approach that we introduce here, will warrant, at least, deeper understanding of $C\&P$ as well as understanding of the scientific practice when dealing with inconsistency through separation.

As we can already see, there are both practical and conceptual limitations to our extension of the method. We shall discuss some of them further in the conclusion.

5.4 Bundles: Local and Global Consistency

We are interested in inconsistencies. In particular, in inconsistencies in information being used to reason or understand phenomena in mathematics or science. We are interested in cases that are a little sophisticated: where we do not simply have a formula or sentence as one piece of information and the negation or denial of the (otherwise) same formula or sentence.

In order to present the bundles, we follow Abramsky et. al. and focus on the re-enforced liar paradox, also called 'liar cycles', where one person says of a second that everything he says is true, while the second says of the first that everything he says is false. This is a liar cycle of two. There can be liar cycles of three, four and so on, and because they might be extensive, we might not be certain whether we are in a liar cycle or not because the cycle is too large, or of indeterminate size. This makes the inconsistency more sophisticated.

There is a similar situation with some proofs in mathematics and computer science. In the langage of classical logic: creating a model or making a derivation can influence what other models are then possible. We add more information —a new result from another theory— and the models that satisfy this new information might preclude the first models —this is a cycle of two. The model cycles might be larger, up to indefinitely large. We might not know that satisfying some premises with a class of models precludes our satisfying other premises with the same models.

In the langage of proof-theory,[13] or of constructive logic, deriving a certain theorem might set parameters on what can be derived next, and further down the line. In an informal proof, we might find that by ignoring some of the work we did earlier, we derive something that contradicts what we derived earlier. This is only possible if we are reasoning under suppositions or hypotheses, and the suppositions or hypotheses are important just for a sub-proof. We might not execute the derivation needed to see the contradiction, and so not be aware of the contradictory milieu we are in.

The other places where we see such reasoning is in quantum mechanics, reasoning from inconsistent data sets and so on (Abramsky et. al., 2015: 1). Or, there are situations where a mathematician borrows theorems or results from various theories to prove her conclusion, suspects that she might be flirting with inconsistency, but is, nevertheless, confident (or the mathematical community is confident) that her result stands. For example, there might not be a tight and loyal translation between the theories, and usually not even an equi-consistency proof between the theories. The mathematician then borrows information from other theories that is locally consistent. But if we were to mix all the theories together, we might well be able to derive a contradiction. Moreover, she thinks she is reasoning classically or constructively. Such reasoning is sensitive to context: that it should be local. For this reason, Abramsky et. al. call this 'contextual' reasoning. (Abramsky et.al. 2015: 1) When we develop Bundled $C\&P$, contextual reasoning will be treated as a chunk.

We can represent liar cycles, and similar sorts of reasoning using bundle dia-

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[13] If we are doing formal proof theory, then there is no danger of inconsistency. However, here we are thinking in terms of informal proofs or proofs using suppositions. We move from the model theory story to the proof theory story to respect classical reasoning and constructive reasoning, respectively.

grams from topology. Precisely:

> The key idea is to understand contextuality as arising where we have a family of data which is *locally consistent but, globally inconsistent*. This can be understood and very effectively visualised... in topological terms: we have a base space of *contexts* (typically sets of variables that can be measured or observed), a space of data or observations fibered over this space, and a family of local sections (typically valuations of the variables in the context) in these fibres. This data is consistent locally but not globally: there is no *global* section defined on all the variables that reconciles [makes together consistent] all the local data. In topological language we say that the space is "twisted" and hence provides an *obstruction* to forming a global section. (Our emphasis, Abramsky et. al., 2015: 1).

In Sec. 4.5, We shall extend the bundle diagrams to accommodate other cases, by considering other sorts of variables (base spaces) and other sorts of valuations on those variables. We shall then see how they fit with $C\&P$ to vindicate mathematician's practice of reasoning with inconsistent premises.

Bundles are a type of diagrammatical representation. We shall first construct a simple diagram, showing a consistent set of formulas (figures 2, 3, 4), then we shall show liar cycles of three (figures 5, 6). We shall then widen the cycle to five (figure 7). Next, we change some of the parameters on the bundle diagram to accommodate different sorts of proof; and finally, we transpose this idea to the notion of $C\&P$ as a methodologically tight rational reconstruction of reasoning with inconsistent premises, solve problems or trying to understand scientific phenomena from the point of view of scientific theories that contradict each other.

We introduce the bundel diagrams. An easy bundle diagram for a consistent set of formulas consists in the following. Assuming that everything Aristotle says, Plato says and Socrates says is internally consistent, we make up our *base space* of: (A) everything Aristotle says, (B) everything Plato says and (C) everything Socrates says. Represent this as three points on a horizontal surface. Rising vertically upwards from the points (A), (B) and (C), we draw fibres. See figure 2.1.

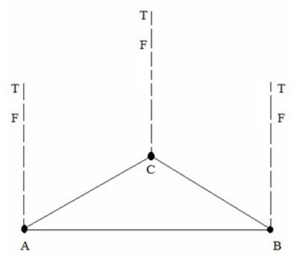

Figure 2.1. Base space.

Along the fibre are two stops: the possible valuations (true or false) of the set of sentences uttered by Aristotle, Plato and Socrates respectively. Let the lower stops represent false and the upper stops represent true. That is, it is possible that everything Aristotle says is true, and it is possible that everything he says is false, similarly for Plato and Socrates.

We add further information. Each of Aristotle, Plato and Socrates utter a special sentence. Aristotle says: everything Plato says is true. Plato says: everything Socrates says is true, and Socrates says: everything Aristotle says is true. Assuming that what all three say is true, this connection is represented by drawing an edge from the T stop, up the fibre from Aristotle, to the T stop, up the fibre from Plato and drawing an edge from the T stop, up the fibre from Plato, to the T stop up the fibre from Socrates. Finally draw an edge from the T stop, up the fibre from Socrates, to the T stop up the fibre from Aristotle. See figure 2.2. Drawing these edges makes a section.

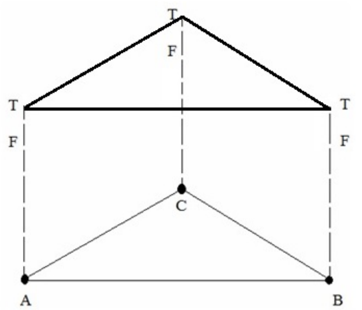

Figure 2.2. Bundle diagram global consistency (all true).

This set of three edges represents the idea that Aristotle, Plato and Socrates say only truths, and that they attribute truth to each other (we ignore the direction of the attribution and the historical veracity of the claims in the base space). More technically, the three edges constitute a *closed path* that *traverses* each of the *fibres* only once (Abramsky et. al. 2015, 7). This represents *global consistency* in what Aristotle, Plato and Socrates say. They could also have all said only falsehoods rather than truths, and attributed falsehood to everything each other says. This could still be a consistent set of sentences. In this case we would have a path connecting each of the F stops up the fibres. See figure 2.3. Global consistency can also occur with a mixture of truths and falsehoods. For example, see figure 2.4.

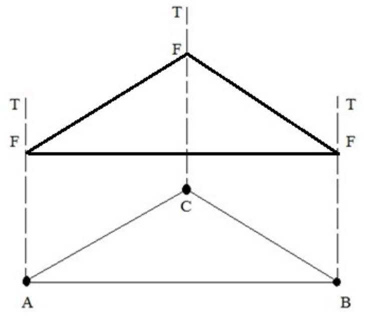

Figure 2.3. Bundle diagram global consistency (all false).

Aristotle might say that everything Plato says is true, but Plato says that everything Socrates says is false, and Socrates says that everything Aristotle says is false, but what he says is false, so the edge goes from the F stop up the fibre from Socrates to the T stop up the fibre from Aristotle. This is globally consistent, so we have a closed path that traverses all the fibres only once. But say Aristotle is uttering a falsehood when he says that everything Plato says is true. Then we have another closed path. See figure 2.4. There are all together eight possible paths traversing each of the fibres only once when we have a base space of three and two values up the fibres. Such a "closed path" is also called a "global assignment" (Abramsky et. al. 2015, p. 7). To introduce more vocabulary: any such closed path (traversing each fibre only once) is also called *univocal* since it assigns one value to each variable.

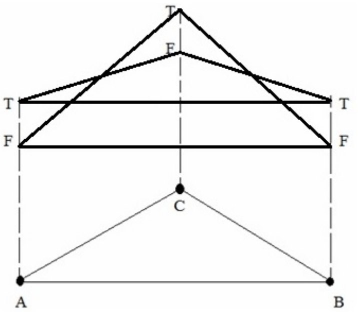

Figure 2.4. Bundle diagram: global assignments

We do not always have global consistency, although we might have local consistency. Let us now consider a liar cycle of three. We have the same base space. The special sentences are the same, with one exception. This time, Aristotle says that everything Plato says is false. The bundle diagram now is given in figure 2.5. If we follow the path made by the edges, starting with assuming that what Aristotle says is true, it will cross the fibres twice.

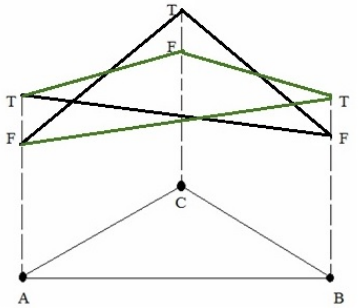

Figure 2.5. Bundle diagram: liar cycle of three.

If we start with the assumption that everything Aristotle says is false, then we have another path, that also crosses all of the fibres *twice*. See figure 2.6.

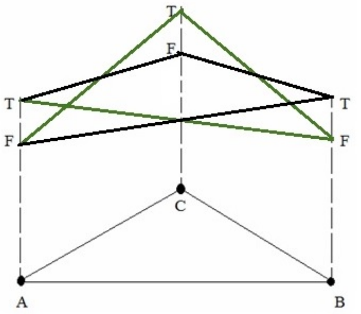

Figure 2.6. Bundle diagram: liar cycle of three.

This is because the variables: (A) what Aristotle says, (B) what Plato says, and (C) what Socrates says cannot be globally true, although any pair is locally, or pairwise, consistent. This is a liar cycle of three. We can expand it to four, five or more. We add Parmenides (D) and Heraclitus (E) between Plato and Aristotle. Plato's special sentence is now "Everything Parmenides says is true", Parmenides' special sentence is "Everything Heraclitus says is true", and Heraclitus' special sentence is "Everything Plato says is false". So this increases the liar cycle. See figure 2.7.

