Inconsistency in empirical sciences (Draft)*

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Abstract

This paper deals with a relatively recent trend in the history of analytic philosophy, philosophical logic, and theory of science: the philosophical study of the role of inconsistency in empirical science. This paper is divided in three sections that correspond to the three types of inconsistencies identified: (i) *factual*, occurring between theory and observations, (ii) *external*, occurring between two mutually contradictory theories, and (iii) *internal*, characterising theories that entail mutually contradictory statements.

1 Introduction

> Now, when I am fighting with cancer of the colon, I came to the opinion that most (or even any) case of cancer is an inconsistency occurring in the world, which should be taken as the paradigmatic case by any true dialectical theory. I am therefore preparing myself to become a Hegelian after death.
>
> —Jerzy Perzanowski (2001)

Inconsistencies are usually rejected not just because they are (considered to be) false or because from them anything follows, according to classical logic. Inconsistencies can actually have very dangerous practical consequences. For instance, the parallel use of the metric and the traditional Anglo-American systems of measures caused a maladjustment in the trajectory of the Mars Climate Orbiter that led to its destruction in 1990 (cf. Alder 2003, p. 11).

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Note that these systems contradict each other only because they express different languages or conventions for measuring the same reality, and in no way do they presuppose different theories about the world: what we say in one system can be said in the other without altering the meaning.

Inconsistencies about the nature of reality might result in deeper incompatibilities, which justifies the aversion that most scientists and philosophers have to them. Before going further we need to introduce some conventions which will allow us to speak about our scientific representations of the world. The shortest kind of signs whereby we convey information about the world are sentences, statements or propositions, which we will represent by lower-case Greek letters α, β, etc. Some particular sentences, like "Nina has blue pants", are especially connected to experience and can be verified by experience, given some observations. These sentences are often known as *observation* sentences, which I will denote with ϕ and ψ.

Scientific theories can be represented as sets of sentences closed under a logical relation of consequence. That is, we start by a finite set of scientific laws (see sec. 2.1) and add to this set all the logical consequences that we can obtain from those statements, including the observational sentences that follow from them. If $\mathbf{A}$ is a finite set of sentences, and $\vdash$ is a relation of logical consequence, then $\mathbf{T} = \mathbf{A}^{\vdash}$ is a scientific theory. In what follows, I will use just $\mathbf{T}^{\vdash}$ to denote a theory closed with respect to $\vdash$.

This way to represent scientific theories corresponds to what is know as the *syntactical view*. In this view, the relation of consequence $\vdash$ has the properties of the classical consequence relation, which satisfies the *principle of explosion*, according to which *ex contradictione sequitur quodlibet* (from a contradiction, anything follows):

Postulate 1 (Principle of explosion)**.** $\alpha, \neg\alpha \vdash \beta$, for all α and β.

A consequence relation is explosive iff it satisfies explosion.

Definition 2 (Explosive consequence relation)**.** The consequence relation $\vdash$ is *explosive* iff it satisfies postulate 1.

The classical relation of consequence is explosive, for it satisfies the principle of explosion; but not all explosive relations are classical. A paraconsistent relation of consequence is one where where the principle of explosion does not hold in general. That is, a paraconsistent relation of consequence is just a no-explosive one. This is can be expressed by the following definition:

Definition 3 (Paraconsistent consequence relation)**.** The consequence relation $\vdash$ is *paraconsistent* iff $\alpha, \neg\alpha \nvdash \beta$, for some α and β.

Following Gotesky (1968), Bartelborth (1989, pp. 95–6) and Priest (2006a, p. 144), we can classify the inconsistencies that we can find in empirical or factual science into three groups:

Factual: Inconsistencies between a theory $\mathbf{T}^{\vdash}$ and a verified observational statement ϕ, where $\mathbf{T} \vdash \neg\phi$. (cf. Gotesky 1968, p. 488)

External: Inconsistencies between theories $\mathbf{T}_1^{\vdash}, ..., \mathbf{T}_n^{\vdash}$ that aim to describe the same system, but that "attribute different natures to the same thing or arrive at different conclusions about it" (Gotesky 1968, p. 484), implying mutually contradictory statements.

Internal: Which characterises a theory $\mathbf{T}^{\vdash}$ that entails two contradictory sentences, so that both $\mathbf{T} \vdash \alpha$ and $\mathbf{T} \vdash \neg\alpha$ mutually hold.