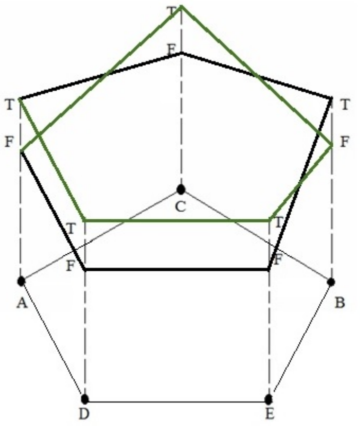

Figure 2.7. Bundle diagram: liar cycle of five.

Here is an extension of bundle diagrams. We now depart from the liar cycle analogy, by saying that instead of truth values up the fibres, we have models. So, we now put on our classical, realist, model theorist hat. Our base space is now made up of formulas that are satisfied by models that are represented by stops up the fibres. It is paraconsistently (note! we shall return to this in the conclusion) possible to have mixtures of formulas whose models are locally consistent but globally inconsistent. In the 'model-extension' of the bundle diagram, the functions mapping models under formula (A) on to models under formula (B) and so on, fail to make a global assignment. There is a class of models that satisfies some formulas but if a particular subclass is satisfied, then some of the first formulas are no longer satisfied by those models, and the fibres are crossed more than once along their path.[14]

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[14] We do not know if this might also be due to a reflexive iteration that causes what Dummett

Changing the vocabulary to the more constructive, proof-theoretic version: instead of models, we can discuss possible inferences from a set of suppositions or hypotheses. If we cannot make a global assignment, then we 'need' (under our respect for the assumptions concerning the practice of mathematics, that mathematicians think of themselves as thinking consistently, classically or constructively) something like $C\&P$ to reconstruct the reasoning.

5.5 Bundles and Chunk and Permeate

Now, because the main purpose of introducing $C\&P$ and bundle diagrams was to formally depict the mathematicians' reasoning when they are using inconsistent premises. First, in the classical mathematical proof case of the reconstruction, the *base space* is the premises and the conclusion. The stops on the fibre represent values for the variables of the theory. The values are either (i) the set of truth values that we can assign to the premises or conclusion, this is all we need if our logic is propositional or (ii) the (open) set of models satisfying the premises or conclusion, we need this for a first-order classical theory or (iii) the inferences that can most immediately be made (under some normal form and ordering of inferences) from the premises and the conclusion. We need this sort of stop up the fibres if we are reasoning in a first-order constructivist theory or proof theory.

A chunk can then be individuated by a *local* path that traverses *some* fibres each only once and traverses no fibre twice. Now we pay attention to the notion of a path being directed. This is not strictly necessary, and in some cases will not be appropriate; but it helps for the description here. Find a path that leads to the conclusion. The conclusion is in the last chunk, the *target* chunk. There will also be some premises or theorems in the target chunk. But the target chunk cannot include all of the premises. Information permeates, so there will be some overlap in information between the chunks. To keep things simple, try to minimize the number of chunks. In fact, it will often be possible to have only two chunks, depending on what information has to permeate to the other chunk. In bundle language: chunks consist in elements of the base space that are together

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calls an 'indefinitely extensible concept'. This might correspond to the idea that we do not know if we are in an inconsistent cycle, so the edges might spiral up, but we have no way of knowing at any one point if we might then be brought down again. A bundle diagram where the edges spiral upwards indefinitely might represent something like a fractal where a new value is generated as a result of both the formula and the last value or last few values. This is all speculation that requires further investigation. We thank Jean-Paul van Bendegem for asking about spiralling edges.

consistent. So, as a first approximation, we individuate chunks as the base space below an edge that does not cross itself. Because some information permeates, there will be elements of the base space that find themselves in more than one chunk. That is what permeates.

Let us be more precise about what permeates. As we saw with the bundle representations, pairs of variables in the *base space* are locally consistent iff there is an edge between them. Following the bundle representation, we allow permeation and individuation of chunks in a 'back-and-forth' play between: on the one hand wanting to maximize chunks by taking the longest locally consistent assignment, and on the other hand, letting only consistent-with the-next-chunk-and-used-in-the-next-chunk information to permeate to the next chunk. So, now we are not maximising, but optimising between two considerations. A given maximal chunk might have to be made smaller, for reasons of permeation. There is some artistry here. This is a casualty, or strength, of our giving more specific guidelines than were hitherto available, while not wanting to make too formal and rigid the specifics of the bundle diagram. Situations that are amenable to more formal representation can be given a more effective recipe for individuating chunks and determining permeation.

Under our guidelines, we might break up our premises into sub-premises thus changing the base space. This corresponds to weakening axioms or splitting axioms into two. For example, in our liar cycle we could separate the special sentence from the quantified sentence (which would not include the special sentence, so the quantifier is bounded in an odd way!). Ignore this possibility, since it makes the notion of 'maximizing' or even 'optimizing' more complicated. It is because of such added complications that the method we propose here is not effective.

5.6 Generalizing Further: Bundled Chunk and Permeate to Reconstruct *Scientific* Reasoning

We generalize further. In mathematical reasoning especially today, we are fairly clear about what our premises are, where borrowed lemmas and borrowed theorems come from and what our concluding theorem is. In science, these matters are not always so clear. Moreover, we might be reasoning, not in the sense of deriving a theorem, albeit informally, but in the sense of reasoning about a concept, a phenomenon or a structure and so on, in order to deepen understanding rather than come to conclusions of deductions. Regardless of the difference in purpose, we might still be concerned about reasoning in an inconsistent context, where the ideas we bring to bear, in order to deepen our understanding, contradict each other under some representations in a formal language.

We shall work through an example very soon. Staying at the very general level for now: to deploy the *Bundle Chunk and Permeate* strategy (henceforth, *BC&P*) for reconstructing, or even guiding the future reasoning, we start with deciding on defining the *base space* of contexts. For *BC&P*, *contexts* will be sets of premises, ideas, theorems, results, data from observations, or descriptions of phenomena which are jointly used when solving specific problems;[15] these sets will often coincide with the chunks (if already specified).These premises/ideas/theorems/results could be of two types: context–dependent, their interpretations and constraints are determined by the context in which such premises are being evaluated, or context-independent, their interpretations and constrains are given independently of which other premises are being evaluated (and, sometimes, the premises' value may be fixed and seem self-evident to scientists).[16]

We then have to ask a very fundamental question about valuation, in order to determine what we shall find going up a fibre. Valuations might be measurements, or they might even be qualitative, in the form of properties. An edge will connect values in adjacent fibres when the corresponding joint outcome is possible (Abramsky et al. 2015, p.7). A global assignment will be indicated by a closed path traversing all the fibres only once. A contradiction could be –partially- pictured by assigning two *mutually incompatible* values to the same premise/idea/theorem/result. But that is not enough, to show the presence of global inconsistency, we need to show a twisted segment in topological language (Abramsky et al. 2015, p. 1-2). As it has been described in (Abramsky et. al. 2015), such a twist will help us to visualize the lack of a global assignment and the presence of mutually incompatible contexts.

We have identified at least four types of path over a global base space:

1. An open path that does not cross all the fibers. This indicates nothing about the global base space.

2. A closed path that crosses all fibers only once. This indicates a global assignment and with it, the possibility of consistent reasoning in the global

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[15] For related notions see Abramsky et. al. 2015, p.1.
[16] For related notions see Abramsky et. al. 2015, p.6.

theory.

3. A closed path that crosses some or all fibres twice or more. This indicates that the valuations change as the reasoning along edges is carried out: this shows contextual reasoning, but not necessarily a contradiction. There is just a feed-back loop (recursive reasoning) that changes the valuations next time that you consider the base information. This is enough for identifying *logical contextuality.*[17]

4. A closed path that forces you to cross at least some of the fibers twice (or more). The segment carved out by the edges makes a twisted space: one where there is inconsistency in the global base space. To force the crossing twice there has to be a cross-over *between* fibers. This shows that the base space contains a contradiction. It shows global inconsistency.

In the following sections we present an application of *BC&P* for modelling inconsistent reasoning in empirical sciences. In order to do so, we shall first provide some definitions so as to make it easier to understand a particular case of inconsistent science and the particular application of *BC&P*. Then, in Sec. 5.6 we shall introduce a case study from nuclear physics. And finally, we shall proceed to illustrate how *BC&P* could give a satisfying account of this particular case.

5.7 Some Preliminaries from Empirical Inconsistent Science

While paradoxes and internal inconsistencies in the more formal sciences are well-documented in the literature and have called the attention of many paraconsistent logicians; inconsistencies from empirical sciences (in particular, inconsistencies between theories or models)[18] have not enjoyed as much attention. Some exceptions are mentioned here. We think that the presence of some contradictions

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[17] Characterized in Abramsky et al. (2015) as: "there is a local assignment which is in the support, but which cannot be extended to a global assignment which is compatible with the support." (2015, p.6).

[18] Schummer (2015, pp. 64–5) argues convincingly that, especially when considering problems in chemistry, we use the term 'model' rather than 'theory', since this better reflects the practice of chemists when reasoning about phenomena in chemistry. Of course, here we do not mean model in the model theory sense of the term.

in scientific reasoning is not a minor issue; so, if a case of inconsistent (non-trivial) science is spotted, it seems necessary to offer an explanation about how inconsistent information can be combined and not become trivial in the empirical sciences. We believe *BC&P* can help us to achieve such an explanation or reconstruction of the reasoning.

5.7.1 Different Groups of Propositions

In the empirical sciences, the different disciplines and research domains are never completely independent of each other (Laudan 1977: 53). As a matter of fact, more often than we expect, in actual scientific practice different theories (from different disciplines) and different models (from different theories) are often combined for solving specific problems. Some problems are complex enough that they cannot be clearly solved by one theory or model alone.[19] We shall focus mainly on inconsistencies that involve two or more different —original— groups of propositions.

That being said, a natural question emerges: how can scientists individuate groups of propositions as 'distinct theories' or 'distinct models'? This is not a trivial question. When we individuate objects we give the necessary and sufficient conditions under which we are able to tell when two objects are different from each other, and when, what we thought were two distinct objects, turn out to be the same one. Famous examples include the discovery that the evening star is the morning star or that 'jade' is really two different chemical compounds: now called 'jade' and 'jadeite'.

Nonetheless, individuating scientific theories along the sorts of standard lines we would use to individuate mathematical theories is disingenuous towards scientific practice.[20] When talking about scientific theories the challenge is double: it is difficult to say when a set of objects, substances or ideas is different from another set of objects, substances or ideas, and it is also difficult to specify when two set of objects, substances or ideas are part of the same scientific theory (Needham 2015). As a matter of fact, the history of philosophical and scientific debates has shown that sometimes

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[19] For examples of this see Elsamahi (2005) and Morrison (2015).

[20] If we were to be normative, or even prescriptive, about science we could disregard scientific practice and force individuation of scientific theories in order to avoid inconsistency within a scientific 'theory'. We do not propose to do this, since, as we shall see, this would be quite unnatural to the practice, and of rather limited interest.

> we can't agree on which set of 'things' constitute 'Newtonian cosmology', 'classical electrodynamics', and the rest. We see in (...) particular examples of disagreements about whether some theoretical constituent (equation/model/proposition) should or shouldn't be considered 'a part of the theory'. (Vickers 2014, 2892).

Such disagreements are hostage to the abstract activity of theory individuation. When philosophers or scientists ask themselves if a particular scheme is 'really the theory X', there is often miscommunication (Vickers 2013 chap. 2, 2014). However, if they ask themselves which are the theoretical constituents that are sufficient to solve a particular problem given certain constraints, agreement is reached more easily.

Taking that into account, we take a naturalist stance, and observe that often in actual scientific practice, scientific theories are not individuated abstractly, but in terms of specific problem solving goals. Here we shall claim that a theory (or model) will be successfully *individuated according to a particular problem*, if the set of propositions that constitute such a theory (or model) entails a solution to the problem, or a statement of the problem as well as a neat, or systematic understanding of it.[21] Once a theory is individuated, it could be studied by analyzing it *globally*, this is, through the revision of the properties that the whole theory possesses; or it could be studied through the analysis of some of the properties that only some of its subsets possess.[22]

A theory could be separated into *meaningful subsets* if and only if the elements contained in such subsets are considered to be sufficient for solving interesting problems in the discipline to which they belong —if they are too minimal for solving problems, we will not consider the separation to be a candidate for being a chunk. The study of the properties that meaningful subsets of the original theory possess is what we understand as *local* analyses. The properties that are present in meaningful subsets of a theory, are not always present in the theory as a whole. For instance, a theory could be locally consistent, i.e. could have consistent subsets, without necessarily being *globally* consistent.

A particular theory (or model) A will be distinct from another theory (or model) B according to a particular problem if and only if the solution of the

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[21] See (Laudan 1977) for related notions of *problem solving.*

[22] Even though almost any scientific theory could be fragmented in infinite ways, here we shall focus only on such subdivisions that are compatible with the way in which scientists use their theories in their standard practice. Henceforth, we shall refer to this way of choosing chunks as separating it into *meaningful subsets.*

problem that is entailed by the theory (or model) B cannot be achieved without the theory (or model) A.[23] Two distinct theories (or models) could be *satisfactorily combined* if and only if they are distinct theories and if their combination allows for larger explanatory or predictive power, solving other problems or contribute to greater understanding, than the one that each theory alone possesses.

In sum, we consider that the relation between a particular group of propositions and the solution to a specific problem are necessary for successfully individuating theories (or models).

This way of individuating theories is not only loyal to the practice, but it will be useful for separating groups of propositions when applying $BC\&P$ to particular cases.

5.7.2 Global and Local (In)Consistency

As we claimed in the Introduction, scientific inconsistent theories have often been analysed at two scales of analysis: global and local. What interests us here are cases where the global theory, or model is inconsistent, but sub-theories or models are consistent.

While we take a 'naturalist stance' in the sense of respecting the scientific practice and allowing it to guide our analysis on the relations between different theories and between different models, we should note that respecting the practice comes at a price: many of the scientific theories that are built and individuated under problem solving considerations are, at some point in their development, inconsistent. For example, Bohr's theory of the atom was initially designed for explaining why hydrogen emits and absorbs light at certain specific frequencies and, since the beginning, the theory succeeded at its main goal. However, despites this success, the early versions of the theory were inconsistent (Fowler 1913, Brown and Priest 2015).

As a matter of fact, the list of theories that (allegedly) have been inconsistent is long and diverse, some examples of inconsistency in science are Bohr's theory of the atom (Fowler 1913, Brown and Priest 2015), the Early Calculus (Berkeley 1734; Lakatos 1956, 59; Feyerabend 1978, 158), Classical Electrodynamics (Frisch 2004, 2005), Prout's hypothesis (Priest 2002), the models of the atomic nucleus (Morrison 2015), among others.

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[23] In what follows, we shall assume that scientific theories are often individuated following specific problem solving considerations, and that this individuation is often in terms of objects, sets of phenomena, sets of forces acting together, or classes of axiomatic theories, among others.

Although all those case studies aim at illustrating inconsistent scientific theories, some logicians and philosophers of science have pointed out that the inconsistencies that have been portrayed by these cases are not really homogenous (Laudan 1977, Priest 2002, Davey 2014, Martínez-Ordaz 2014). As a matter of fact, "if we distinguish between observation and theory (what cannot be observed), then three different types of contradiction are particularly noteworthy for our purposes: between theory and observation, between theory and theory, and internal to a theory itself." (Priest 2002, 144).

These differences play a crucial role in the philosophical analyses of inconsistencies in the empirical sciences. Nevertheless, here we are analysing inconsistency in science as a logical concept.[24] We shall gloss over the differences by focusing on sets of sentences or formulas. Thus, the sets of sentences might be about observations and theory, might belong to 'different' theories or might all belong to a theory. Since we are interested in representing the inconsistencies using the bundle diagrams, the sets of sentences are our variables. They make up the base space.

We then focus on the distinction between local and global. These could be characterized as follows. Given a specific problem X, and two different groups of propositions,[25] a and b,[26] that are put together to provide a solution for X:

- a is locally consistent if and only if a does not contain nor entail a contradiction.
- b is locally consistent if and only if b does not contain nor entail a contradiction.
- The union of a and b is locally consistent if and only if the union does not contain or entail a contradiction.[27]

While in the sciences (formal and empirical) it is often expected that the union of two locally consistent sets of information is still consistent, because true and about the world, this is rarely the case. However, we shall show that it is not

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[24] For our bundle diagrams these differences would be drawn out by our choice of variables: be they observations, theories or ideas within a theory.

[25] The propositions could be empirical assumptions, observational reports, laws, theorems, axioms, etc.

[26] Here a and b could be either distinct theories or distinct meaningful subsets of the same theory.

[27] Of course, the union of two locally consistent sets of information need not be consistent with each other.

as dangerous as has been traditionally thought (Popper 1959, Hempel 2000). In what follows, we shall provide a case study from nuclear physics where we combine two theories or models, that each is internally consistent, but their union is inconsistent. Moreover, the union is needed for having a more complete understanding and to solve some problems in science.

5.8 A Scientific Example of Bundled Chunk and Permeate

The first case studies that were modelled by using $C\&P$ were cases of internal inconsistency, but recently, an example of a different kind has been provided as candidate for using $BC\&P$. The example is the combination of two mutually inconsistent climate models that allow for accurate predictions regarding temperature, pressure, humidity and other meteorological quantities (Brown, 2016, 2017). We consider it important to emphasise the fact that scientists very often make use of mutually contradictory bodies of knowledge in order to solve problems in their discipline, here we shall introduce a similar case study from nuclear physics. We chose to present this particular case taking into account three main goals: to introduce a new case of inconsistency toleration in empirical sciences (Sec. 5.8.1), to illustrate an application of $BC\&P$ (Sec. 5.8.2), and also, to draw some philosophical conclusions about inconsistency toleration and the unification in science (Sec. 5.9 and Sec. 5.10).

5.8.1 The Case Study

In a nutshell, the case study goes as follows: the Liquid Drop Model and the Shell Model contain incompatible basic principles regarding the structure of the nucleus of an atom; it is only when nuclear physicists combine some of the predictions of both models that they gain accuracy in their predictions and measurements of binding energies for all the chemical elements of the periodic table and in their predictions and explanations of other nuclear processes such as fission. This case study illustrates a scenario in which each model can accurately predict only a segment of the elements in the periodic table and only part of a general phenomenon, but in which combining the predictions of both models provides successful descriptions and predictions of more general phenomena.

First, the nucleus of an atom is the small region in which 99.9% of the total mass of the atom is located. The nucleus consists in protons and neutrons bound together. The protons are responsible for the positive charge of the atom. The behaviour of the nucleus is explained by appealing to two different forces: the strong nuclear force and the weak nuclear force. The strong nuclear force is what binds nucleons (protons and neutrons) into atomic nuclei, while the weak force is responsible for the decay of neutrons to protons. Any atomic nucleus (of any chemical element) will exhibit binding between protons and neutrons and decay of neutrons and protons.

The binding energy of a nucleus is what in large part determines the stability of the nucleus. Ideally, binding energies are necessary for understanding and determining under which conditions a nucleon can change to another (from neutron to proton, for instance) or escape from the nucleus. Considering that binding energies are necessary for predicting and describing different aspects of the nuclear structure (for instance, correlations present in the nuclear ground state (Fossion et. al. 2002)) physicists have tried to come up with a homogeneous theoretical framework to calculate this type of energy.

Our current nuclear physics provides us with models of features that allow us to, at least, describe, predict and measure this type of behaviour of atomic nuclei. Such models have been achieved by different research programmes that have a main goal in common, namely: to provide some insight into the structure and dynamics of atomic nuclei. Today, there are 31 different successful and internally consistent nuclear models that offer some insight into the nucleus of the atom (Cf. Cook 2006, Morrison 2015). These models are often classified into three main groups: microscopic models (focused on nucleon-nucleon interactions), collective models (focused on bulk properties of the nucleus as a whole) and mixed models (which are somewhere in between the two previous ones).[28] However, as yet, there is no consistent or coherent global account of the structure of the nuclei that allows us to explain, predict and measure all nuclear behaviours.