We can summarise the *traditional* theses about each type of inconsistency as follows:

Factual: If a theory is inconsistent with the data, then it is false.

External: If two theories contradict each other, at least one of them is false.

Internal: An internally inconsistent theory is trivial (entails everything) and, therefore, useless.

Davey (2014) calls *counter-tradition* to the spectrum of ideas —mainly associated to the paraconsistent programme— that moderately or radically challenge at least one of the theses above.

Although there are very few books exclusively dedicated to this subject (see Vickers 2013), it is worth mentioning that the study of inconsistency in empirical sciences has been very well represented in collective publications like Meheus (2002) and Martínez-Ordaz and Estrada-González (2017). Furthermore, in December 2019, the first workshop on *Inconsistency in Factual Science* was held in Rio de Janeiro, as part of the First World Congress of the Academia Brasileira de Filosofia.

2 Factual inconsistencies

Du hast dich da arg vergessen, du hast den Humor meines kleinen Theaters durchbrochen und eine Schweinerei angerichtet, du hast mit Messern gestochen und unsre hübsche Bilderwelt mit Wirklichkeitsflecken besudelt. … Mit dieser Figur hast du leider nicht umzugehen verstanden – ich glaubte, du habest das Spiel besser gelernt. Nun, es läßt sich korrigieren.

—Hermann Hesse, *Der Steppenwolf* (1927)

2.1 The tradition on factual inconsistencies

We learn from failure, not from success!

—Vlad Țepeș, *Dracula* (Bram Stoker, 1897)

Factual inconsistencies occur when our theories or scientific laws are contradicted by observations. Scientific laws, are perhaps the most interesting scientific statements, and also the most difficult to justify. Where $\mathrm{S}x$ stands for "x is a swan" and $\mathrm{W}x$ for "x is white", the law-like statement:

Sentence 4. All swans are white. $\forall x(\mathrm{S}x \rightarrow \mathrm{W}x)$

states that all possible individuals of a class (the class of swans) belong to another class (the class of white coloured things). Scientific laws have the logical form universal sentences, which is why they are so difficult to justify, for we cannot test all individuals that can possibly belong to some class. No matter our conventions, we simply cannot see that all swans are white.

Now, although we cannot verify 4, we can verify an instance where it holds, like:

Sentence 5. The swan a is white. $\mathrm{S}a \wedge \mathrm{W}a$

which states that the individual a that belongs to a class (the class of swans) also belongs to another class (the class fo white coloured things). Sentence 5 is a verifiable sentence that logically follows from 4 (assuming a is in the domain of discourse), which means that 5 is a potential corroborator of 4. Accordingly, sentence 5 is also a potential corroborator of any theory $\mathbf{T}^{\vdash}$ such that $\mathbf{T} \vdash \forall x(\mathrm{S}x \rightarrow \mathrm{W}x)$, for it would also be the case that $\mathbf{T} \vdash \mathrm{S}a \wedge \mathrm{W}a$.

Popper calls *corroboration* to the process whereby we verify the empirically verifiable consequences of a theory. It is in this sense that "theories cannot be verified; but they can be corroborated" (1935, p. 185).[1] However, Popper does not propose to accept a theory because it has been sufficiently corroborated, but because it has resisted several attempts to falsify it.

It may be though that, if we can corroborate for sufficiently many x that $\mathrm{S}x \wedge \mathrm{W}x$, then we could infer that $\forall x(\mathrm{S}x \rightarrow \mathrm{W}x)$. Often known as *incomplete induction* (induction from now on), this kind of inference goes "from *singular sentences* describing, for example, observations, experiments, etc., to *universal sentences*, hypotheses or theories" (Popper 1935, §1). Notwithstanding this, induction poses several challenges.

First, as the logical empiricists noticed, there is some arbitrariness to what constitute a valid inductive inference. Carnap, for example, warned of the hypothetical character that scientific laws have with respect to singular statements, and of the latter with respect to protocol sentences (1931, p. 400). Neurath, for his part, pointed out that the induction that makes it possible to obtain laws is justified by a decision (*Entschluss*) and that, therefore, all attempts to justify it are logically bound to fail. Hence, for him there can be no other concept of scientific truth that does not express, at bottom, our failure to try to falsify (1931, p. 299).