The diversity of models itself is not problematic; especially if "each model has its particular successes, and together they are sometimes taken as complementary insofar as each contributes to an overall explanation of the experimental data" (Morrison 2015: 179). However, the case study that we are presenting here, illustrates how the basic assumptions required by one model contradict those required by another model (Cook 2006; Morrison 2011, 2015), more important, none of these conflicting assumptions seems to be idle, and they all are, allegedly, strongly linked to success in particular applications of each model (Morrison 2015). Let us press this point further by describing two such mutually incompatible nuclear

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[28] This classification was first developed in (Geiner and Maruhn 1996), and later in (Cook 2006).

models.

The first of these two models is the Liquid Drop Model (*LDM*). It is one of the most successful nuclear models. The LDM was formulated more than 80 years ago under the assumption that the nucleus of an atom exhibits classical behaviour (protons and neutrons strongly interact with an internal repulsive force proportional to the number of nucleons). The model was based "upon the experimentally established dependence of total binding energy of a nucleus upon the number of nucleons. As expected for a liquid, the nuclei proved to be almost incompressible and their total binding energy included a negative term, proportional to the volume of a nucleus, and a positive term, proportional to its surface" (Amusia and Kornyushin 2000: 219). Since the beginning, the *LDM* could predict and describe a series of nuclear properties, such as the growth of the nuclear charge, the instability related to Coulombic forces, the evaporation of nucleons after heating, the nucleus' change of shape, and the phenomenon of spontaneous fission, among others (Amusia and Kornyushin 2000, Cook 2006, Morrison 2015).

However, despite its success, the *LDM* fails to describe the way in which the nucleus often displays distinctive energy levels forming shells and subshells (the so-called shell effects)-it also fails to give a full account for the ground-state properties of nuclei (Groote, Hilf and Takahashi 1976; Amusia and Kornyushin 2000). Additionally, "the quantitative description of the nuclear force that emerges from nucleon-nucleon reaction studies is incompatible with what is known about nuclei" (Morrison 2015, 178). Finally, while the *LDM* can be used to predict and describe binding energies of nuclei of any element of the periodic table, in the corresponding experiments, some nuclei show systematic deviations with respect to the *LDM* predictions. Experimentation has shown that some nuclei are bound more tightly together than predicted by the *LDM* depending on the number of nucleons that they possess. To explain this phenomenon, scientists refer to the so-called 'magic numbers'. The phenomenon can be detected in the nuclei of atoms of, at least, Helium (He), Oxygen (O), Calcium (Ca), Nickel (Ni) and Lead (Pb).

Nonetheless, the partial failure of the *LDM* does not mean that scientists are left empty handed. When dealing with the phenomena that the *LDM* cannot describe, physicists often rely on other models, one of the most important is the Shell Model (*SM*). This nuclear model was formulated more than 70 years ago and aims at describing and predicting, among other nuclear properties, the shell effects of the nuclei. In this model, a shell represents the energy level in which particles of the same energy exist, and so, the elementary particles are located in different shells of the nucleus. According to the *SM* the nucleus itself exhibits quantum-mechanical behaviour. “The basic assumption in the nuclear shell model is that, to first order, each nucleon (proton or neutron) is moving in an independent way in an average field” (Heyde 1994: 58); that is, for this model “nucleons are assumed to be point particles free to orbit within the nucleus, due to the net attractive force that acts between them and produces a net potential well drawing all the nucleons toward the centre rather than toward other nucleons.” (Morrison 2015: 185). One of the most important virtues of the *SM* is that it accounts for the magic numbers phenomenon, among other important experimental data.

Considering the diversity of models and the obvious conflicts between them, nuclear physicists’ have untiringly attempted to combine both microscopic and collective models in order to provide a unified framework of the behaviour of the nucleus (Cook 2006). Common manoeuvres have been related to the combination of elements from the *LDM* with elements from the *SM* (Cf. Groote, Hilf and Takahasi 1976; Amusia and Kornyushin 2000, Cook 2006; Fossion et. al. 2002), however, the success of any of the attempts is still unclear.

A large number of nuclear physicists agree that “material systems such as nuclei are too complex and contain too many constituents to be handled precisely with formal “bottom-up” theories, but they are too small and idiosyncratic to be handled with rigorous statistical methods that normally require large numbers to justify stochastic assumptions” (Cook 2006, 57), and in that sense, even if endorsing unificationist commitments, physicists take for granted that nowadays there are some scientific problems whose solution requires the use of more than one nuclear model. For instance, the calculation of binding energies of all elements of the periodic table; which, for accuracy, requires the use of the *LDM* for almost all the elements, and to use the *SM* for those nuclei with magic numbers. An important remark: to provide accurate predictions concerning binding energies is not an idle task for nuclear physicists, especially in light of the privileged role that such energies play when describing, calculating and explaining nuclear processes such as fission.

So, if they want to address the domain of binding energies of all the chemical elements, physicists have to agree that at present there is no single direct way to calculate them; instead, we have to use two mutually contradictory models, each one of them accurately predicting only a segment of a general phenomenon. The contradiction involved is even more troubling when we consider that both models contradict each other about the structure of the nucleus, and that such characterizations of the nucleus are, allegedly, what is largely responsible for the success of each model in particular applications (Morrison 2015, Chap. 5).

Nuclear physicists use both models, LDM and SM, to calculate the binding energies for all elements of the periodic table; later on, they use such results for predicting nuclear reactions such as fission. They calculate binding energies of the nuclei with magic numbers using the SM and (for simplicity) use the LDM for the rest.

If scientists want to reason classically or constructively, and avoid triviality when solving these problems, they either have to get rid of some basic assumptions of specific models (by deciding that they are in fact idle, for instance), or they have to find a way to connect the consequences of both models without allowing explosive reasoning. Insofar as what has been said here is correct, this example from nuclear physics is a good candidate for being modelled by $BC\&P$.

5.8.2 Nuclear Physics and $BC\&P$

For simplicity, here we shall only illustrate the case of nuclei of Helium-4 ($He4$). First, the individuation according to a particular problem goes as follows: the *problématique* that requires explanation is the behavior of the atomic nucleus, in particular, the phenomenon of fission of nuclei of $He4$. The theoretical constituents that are sufficient for solving that problem are the LDM and the SM. For, atoms of Helium-4, we also include information about how $He4$ is one of the nuclei with magic numbers, as well as the fact that the nucleus of $He4$ is identical to an alpha particle.

The basic assumptions of the *Liquid Drop Model* we need are, at least, the following:

($D1$) The nucleus behaves as a classical fluid consisting in protons and neutrons that strongly interact with an internal repulsive force proportional to the number of protons.

($D2$) Nucleons move randomly and bump into each other frequently.

($D3$) The nucleus itself exhibits classical behaviour. (Cf. Chen 2011, Morrison 2015)

(Dn) The *semi-empirical mass formula*:[29]

$$E_b(MeV) = a_v A - a_S A^{\frac{2}{3}} - a_C \frac{Z^2}{A^{\frac{1}{3}}} - a_A \frac{(A-2Z)^2}{A} + \delta(A, Z)$$

(Dc) The LDM-predictions regarding fission of $He4$ nuclei.

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[29] The formula is based on the DLM and is used to predict binding energies of nuclei. It is also called “Weizsäcker’s formula”.

The basic assumptions we need from the *Shell Model* are, at least, the following:

($S1$) The nucleus exhibits a quantum-mechanical behavior.

($S2$) The atomic nucleus is a quantum n-body system.

($S3$) The nucleus is not a relativistic object and its equation of motion (the system wave function) is the Schrödinger equation.

($S4$) The nucleons interact only via a two-body interaction which is, in effect, a practical consequence of the exclusion principle.

($S5$): The nucleons are assumed to be point particles free to orbit within the nucleus, due to the net attractive force that acts between them and produces a net potential well drawing all the nucleons toward the centre rather than toward other nucleons. (Cf. Morrison 2015).

(Sc): The SM-prediction of the $He4$ binding energy,

We might think that we should make two chunks, one for each model with the common information permeating from one chunk to the other, but in standard explanations in nuclear physics, we more naturally find four source chunks:

- *Einput*: contains the empirical data about $He4$ nuclei, including that it has a magic number.
- LDM: contains the assumptions of the LDM, $D1$,$D2$ and $D3$ and Dn. This chunk will *grow*, as we let in data contained in *Einput* and obtain as a result the LDM-predictions regarding fission of He4 nuclei (Dc).
- SM: contains the assumptions of the SM, $S1$,$S2$,$S3$,$S4$ and $S5$. This chunk will *grow*, as we let in data contained in *Einput* and obtain as a result the SM-predictions if the $He4$ binding energy (Sc).
- *Exp*: contains the experimental reports on binding energies of He4. Of *Einput* and *Exp* chunk, one is (locally) true whenever the other is (locally) true, and they are always assumed to be so.

Our $BC\&P$ reconstruction recognises also one target chunk:

- *Eoutput*: contains the empirically adequate predictions concerning binding energies and fission of He4 nuclei.

That considered, the base space of our $BC\&P$ includes four source chunks (*Einput*,*LDM*,*SM*, *Exp*) and one target chunk (*Eoutput*). In addition, along each of the fibres are four stops, which represent the possible valuations considering the two main goals: first, to calculate the binding energy of He4, and second, to calculate fission for the $He4$ nucleus. The first two stops indicate if the statement that is evaluated is considered to be true (*Tb*) or to be false (*Fb*) when calculating the binding energy, the second pair of stops, indicate if the statement is assumed to true (*Tf*) or to be false (*Ff*) when predicting fission[30]

Now, when pursuing the target chunk, it is necessary to first determine the binding energy of $He4$ nucleus. In order to do so,we first assumed that the sentences contained in *LDM* are false, and then assume that the data from SM is true.We start to construct our bundle diagram to represent this. See Fig. 2.8.

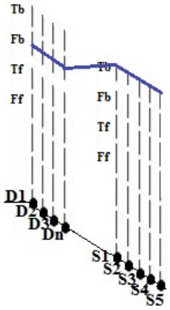

Figure 2.8. Bundle diagram: basic assumptions of *LDM* and *SM*.

That this is compatible with *Einput* being true –especially considering that *Einput* includes the concept of $He4$ having a nucleus with a magic number. See Fig. 2.9.

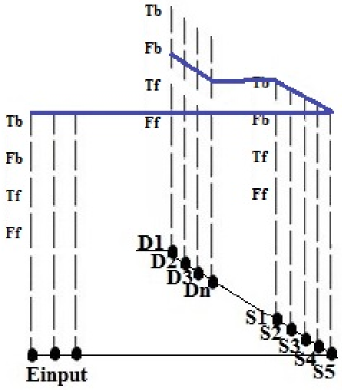

Figure 2.9. Bundle diagram: adding empirical input.

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[30] Note that Tb and *Fb* are mutually exclusive, and the same goes for *Tf* and Ff . Nonetheless, the following pairs are mutually compatible: *Tb* and *Tf* , *Tb* and *Ff* , and Tf and *Fb*.

Due to the falsity of LDM, and the assumption of sentences in SM being true, it is according to our scientific reasoning, allowed to combine SM with $Einput$ to obtain Sc –which is taken as true. See figure 2.10.

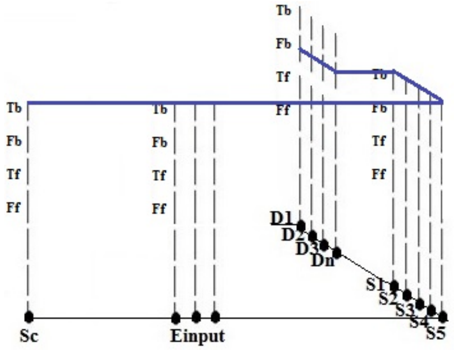

Figure 2.10. First result: Binding energy.

As Sc coincides with what is contained in Exp, and so, what is expressed by Sc we are then is allowed to move to the target chunk, $Eoutput$. See figure 2.11.

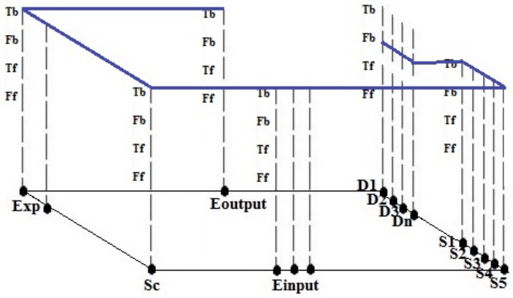

Figure 2.11. Contrasting with the experimental results.

Once the accurate prediction of the $He4$ binding energy is available, the next step is to explain and predict fission for $He4$ nuclei, and for that, physicists will use the LDM. Thus, the sentences in the LDM chunk are taken as true. See figure 2.12.[31]

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[31] Note that we have changed the color to indicate that we have moved to the next step in the calculations involving nuclear fission for $He4$ nuclei. As it is in scientific reasoning, the edges have direction, Sc has to be moved into $Eoutput$ before it is possible to make any prediction regarding nuclear fission. This is new (to the bundle diagram construction) but it is inherent to standard scientific reasoning.

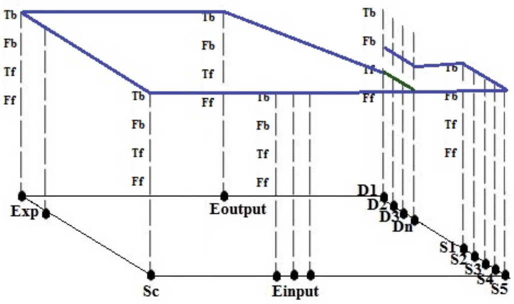

Figure 2.12. Contextuality and the beginning of the second stage.

Figure 2.12 is a representation of a base space that includes: propositions of each model and predictions of each model regarding nuclear reactions of $He4$ nuclei, as well as a general description of the phenomena of fission and binding energies (regarding $He4$ nuclei). We need the whole base space to predict fission for $He4$ nuclei. There is an edge between variables when they can be used together in order to enable measurements of binding energies. The fact that there is no closed path traversing the fibres only once connecting particular base space points, such as *Eoutput* and $D1$, shows the *logical contextuality* of the model. It shows that there is no global assignment that allows for recuperating the phenomena of binding energies and nuclear fission as a whole: for instance, when $S1$-$S5$, Sc and Exp are true of the phenomena, $D1$ to $D3$ and Dn cannot be true. However, at the end, LDM-assumptions and SM-assumptions are both necessary for giving an account of the general phenomena of fission for $He4$ nuclei.

Finally, once again, what is in *Einput* is taken as true. And because LDM is true, SM is taken as false.[32] LDM is combined with what is in *Einput*, and it is possible to obtain Dc (predictions about fission). Due to the compatibility between *Exp* and *Dc*, *Dc* is allowed to flow to the target chunk, *Eoutput*. Now, in *Eoutput* nuclear physicists have both the predictions of binding energies for $He4$ nuclei as well as the ones for nuclear fission for such atoms.[33] See figure 2.13.

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[32] *Sc* is kept as true because it is compatible with the empirical assumptions (*Einput*), the experimental reports (*Exp*), and the LDM-predictions for fission, and also because it is now part of the target chunk.

[33] DLM-assumptions are next taken as false in cases in which the next move is to predict other properties of nuclei, such as spin and parity of nuclei ground states.

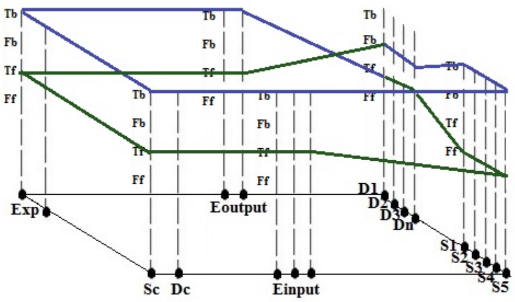

Figure 2.13. Bundle diagram: binding energy + fission.

What the bundle diagram has shown is revealing in more than one sense. First of all, it allows us to see that even though the target chunk, *Eoutput*, can be somehow constructed, it only happens in a very conceptual and abstract sense, where we assume that some information used for the construction is false ($D1$ to Dn) or ($S1$ to $S5$). The twist between Dn and $S1$ shows logical contextuality and the presence of only logical contextuality.

In addition, the diagram can also suggest which chunk is fully compatible with which other chunk (for instance, *Einput* is fully compatible with SM and with LDM, but it is also fully compatible with *Exp* and with *Eoutput*) and also suggests in which cases information ought to be filtered (for instance the fact that SM and LDM are mutually contradictory and they both still feed the target chunk, clearly suggests that only limited information should be allowed to move from such chunks to the target one).

Finally, the use of *BC&P* diagrams can also help us to see how each of the nuclear models is locally consistent. When looking at the areas in the diagram that correspond only to each model, we can identify a local assignment that corresponds to a closed path traversing all the fibres over that part of the base space exactly once; and the same happens for the shell model. However, as should be clear to the reader, here, the local consistency comes at the price of the impossibility of predicting both binding energies and fission for atoms of a certain type (those with magic numbers). Nevertheless, nuclear physicists do successfully make such predictions.

5.9 Problems with Global Inconsistency in Science

There are two related problems. One is that it has long been presupposed, especially in the more Western scientific traditions,[34] that *good* reasoning should be consistent (Popper 1959, Hempel 2000, Davey 2014). The other problem is that it is often assumed that, in the long run, our best science can be, and *should* be unified into one body of knowledge; where unification presupposes consistency. One can find several research projects that are in line with this particular pretension, for instance, the current project for unifying the four fundamental forces (da Costa 2000). Call the first 'the meta-logical problem' and the second 'the unification problem'. The second presupposes the first.[35]

Put another way, the first problem is that if we were to meet a contradiction in our science, then reasoning would be impossible. We inherited this meta-logical idea at least from Aristotle (Priest, 2006) if not from before. We call this presupposition '*meta*-logical' because it concerns the limitations of logic. We call it a *presupposition* because there are perfectly rigorous formal systems of reasoning that include contradictions as features, so it is unnecessary. chunks consist in elements of the base space, but only up to a cross-over between edges

Why are contradictions so detrimental to reasoning in science? Rehearsing what we learned before in Sec. 5.1, if we are classical or constructive reasoners, then we endorse *ex contradictione quodlibet* reasoning as *valid.* Assuming classical or constructive reasoning in our science, in the face of a contradiction, we have explosion. Explosion in a theory means that every sentence written in the language of the theory, or every formula written in the language of the theory is true, or is derivable. So if the langage contains some form of negation or denial, then a sentence and its opposite are both true, or both derivable. This is what we call trivialism. The problem with trivialism is that it is undiscerning. There is no error and so no correction possible. Anything goes (within the constraints of the language).

What does this mean for science? In a science this might well also include observation sentences. So, we might observe that the temperature indicated on the thermometer is roughly zero, but also that it is one hundred degrees, or

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[34] Arguably, in more Eastern traditions of 'science', contradictions are tolerated, (Garfield: Engaging Buddhism). Such Eastern 'science' might not be recognised to be science at all *a priori* because reasoning with contradictions is *a priori* impossible.

[35] Of course, it does not meta-logically have to, but it happens to.

roughly seventeen degrees, all at the same time and in the same situation. This makes nonsense of our ‘science’. Of course, note that this is all qualified by the antecedent of the conditional of the second sentence of this paragraph: that we are classical or constructivist reasoners.

Turning to the second problem: it is that we have an ideal towards which we strive as scientists, and this is to unify science. What does this mean? Philosophically, it is presumed[36] that the real world is a ‘unified’ place, and this intimately includes the presupposition that ‘reality’ is not contradictory.[37] To our delight we have also found that our scientific theories ‘work’ and that we have tangible progress in science. This falls in line with the ‘cumulative retention’ tradition in philosophy. That is, science serves us to predict, explain and control our natural environment. So, it is in this sense that we have ‘success’ in science.