This idea is further developed by Popper when he characterises scientific theories as *partially decidable* (*teilentscheidbare*) systems of statements, in that they are "logically unverifiable, but *unilaterally falsifiable*" (1932, p. 426). This is due to the asymmetry between the possibility of falsifying and verifying a universal statement. While verifying 4 would require the verification of all its infinite instances, disproving it would only require falsifying one of its counter-instances expressed in a singular statement such as:

Sentence 6. The swan a is *not* white. $\mathrm{S}a \wedge \neg \mathrm{W}a$

which states that the individual a that belongs to a class (the class of swans) does not belong to another class (the class fo white coloured things). This statement has the logical form of a singular statement, and it is related to 4 in a very special way because, according to classical logic, they cannot be both true at the same time. This means that sentence 5 is a *potential falsifier* of sentence 4, and of any theory $\mathbf{T}^{\vdash}$ such that $\mathbf{T} \vdash \forall x(\mathrm{S}x \rightarrow \mathrm{W}x)$.

[1] With regard to the term "corroborate", see the following footnote of the English edition: "Carnap translated my term 'degree of corroboration' ('*Grad der Bewährung*') [...] as 'degree of confirmation'. [...] I did not like this term, because some of its associations ('make firm'; 'establish firmly'; 'put beyond doubt'; 'prove'; 'verify': 'to confirm' corresponds more closely to '*erhärten*' or '*bestätigen*' than to '*bewähren*'). [...] I fell within his usage, thinking that words do not matter. [...] Yet it turned out that I was mistaken: the associations of the word 'confirmation' did matter, unfortunately, and made themselves felt: 'degree of confirmation' was soon used —by Carnap himself— as a synonym (or 'explicans') of 'probability'" (Popper 2002, ch. 10, n. ∗1)

It is in this sense that the epistemological tradition maintains that a theory is false whenever it is inconsistent with the data. The falsification of a theory is much more tricky than it may seem, though. From the counter-tradition, Priest points out that no scientific theory is discarded because of a contrary observation, for we could simply treat it as an anomaly (2006a, p. 145). None of this goes against anything stated by the tradition, even removing Kuhn (1996), Lakatos (1978) and Feyerabend (1981) from it.

Popper's falsificationism was criticised by Reichenbach on the grounds that no scientific theory is really disproved by a counter-example (1932, p. 428). In his response to *Logik der Forschung*, he adds that we can always explain an inconsistency between a theory and a fact (i.e. a factual inconsistency) by "shifting the error" from the theory itself to the determination of fact (1935, p. 270). Reichenbach addressed this by defining scientific truth in probabilistic terms, where a true scientific statement has, by definition, a very high probability equivalent to certainty, and a a false one, a very low probability equivalent to impossibility (1936, p. 269).

In this framework, Reichenbach updated the principle of induction into a probabilistic one, that would allow us to infer that 4 is (probably) true if we can verify sufficiently many sentences like 5. The most attractive feature of this treatment in terms of factual inconsistencies is that it allows us to justify belief in a scientific law in spite of some irrelevant counter-evidence, whereas the falsificationist treatment seems much less forgiving with respect to negative evidence.

This apparent unforgivingness of falsificationism suggested Neurath a change in terminology similar to the one proposed by Popper. While the latter speaks of "corroboration" instead of "verification", the former speaks of "shaking" or "distress" (*Erschütterung*) instead of "falsification". Furthermore, he opposes both Reichenbach's and Popper's attempts to propose a general methodology of *induction* or *control*, respectively, applicable to any theory of any discipline.

He also criticises Popper's analysis of scientific theories as well-defined abstract *systems*, free of ambiguities, and with very clear logical relationships. Such an analysis, according to Neurath, is not applicable to all scientific theories because the statements "that we actually work with use many imprecise terms, so that 'systems' can only stand out as abstractions" (1935, p. 354). He proposed, instead, to analyse scientific theories as *encyclopedias* chosen by a researcher according to the nature of the object studied, and whose logical relationships with experience are not determined by a general model of scientific testing. This does not imply that there should not be some effective way of connecting such encyclopedias with observational statements. However, no general method of induction or control should determine in advance the nature of this connection for all scientific theories.