> [V]irtually all models of scientific progress and rationality (with the exception of certain inductive logics which are otherwise flawed) have insisted on wholesale retention of content or success in every progressive-theory transition. According to some well-known models, earlier theories are required to be contained in or limiting cases of, later theories; while in others, the empirical content or confirmed consequences of earlier theories are required to be subsets of the content or consequence classes of the new theories. (Laudan 1981)

Under this ideal conception, our individual scientific theories represent parts of the unified (consistent) reality. It then follows that insofar as out theories reflect reality, they *should* be consistent, not only within themselves but also with each other. The unity of science consists in the global scientific project of making one consistent theory that predicts and explains the whole of our natural world. So, rather than separating the liquid drop model from the shell model of the nucleus of an atom, we should be able to seamlessly reason from one to the other without meeting contradictions. Of course, to arrange for this seamless reasoning, we would have to alter the theories. Under a unified science, the distinctions between theories would then be a matter of history and convenience; both conceptual and institutional. A unified theory of the whole of scientific reality would consist in one set of laws from which we would derive natural phenomena given some initial data.

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[36] We shall show exactly why this is a presumption, and on what it rests.

[37] For a short discussion of this issue where the possibility of a some-places contradictory real physical world, see Friend (2017).

In the concluding section, we shall question the presumptions made concerning the unity of science, but for now, we recognise it as intrinsic to some of the practice of science. An exception is chemistry (Schummer 2015). Recognising the ideal of science, that it should eventually be unified, we can see the problem if we find contradictions in science. If there are contradictions within and between our theories, then this, by definition, impedes unification. To think of contradiction as such an impediment depends on the presumption that led to the first problem.

5.10 Final Remarks

We have been trying to 'make sense of' mathematician's reasoning with inconsistent premises. 'Making sense', here, means that we want to make a rational reconstruction under the pressure of scepticism that such reasoning is illegitimate. Such pressure arises when a proof is relatively informal, and uses information from different mathematical or scientific theories that we know are incompatible with each other, where 'incompatible' means that we know or suspect that if the two theories were written in the same language, then it would be possible to derive a contradiction from the two theories.

Do we see such reasoning in mathematical or scientific practice? Yes. We have done so for a very long time. The example we are most familiar with are those of the early calculus and of Lobachevsky solving a problem in Euclidean geometry (about the space under an indefinite integral) by appealing to his hyperbolic geometry. The latter case is a rather simple one for $C\&P$ to work with, since there is no information about parallel lines that is used in the proof. This corresponds to the cases we referred to in the introduction where premises come from inconsistent theories, but are not themselves inconsistent with each other. More important and blatant uses of inconsistent premises are reconstructed in (Brown and Priest, 2004). We have shown similar reasoning in physics where we mix the liquid drop model with the shell model. Increasing numbers of PhDs in mathematics are written using informal proofs that borrow from different areas of mathematics; similarly for science. Since these are original, and mark new territory in mathematics and science, they are exactly the sorts of proofs, or types of reasoning, we should be cautious, and sceptical, about. In physics, chemistry and biology we only have incomplete theories. The great unification of the sciences is not on the horizon. Even the methodologies are sometimes in direct competition (Schummer 2015).

For example, both the Andréka-Németi group's (2008) and the Krause and

Arenhart (2017) approaches to physics urge us to develop a logical explanation of physics. They disagree on what counts as a logic, and what counts as a reduction. The first group prefer a first-order logic without the notion of forces, and where 'causation' is simply expressed in terms of before and after on the trajectory of a body, or on a spatio-temporal relationship between two bodies. Any vestige of causation that they have is metaphysically bare.

In contrast, the Krause-Arenhart approach uses a higher-order language with proper classes and forces that are causal. The 'reduction' of the Andréka-Németi group is thought of in terms of several formal theories and the limitative relations that bear between them at the meta-level. The 'reduction' for the Krause-Arenhart approach follows Suppes, to have one set of axioms, so one logical/mathematical theory. Thus, even here, where we have a highly mathematical, nay logical, approach to problems in physics, there is no promise of unification in a traditional sense of one theory. The fragmentation of science is in evidence. "*Quant à l'unité de la science, si ardemment projetée jusqu'au début du xxe siècle, elle est finalement restée pure pétition de principe devant la spécialisation croissante des domaines scientifiques*"[38] (Lévy-Leblond, 2014, 13).

Because of the fragmentation of the sciences on the one hand, and the need to use ideas from incompatible areas of the science to give fuller understanding and explanations, and to make better predictions and control on the other hand; it is pressing to reconstruct the reasoning, in order to show its coherence in the presence of global inconsistency. For the reconstruction we use an enhanced version of $C\&P$ —bundle informed chunk and permeate: $BC\&P$. The main motivation for using $C\&P$ over a paraconsistent formal representation of the reasoning is to preserve the meta-logical intuition that scientists tend to share, that they reason either classically or constructively, and even if they are not able to articulate these meta-logical intuitions in these words, they would all find reasoning through a contradiction in science to be problematic. Abramsky (in private conversation) uses the bundel diagrams to counsel us to reason short of inconsistency, we should not reason through the inconsistency. What we add to $C\&P$ as it has been developed, is the bundle diagrams, as a guide to individuating chunks and selecting what information permeates from one chunk to the next.

In our particular example, we used the two models of the nucleus of an atom, the liquid drop model and the shell model. In the presence of measurements,

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[38] As for the unity of science, so adently pursued up to the beginning of the twentieth century, what has remained is nothing but a guiding principle, unatainable under the increasing fragmentation of the domains of science.

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we find that the models contradict each other. Nevertheless, both are needed to explain the phenomenon of binding energies. We thus extended the application of the bundle diagram to include not just arguments, but more broadly the relationship between models when they are both used in an explanation.

This was one example. We could extend the technique further. To do the *BC&P* reconstruction for non-model theoretic proofs, ones that more closely resemble proof theory, we would need to change the bundle diagram, so the 'values' are now, say, mediate inferences from suppositions, where we make the formulas unique via a combination of normal-form of language and by imposing some ordering on formulas.

The bundle diagrams represent a situation where we have global inconsistency. The same can be done, without diagrams using sheaf theory and cohomology theory (Abramsky et. al. 2015). Both tell us when it is 'safe' (i.e. consistent) to extend our reasoning, and when it is that we overstep the bounds of consistency. *C&P* helps us to stay just within the bounds: we can be systematically careful about what formulas, axioms, assumptions, measurement statements we can locally consider together, and which we cannot. So the bundle-extension of *C&P* can handle quite a lot of cases.

What cannot be handled? If we are using formulas, theorems, results from different theories or measurements where inter-translation is not obvious, it is not clear that we could come up with a bundle diagram, and it might be more work than it is worth. That is, it might be just as difficult to do this, as it is to generate some other meta-proof of local consistency. The limitations to such extensions concern deciding what the *variables* are, what count as *valuations* for the variables (since they might not be common) and in cases where the valuations are different up each fibre, what is to count as an edge, since the semantics is quite different, it is not clear how to make a translation to then determine if two elements of the base space are pairwise consistent.

For example, say, one premise comes from model theory, and another from proof theory, then the valuations for the model theory formula will be various models (note also that we might not be able to order them up a fibre, and this is another limitation), and up the proof theory 'variable's' fibre, we might have immediate and mediate inferences. For the purpose of drawing edges between fibres, we might need to have some sort of translation, and this might not be obvious or desirable in all cases.

Another criticism of this approach is that it might not remain loyal to the intended reasoning of the mathematician who came up with the proof in the first place, or of the scientist who came up with the mixed explanation. This is quite correct. All that *BC&P* promises is that it is a means of staying loyal to the idea that is wide-spread in the mathematical and scientific communities that local reasoning is consistent, and usually classical or constructive.

There is a more interesting and thorny issue that we are touching on. It is that while at the object-level, we are being careful to 'stay locally consistent' at the meta-level, we must be reasoning paraconsistently in the very limited sense that we are reasoning about reasoning consistently within globally inconsistent theories, or models. A bundle diagram which has no path traversing each fibre only once *represents* reasoning inconsistently. So we are looking at a diagrammatic representation of inconsistency and reasoning *about* inconsistency, and this might be thought of as 'reasoning paraconsistently' without reasoning using a particular paraconsistent logic.

Some paraconsistent logicians claim that mathematicians reason paraconsistently, unbeknownst to them, in exactly this way. The claims of the developers of *C&P* are a bit ambivalent about the relationship between the *C&P* strategy and paraconsistency. Brown recognises that we *could* use a paraconsistent logic within a chunk, in principle, although this was not his original intention. Also, we are reasoning paraconsistently at the meta-level in the thin sense that we recognise the presence of inconsistency, and want to avoid bringing about explosion. What the very possibility of *BC&P* reconstruction shows us is that what we immediately fear is explosion, and only mediately, indirectly, inconsistency. This is one of the lessons of paraconsistency. Moreover, we are at pains, at the meta-level, to make very clear the distinction between explosion and inconsistency through the bundle diagrams. If we can represent the inconsistency, and avoid it, by reasoning short of it, then we reason paraconsistently in the limited sense of exercising damage control over the inconsistency. The details of the reasoning could be captured using a formal paraconsistent logic, but this is unnecessary.

What is interesting is to draw the lesson that it is crossing the *ex contradictione quodlibet* boundary into triviality or detonating explosion that is otiose in the present practice, not the lingering background possibility —although this is enough to already upset more sensitive souls. In the practice of mathematics and science today: having a bomb and a detonator is fine, using it is not. So, what we have done with *BC&P* is give a means of vindicating inconsistency toleration in many cases in mathematics and science. By using the bundle diagrams to choose the chunks and permeating information, we see the edge of consistent reasoning at the meta-level. So, we have pushed the problem of explosion into a smaller corner than it once occupied.

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6 Conclusion

6.1 Introduction

Inconsistency toleration is a phenomenon that takes place in the sciences once a contradiction is recognized (either in a scientific theory or within scientists' doxastic commitments), and despite this, the users of the theory remain able to identify specific inferential mechanisms that allow them to work with the inconsistent set of information and still preserve sensible reasoning −this is, she is able to escape from logical triviality.

A number of philosophers of science, call them the supporters of the *Paraconsistent view*, submit that inconsistency has been part of some of our best scientific theories without implying the irrationality of the scientific community. Some of the most popular examples of this are the early calculus (Cf. Brown & Priest 2004), Bohr's Hydrogen Atom (Cf. Brown & Priest, 2015), Classical Electrodynamics (Cf. Frisch, 2004), the Dirac Delta function (Cf. Benham et al., 2014) and inconsistencies related to Carnot's theorem (Cf. Meheus, 2002). All this contrasts sharply with the supporters of the *Traditional view* (Cf. Popper, 1959; Hempel, 2000; Vickers, 2013, 2014; Davey, 2014), which submits that if one takes for granted the overall rationality of scientists, one ought to conclude that scientific communities have never believed an inconsistent theory.