Neurath thus proposes a broader model of scientific theorisation and testing, which has Popperian systems as a special case. Although he criticises Popper's universal representation of scientific theories as systems, he recognises that Popper's and Reichenbach's proposals sufficiently explain certain domains of scientific theorisation —in contrast to Popper, who entirely rejects Reichenbach's proposal.

Critics of falsificationism often omit Popper's distinction between the *logical* and *practical* senses of falsifiability (cf. 1935, §9; 1989). The former is about the logical properties of theories, so that a theory will be falsifiable as long as it is incompatible with a non-empty class of observational statements (1935, §21). The second is more about the practical possibility of disproving the theory. so that "theories that are falsifiable in the first sense are never falsifiable in the second" (1989, p. 84).

We cannot reject a theory on the basis of a few contrary observations. However, that some statements are logically incompatible with a theory is what determines the potential comparative success of a research programme and the logical possibility that a empirical theory be false. Although *theoretical proposals* (see sec. 3.1) do not have translucent logical connections, the our best theories do. This is why we can at least demand from such theories that they establish a priori what facts could possibly falsify them or, at least, *shake* them. This even if it were in practice impossible to determine whether such facts have occurred.

2.2 The counter-tradition on factual inconsistencies

A more counter-traditional claim was made by da Costa, according to whom "the operation of falsification [...] consists in the appropriate restriction of the domains of application of theories (including laws and hypotheses)" (1997, p. 199). In this sense, da Costa proposes that a theory that has enjoyed sufficient confirmation is never falsified, but only restricted in its field of application. This is openly opposed to epistemological and scientific tradition because some falsified hypotheses, such as the Ptolemaic model of the Solar System, have been definitively abandoned and are considered strictly false.

Martinez-Ordaz, for her part, contributed an interesting distinction between independent and auxiliary factual inconsistencies. Let **T** be a empirical theory and **S**, the theory used for the design of an experiment. We say that an inconsistency between **T** and an observation is *independent* when the intersection between the relevant assumptions of **T** and **S** for this experiment is empty, and *auxiliary* otherwise (Martínez-Ordaz 2017, p. 142). Independent inconsistencies are factual inconsistencies that do not depend on the interaction between the assumptions of **T** and **S**. We cannot assert the same about auxiliary inconsistencies since it is not easy to determine to what extent the inconsistency is due to the interaction between **S** y **T**.

Another interesting contribution goes against one of Popper's strongest claims about the nature of empirical science. He differentiates the role of logic in formal and empirical sciences in that, whereas in the former logic is used for proofs, "for the transmission of truth," in the latter is used critically, "for the retransmission of falsity."[2] The critical use of logic in empirical science would, thus, demand that the *testing* (*logical*) *framework*, that is, the logic we use for comparing our theories with data, has to be "a very strong logic, the strongest logic, so to speak, which is at our disposal." Consequently, we have to "use the full or classical or two-valued logic" (1979, p. 305).

The testing framework should not be confused with the logic presupposed by a theory. For example, if a theory presupposes a paraconsistent logic and is empirically consistent, we can use classical logic as a testing logical framework without any problem. Popper's claim is quite reasonable because, being highly intolerant of inconsistencies, classical logic is excellent for retransmitting falsehood. Whether a logic is suitable as testing framework and particularly for falsification, depends precisely on its capacity to retransmit falsehood.

However, Tennant (1985) proved that intuitionistic and minimal logics are as suitable as classical logic for this purpose. However, this does not seem to be the case of paraconsistent logic, since its tolerance to inconsistency can prevent it from ever retransmitting falsehood. This does not seem to have been disputed by the counter-tradition, whose main work has been the analysis of external and internal inconsistencies.

3 External inconsistencies

If I'm going to have a past*, I prefer it to be* multiple choice*.*

—The Joker, *The Killing Joke* (Alan Moore, 1988)

The main problems concerning external inconsistencies also concern internal ones: What kind of epistemic commitment exists towards two theories that are inconsistent with each other or towards an internally inconsistent

[2]Cf. Priest's (2006b, p. 84) claim that: "[T]he central uses of deductive argument are (*i*) to stablish new truths from old (as in mathematics) and (*ii*) to establish old falsehoods from new (as in experimental refutation)."

theory? On the other side, is a scientist who operates with inconsistencies rational and could such rationality be explained by a paraconsistent logic? In this section I address these questions for both types of inconsistencies, and in the next one I discuss the problems unique to internal inconsistencies.