Up to now it seems that both stances are interested in the same issue: to explain how it is possible for an epistemic agent -a scientist- to trust an apparently inconsistent theory and not be irrational at the same time. The only difference is that one side of the debate submits that as the contradictions are only apparent, they do not pose any danger against scientific rationality; while the other side contends that some of these inconsistencies are legitimate challenges that have to be addressed as what they actually are, this is, contradictions. This considered, the research presented in this dissertation might be of the interest of all participants of this debate, supporters of the Traditional view as well as supporters of the Paraconsistent view.

The aim of this dissertation could be phrased in terms of using certain (paraconsistent) formal tools to achieve philosophical understanding of the ways in which scientists tolerate (and have tolerated) contradictions between theory and observation in the empirical sciences. For this reason, the work presented here consisted of a combination of espistemological, historical and formal approaches to the study of contradictions in the empirical sciences.

In what follows, I provide a summary of the results presented in the five previous chapters and some concluding thoughts.

6.2 The ignorance behind contradictions

If inconsistency toleration actually takes place in the sciences, and if it is a rational practice, philosophers of science would need to provide an explanation that addresses: (i) under which circumstances scientists could be rationally inclined to tolerate a contradiction, (ii) how they could preserve sensible reasoning while using inconsistent information and, (iii) how they could work relying on seemingly false information.

The first two chapters of these dissertation were devoted to argue in favor of the actuality of inconsistency toleration in the sciences as well as to explain under which circumstances scientists could be rationally inclined to tolerate a contradiction.

First, Chap. 1, was devoted to address the latter issue, this is, considering that contradictions are extremely problematic, why scientists would feel motivated to tolerate them. As a response to this, I submitted the following thesis:

> **Thesis *Ign→IT*:** When scientists find a contradiction in their theories, if they recognize to be ignorant regarding either the truth values of the conflicting propositions or segments of their theory's *theoretical structure*, they can be rationally inclined to tolerate such a contradiction.

Factual ignorance regarding s is often understood as the lack of knowledge of s' truth value. When scientists are pushed to tolerate a contradiction, I argued, it is often because they ignore both the truth value of s and the one of $\neg s$.

The sources of this type of ignorance can be diverse. On the one hand, it could be ignorance caused by lack of evidence in favor of s and lack of evidence in favor of $\neg s$, it could be caused by lack experimental resources that help scientists to decide whether s is the case, among other reasons. I proposed to see this type of ignorance as the *temporary undecidability*[1] of the truth value of a given pair of propositions s and $\neg s$ by an epistemic agent S at a specific time T_1. On the other hand, factual ignorance could be more complex, for instance, it could be seen as absence of knowledge regarding the (relevant) inferential connections that scientific theories allow for. I called this type *ignorance of theoretical structure*.[2]

In order to support this distinction, I provided two cases studies that illustrated the two kinds of factual ignorance: the intertheoretic contradiction between the Permanentist theory and the Continental Drift theory, and the anomaly in the measuring of the solar neutrinos' flux.

Once scientists are aware of their ignorance regarding the truth values of s and $\neg s$, it is not clear how they could be rationally justified to reject any of the two elements of the contradiction. Therefore, it seems that they are, at least temporarily, motivated to tolerate the contradiction while solving the problem.

6.3 The many contradictions

As not all ignorance is of the same kind, not all contradictions in the sciences look the same either. Chap. 2 was devoted to follow this intuition and analyze different types of contradictions in the empirical sciences. In order to do so,using the *Partial Structures approach* to scientific theories (*PPEE*),[3] I characterized an *empirical theory* in the following way:

Given an *empirical theory* T, $T = < \boldsymbol{D}, \boldsymbol{R}_i^n >$ "where $\boldsymbol{D}$ is a particular domain (a set of objects to which the theory is supposed to apply) and $\boldsymbol{R}_{i3}$ is a family of n-place relations holding between the elements of $\boldsymbol{D}$" (Bueno, 1997: 588). T consists of a set substructures (*partial structures*), $< A, A', ...A^n >$, of the form $A = <D, R_k>_{k \in K}$.

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[1] 'Undecidability' understood not necessarily as it is perceived in the literature of logic and philosophy of mathematics –as for the cases of inconsistent empirical science the truth values in question are, in the long run, likely to be determined (if interested in the connections between this conception of undecidability for statements from the empirical sciences see [Gutiérrez-Ramírez, 2015: Chap. 1. In Spanish].

[2] When ignoring (the relevant parts of) the theoretical structure of a theory, scientists would not be able to grasp abstract causal connections between the propositions of their theory, they can neither identify the logical consequences of the propositions that they are working with nor can explain under which conditions the truth value of such propositions will be false.

[3] Which is a semantic view on scientific theories that aims at explaining the rational use of incomplete and defective information.

In addition, any empirical theory is susceptible to be inconsistent in, in at least, three ways, namely, with itself, with observational findings or with other theories, this is:

- **Internally inconsistent** if a pair of partially true sentences, s and $\neg s$, is found in the structure A' of T', and such A' contains explicitly only the theoretical (non-empirical) elements of T'.
- **Inconsistent with observation** (this is, T' would be partially empirically inadequate) if two empirical structures of T', A' and A'', contain a pair of partially true sentences, s and $\neg s$.

lastly, two theories, T' and T'', would be

- **Mutually inconsistent** if, there is a partial-function that maps elements from one to the other, and a substructure of T' contains the partially true sentence s and a substructure of T'' contains the quasi-true sentence $\neg s$.

From these three different types of contradictions only one has been assumed to be remarkably common and almost never problematic: *contradictions between theory and observation.* For this reason, I was motivated to concentrate on analyzing the ways in which scientists deal with contradictions between theory and observation, in order to, maybe, in the long run gain a better understanding of how to approach the other two types of contradictions.

I characterized *contradictions between theory and observation* in the following way:

> **Contradictions between theory and observation:** consist of contradictions between a prediction, s, that T entails and an observation, $\neg s$.
>
> Let A_{exp} be an empirical substructure which contains observational reports of experiments about T on $\boldsymbol{D}$. This is:
>
> 1. $A_{pred} \models s$
> 2. $A_{exp} \models \neg s$

Later on, I provided three case studies that illustrate the tolerance of contradictions between theory and observation: the anomaly in the precession of the perihelion of Mercury, the anomalous behaviour of the atomic nuclei of elements with Magic Numbers, as well as the anomaly in the measuring of the solar neutrinos' flux. I explained why one should regard these cases as exemplars of contradictions between theory and observation, and once I did so, I submitted the following:

> ***Thesis ≪IT≫*:** History of science has provided us with sufficient historical evidence to conclude that inconsistency toleration is a phenomenon that has actually taken place in the sciences.

The historical evidence that I offered in favor of ***Thesis ≪IT≫***included the two cases that I provided in Chap. 1 as well as the two more that I presented in Chap. 2.

When scrutinizing the three cases that showed the toleration of a contradiction between theory and observation, it became clear that, while the three of them were illustrative of contradictions between theory and observation, these contradictions were significantly different from one another in a revealing sense. The particularities exhibited by each of the cases were not really particularities in a trivial sense, but distinctive features that could be easily generalized. The study of their differences gave rise to the following thesis (and distinction):

> ***Thesis MC*$_{(T\text{-}O)}$:** In the empirical sciences, there are, at least, three different types of contradictions between theory and observation; namely:
>
> - contradictions that satisfy a high degree of observational independence (*Inconsistency T-O (Indp)*),
> - contradictions that require the use of an additional theoretical framework to be identifiable (*Inconsistency T-O (AddFramework)*) and,
> - contradictions that illustrate a low degree of observational independence between the tested theory and a relevant auxiliary theory (*Inconsistency T-O(Aux)*)

The main outcome of this chapter was the typology of contradictions between theory and observation. Such typology could allow for an actual fine grained study of contradictions between theory and observation and the different ways in which they can be tolerated.

6.4 The reconstruction of contradictions

Chap. 3 deals with one of the most important objections to the study of inconsistency toleration in the sciences, namely: The historical reconstructions provided by the Paraconsistent view have often been historically inaccurate. Such a view has used parts of the relevant historical record to motivate applications of particular paraconsistent logics, leaving aside relevant information that could weaken their philosophical claims (Cf. Vickers, 2013: 186-90).

This is not a superficial objection. As a matter of fact, among philosophers of science, it has been commonly assumed that history of science should provide the evidence for generating, supporting and falsifying philosophical theses. It has also been believed that, in order to do so, historical information has to be obtained independently from specific philosophical commitments (Cf. Pitt, 2001; Schickore, 2011; Kinzel, 2015). And for a long time, philosophers thought that that was the way philosophy of science was made. Nonetheless, it has been recently pointed out that, quite often, when doing philosophy of science, philosophers have severely misused historical evidence, making their theses more philosophically biased than historically informed (see Schickore, 2011). If this is the case, in the end, the historical reconstructions that are biasedly used (and made) by philosophers are of no real philosophical use. The combination of these facts leaves us with the impression that the history of science might have shown the limits of the philosophical endeavor when studying the scientific activity.

With this in mind, in this chapter I scrutinized the relation between the theses defended in the previous two chapters and the historical evidence. I argued that, even if ***Thesis ≪IT≫***, textbfThesis *Ign→IT* and ***Thesis*** $\boldsymbol{MC}_{(T^{-}O)}$ fail (due to a lack of historical accuracy of the alleged supporting evidence), some of the resulting inaccurate reconstructions provided to support such theses, if philosophically biased, might still have an extremely high epistemic value, namely, to promote our understanding of either the corresponding philosophical theses or the studied scientific phenomenon.

What was discussed in Chap. 3, constitutes one of the most fruitful consequences of this research. While to see the connections between biased historical reconstructions and philosophical and scientific understanding can help the reader to value the case studies presented in this dissertation, it also can help her to see from a different perspective the use of philosophically biased reconstructions in different areas of philosophy.

6.5 *PPEE* and *Chunk and Permeate*

Due to its complexity, the phenomenon of inconsistency toleration has been studied from three different types of research programs:

- **Historical programs:** this type of programs have a deeply descriptive approach to contradiction in science, “which concerns the question whether inconsistencies commonly appear in science, and whether scientists sometimes accept and reason from inconsistencies” (Šešelja, 2017: 2).