3.1 Epistemic commitment

> *This is an* imaginary story *—which may never happen, but then again may. ... This is an* imaginary story*...*
> *Aren't they all?*
>
> —Alan Moore, *Whatever Happened to the Man of Tomorrow?* (1986)

Some authors believe that the adequate strength of epistemic commitment to inconsistent information must be weaker than that of belief. This is because an inconsistent body of knowledge cannot possibly be true even if we can operate with it through a paraconsistent logic. If we are talking about external inconsistencies, at least one of the theories must be false.

This leads Madan to the counter-intuitive proposal that "a rational scientific objective would be to find [...] factually adequate theories" that are "maximally inconsistent" with respect to each other (1983, p. 453). This objective is reasonable because, the theories being only approximations, there is no impediment to having theories that contradict each other if both have empirical support. This is especially so in disciplines that do not have a unified theory, but rather several theories that are incompatible with each other but that explain different aspects of the same problem. Madan goes further saying that believes that aiming to maximise inconsistencies could prevent the "disillusionment faced by economics students brought up to believe their subject is a science like physics" (1983, p. 454). This proposal seems to be incompatible with scientific realism because it assumes that science —or at least economics— often allows "proposition to be false in the actual world, yet true often enough to warrant attention" (1983, p. 454).

However, there are also proposals that try to make scientific realism compatible with an inconsistent science. The concept of *acceptance*, originally proposed by Bas van Fraassen (1980), is used for this purpose understood as "an epistemic attitude intermediate between belief and mere entertainment" (2007, p. 117). We say that we accept a theory when we treat it *as if* we believed that it is true, or at least its observational consequences. In Lipton's words, van Fraassen looks at our beliefs so that they do "not go too far above or beyond the evidence" (2007, p. 121). Consequently, to affirm that a theory is true may be a sufficient but not necessary for having a realistic commitment towards a theory or a set of theories (Brown 1990, p. 281).

For this reason, Brown proposes to accept an inconsistent set of sentences if it provides "the best and most general account available of their domains", but also to establish "contextual limits on that commitment so as to avoid bringing incompatible claims into play at any point" (1990, p. 285). Lipton proposes instead that we should reduce our belief "must accept because our belief has to be lowered from the full theory in the face of the contradiction" (2007, p. 121). In this sense, it is possible to reduce the original theory or theories to a consistent set of statements, none of which has been deduced from the contradictions of the original system (2007, p. 128).

This line of thought has been criticised by Joel Smith, who coined the concept of *proposal* "to refer simply to a collection of statements", reserving the concept of *theory* for denoting deductively closed collections of statements. (1988b, p. 429). When the deductive closure of a proposal results in an inconsistent theory, it is not correct to say that we believe or accept the resulting theory. Instead, the theoretical scientists "uses the original proposal along with the confirming evidence available for various parts of that proposal to give a schematic "projection" of what the consistent replacement theory (or some fragment of that theory) will look like" (1988b, p. 438). This is why, for Smith, inconsistent theories cannot be treated as final theories whose acceptance must be justified, since inconsistent theories can only be studied within the context of discovery.[3]

For their part, da Costa and French they regard the abandonment of belief in the logical consequences of our proposals "is a radical move to make under any circumstances since it places these scientific developments, profoundly important as they are, beyond the reach of logic altogether" (2002, p. 111). They propose instead to understand scientific truth not in the frame of the correspondence theory, but as *quasi-truth*. In this conception, scientific theories are always *partially true*, which means that we have an epistemic commitment to them, but of a different nature.[4] This simplifies things for it is no longer necessary to establish the contextual limits proposed by Brown.[5]

Davey questions whether scientists' epistemic commitment toward their

[3]This refers Reichenbach's distinction between the contexts of discovery and justification Reichenbach (1961, §1).

[4]The concept of partial truth depends on the concept *simple pragmatic structure* da Costa (1997, ch. III) and da Costa and French (2002, sec. 4).

[5]"However, one may wonder if it is even possible to effect the clear cut division between different 'contexts' or 'sub-sets' withing a theory that this account requires. Even in the Bohr example it can be questioned whether there was quite the 'systematic division' of contexts that this approeach requires." (da Costa and French 2002, p. 108)

inconsistent theories implies belief in their own inconsistencies. For example, liquids can be treated as (1) continuous distributions of matter (as proposed by Navier-Stokes) or as (2) very large sets of particles executing random movements (Davey 2014). In spite of being these treatments mutually inconsistent, Davey does not regard the belief of scientists in such theories as problematic since they need not believe in the literal truth of either (1) or (2), let alone of both together.