- **Logical programs :** these programs have a more “normative perspective, which concerns the questions whether we can rationally reason from an inconsistent set of premises without ending up in a logical explosion, and if so, how” (idem)
- **Methodological programs :** this type of programs have “a normative perspective, which concerns the role of the standard of consistency in evaluations of scientific theories” (ibid).

On the one hand, the supporters of the Traditional view have generally approached the problem of inconsistency toleration via the historical programs −as they do not consider contradictions to be anything but apparent, they expect the historical record to suffice when clarifying this issue. On the other hand, the supporters of the Paraconsistent view have approached the problem, mostly, from the logical programs −as they consider the presence of contradictions in the sciences a motivation for proposing alternative (non-classical) logics.

Considering some of the problems that purely historical and purely logical research programs face, in Chap. 4, I argued in favor of providing a methodological integrative account to inconsistency toleration. Such that could help to scrutinize the historical episodes from different perspectives, at least, from a logical and a historiographical points of view.

As a response to this desiderata, I introduced the *Paraconsistent Alternative Approach* (PAA). This approach consists of the provision of different (very general) formal methodologies and strategies that can help scientists to avoid logical triviality in an ’optimal’ way –what is ‘optimal’ would depend on the own constrains of each of the cases that are being studied. These methodologies suggests ways in which information could be separated, transmitted and updated in order to avoid logical triviality while respecting the historical record (as much as possible). Some of them, like the ones presented by the *Partial Structures approach* (*PPEE*), can be used to provide explanations of the dynamics of inconsistency toleration at the theory level; some others, like *Chunk and Permeate* do the same work but at the agent’s reasoning level.

Considering the above, in Chap. 4 and Chap. 5 y reconstructed two cases of inconsistency toleration using both tools, *PPEE* and *Chunk and Permeate*.

First, *PPEE* constitutes a particular type of semantic approach to scientific theories; which has proved to be extremely handy to explain and model the use of defective (incomplete, partial, conflicting and inconsistent) information in the sciences (see Bueno, 1997; Bueno, French & Ladyman, 2002; da Costa & French, 2003; Bueno & French, 2011; Ladyman & French, 1999, 2002). The case study that I reconstructed using the methodology of *PPEE* was the anomaly in the measuring of the solar neutrinos' flux (a case that in Chap. 2 was characterized as extremely messy due to the holistic properties of the theories involved in the emergence of the contradiction).

I scrutinized this case aided by the *PPEE* methodology and the resulting reconstruction showed the following interesting features:

- The *PPEE* was easy to inform with the historical record in such a way that the reconstruction while systematic, was clearly faithful to the historical episode.
- The reconstruction was highly neater than the purely historical reconstructions of the same episode (that were provided in Chap. 1 and Chap. 2). Remarkably, this was a result that did no require yet to commit to any particular logic in order to be obtained.
- The reconstruction showed with clarity that, while the weakening of doxastic commitments could play a role in the toleration of contradictions, such a weakening is not enough for dismissing the contradiction (this goes against the large majority of the Traditional view-type of explanations).
- In addition, the reconstruction also highlighted the way in which the theory was updated during the time in which the contradiction remained 'active', and the way in which the conflicting information was used and located within the theory.
- Finally, the reconstruction was explanatory of the role that the empirical adequacy of the theory played for the rational toleration of the contradiction during almost 30 years.

In Chap. 5, I introduced *Chunk and Permeate*, a paraconsistent reasoning strategy that aims at explaining how it is sometimes possible -for rational agents- to reason sensibly from a contradiction without necessarily arriving at arbitrary conclusions. The strategy aims at separating an inconsistent set of information into consistent fragments (chunks) and letting a limited amount of information to permeate between them, the underlying mechanism is non-adjunctive. Equally than *PPEE*, *Chunk and Permeate* does not require to commit to a particular logic.

Furthermore, I introduced *Bundle Chunk and Permeate*, an extension of Chunk and Permeate than produces visual representation of the separation and the transmission of information. I explain how this version of the strategy allows reconstructions to be both visually informative but also better guided than with the original.

The case study that I reconstructed with *Bundle Chunk and Permeate* was the the anomalous behaviour of the atomic nuclei of elements with Magic Numbers (first introduced in Chap. 2). The obtained reconstruction showed the following interesting features:

- *Bundle Chunk and Permeate* was easy to inform with the relevant information about the physicists' inferential practices when combining two mutually inconsistent models.
- The visual representation obtained through *Bundle Chunk and Permeate* was informative in the sense that it helped the user to see possible routes for combining the elements of the mutually contradictory models.
- The reconstruction portraits how scientists hold different doxastic commitments towards the models that are combining depending on the specific problems that they are trying to solve.
- The fact that the paths from the bundle diagram were clear and closed showed that, when following that inferential route, scientists are not in danger of logical explosion.
- In contrast with the solar neutrinos' case, for this anomaly we did not have any additional information about the empirical adequacy of any of the models in a different domain than the 'anomalous' one. Nonetheless, the visual representation showed the 'safe' domain of application for each model.
- The resulting reconstruction was not yet informed by any specific logical consequence relation, yet, it showed what information was reliable and which should not be inferred, kept or transmitted.

At this point, it seems to me that the reconstructions of these case studies obtained by combining *PPEE* or *Bundle Chunk and Permeate* with the historical record are extremely revealing of the phenomenon of inconsistency toleration. First of all, both strategies portrait the ignorance revolving each of the tolerated contradictions and reveal the role that this ignorance played before and during the toleration of the contradiction.

For instance, the reconstruction of the anomaly in the measuring of the solar neutrinos' flux (made by using *PPEE*), showed that the holistic properties of the *SSM* as well as the theoretical ignorance that governed the scientists at the time, interacted in such a way that in order to be able to tolerate the contradiction, scientists had to systematically mistrust *different* parts of the theory while still relaying on the adequacy of the theory in other domains. The role that empirical adequacy played for understanding the toleration of this anomaly, is difficult to see only when looking at the historical record, however, with the help of a methodological formal tool such as *PPEE*, the landscape is much clearer. In contrast, the anomalous behavior of the nuclei of the elements with magic numbers and the way in which two different theoretical frameworks are combined to solve a problem was much neater in the formal reconstruction than it was in the historical reports.

This considered, the value of these formal tools used for modeling and explaining inconsistency toleration in the sciences should be recognized.

6.6 Concluding thoughts

This research was initially motivated by some accusations that supporters of the Traditional view, like Peter Vickers, posed against the Paraconsistent view:

> if one looks more carefully at the relevant history o science, most of these cases are not really inconsistent in any significant sense after all. And to reconstruct such cases *as* inconsistent sets of propositions is a highly dubious move. Often when this is done the motivation seems to be to find an application for a paraconsistent logic, and *not* to say something interesting or important about how science works, or even could work (...) There are just too many philosophers failing to read up on the relevant history, partially because is an unfortunate culture of publishing philosophy of science which makes no attempt to engage with the real scientific or historical facts. Instead it is enough for a philosopher to call his or her account a 'rational reconstruction' as if this in itself is enough to justify any disconnect, however glaring, between the claims being made and the real history of science. (Vickers, 2013: 252)

Wile I agree on the fact that historical accuracy is an important feature of a historical reconstruction and that philosophy of science should not be indifferent to the actual scientific activity, Vickers' critique against the quality of the reconstructions provided by the Paraconsistent view seems to me extremely superficial and flawed.

His only advice for those who use paraconsistent logics when studying historical episodes is that they surrender, fully unarmed, to the empire of the history of science. However, as I argued in Chap. 3, during the last decades, philosophers of science learned that the historical record allows for multiple reconstructions of any particular episode, this gives the impression that, without any additional methodology, philosophers and logicians will be lost when diving into the history of science.So it looks like Vickers wants us to lose the formal apparatus that we have for the study of contradictions and gain nothing in exchange.

In addition, the large majority of philosophers and historians of science that are interested in the objectivity of history of science have been pushed to weaken their expectations in this respect. History of science is systematically philosophically biased. Therefore, any reconstruction that we take about any philosophical matter is extremely likely to be biased in at least one sense. If what I have said here is along the right lines, both sides of the debate seem to be in equal situation. The reconstructions provided by any, the Traditional view or the Paraconsistent view, would be susceptible of philosophical biases. But if we would have to choose between these different reconstructions, we better chose the ones that provide us with the larger epidemic good.

As I argued in Chap. 4, solely historical reconstructions will not do the work. It seems naive to consider that explanations that are constructed ‘only by putting historical data together’ are not biased in any philosophical sense. As the characterization of "rational reconstruction" says, to put historical data together is not enough to provide a philosophical explanation of any phenomenon; and the way in which such order is chosen is either theory guided or theory laden. Therefore, for any (philosophical) rational reconstruction of a scientific episode provided, there is going to be a philosophical view behind. So, it seems to me, that the real problem for the historically-informed philosopher of science is to find a way in which the reconstructions that she provides do not render philosophically useless in the long run.

The reconstructions that I provided here, aided by paraconsistent formal tools, were not only faithful to the pertinent historical data but also they were more punctual and precise than the ones given by the Traditional view. For instance, if we compare the solely historical explanation for the anomaly in the measuring of the solar neutrinos’ flux (Chap. 2) with the *PPEE* reconstruction of the same episode (Chap. 4), it should be clear to the reader that the first, while detailed and historically informed is more obscure than the second, and that the latter does not ignore any of data from the former. Even if, in the long run, one discovers that the reconstructions obtained by using these formal tools were philosophically biased and historically inaccurate, as I argued in Chap. 3, they will still have played an important epistemic role for the development of philosophy of science, namely, to promote our understanding of the philosophical view that underlie the reconstruction, as well as the inferential mechanisms portrayed by the formal tool.

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

Published articles

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Martínez-Ordaz, M. del R. (2016): "Chunk and Permeate vs. Estructuras Parciales: Sobre como modelar la tolerancia a la inconsistencia en las ciencias empiricas", in *Rutas Didácticas y de Investigación en Lógica, Argumentación y Pensamiento Crítico*, Academia Mexicana de Lógica, Mexico: 335-349.

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