> Of course, the physicist *is* committed to the claim that for certain purposes it suffices to treat a liquid [(1)] and that for other purposes it suffices to treat a liquid as [(2)] —but there is nothing logically inconsistent in that, any more than there is an inconsistency between the mother who in ordinary circumstances says that her son is 5 feet tall and the very careful doctor who says that he is 5.01 feet tall. (Davey 2014, p. 3018)

In short, Davey argues that the only way to find an inconsistency in a physicist's beliefs under (1) and (2) is to believe that there is a commitment to the literal truth of all the theories he uses. It is not clear, however, whether da Costa's proposal for partial knowledge, which comes from the counter-tradition, is compatible with Davey's proposal. Perhaps what is at the heart of this discussion is the nature of the epistemic commitment with scientific theories and hypotheses, a problem that is common to both tradition and counter-tradition.

3.2 Rationality

If there are inconsistent proposals or theories in science that we consider valid, we must *in some way* reject the generality of the *ex contradictione sequitur quodlibet*. However, it is not clear in what *way* we should do so. In the case of two mutually inconsistent theories $\mathbf{T}_1^{\vdash}$ y $\mathbf{T}_2^{\vdash}$ it could suffice to believe in their union $\mathbf{T}_1^{\vdash} \cup \mathbf{T}_2^{\vdash}$, but not in its deductive closure $(\mathbf{T}_1^{\vdash} \cup \mathbf{T}_2^{\vdash})^{\vdash}$ —where $\vdash$ is the classical relation of consequence. This would mean to believe every statement of $\mathbf{T}_1^{\vdash}$ and $\mathbf{T}_2^{\vdash}$, including the contradictory pairs of statements, but in no logical consequence (classical or otherwise) of the union of both theories. This because, even if $\mathbf{T}_1^{\vdash} \cup \mathbf{T}_2^{\vdash}$ were inconsistent, it would not be trivial as $(\mathbf{T}_1^{\vdash} \cup \mathbf{T}_2^{\vdash})^{\vdash}$ would be. To believe $\alpha \in \mathbf{T}_1^{\vdash}$ and $\neg\alpha \in \mathbf{T}_2^{\vdash}$ would not automatically make us believe anything for our belief is not logically closed on their union. If non-triviality is a necessary condition of rationality, then belief in two inconsistent theories does not automatically entail irrationality.

A different case is that of internally inconsistent theories. Unless we treat them only as proposals in Smith's sense, classical closure makes them trivial and therefore useless to science. It seems that here we do need a paraconsistent logic to prevent inconsistent theories from becoming trivial. Something similar happens when we want to use two inconsistent $\mathbf{T}_1$ and $\mathbf{T}_2$ theories to derive, for example, technological applications. What if $\mathbf{T}_1$ and $\mathbf{T}_2$ offer complementary technical applications but at the same time are mutually inconsistent? A similar problem arises for theories with verified predictions that we later prove to be inconsistent. How do we justify these predictions and not their negations? How do we justify the acceptance of the applied technologies of $\mathbf{T}_1$ and $\mathbf{T}_2$ without getting lost in their mutual inconsistency?

In the *logic-driven* strategy we avoid triviality by assuming that $\vdash$ in $\mathbf{T}^{\vdash}$ is a paraconsistent relationship. This would also make it possible to believe in the logical closure of $(\mathbf{T}_1^{\vdash} \cup \mathbf{T}_2^{\vdash})^{\vdash_{\mathsf{P}}}$; where $\vdash$ is still classic but $\vdash_{\mathsf{P}}$, paraconsistent. Several works have been done in this line of research.

Drago (2002) has argued that classical logic has not been the only logical framework presupposed by scientists, and that there are historical examples of intuitionist and paraconsistent reasoning in science; examples of the latter would be Lobachevsky's treatment of his geometry and Carnot's treatment of thermodynamics.

The main area of research, however, is that of rational reconstructions of the logical procedure with which certain theories operate. For example, multi-deductive logic was developed by de Souza, da Costa, and others in order to unify incompatible physical theories within a single formal system. Thus, classical particle mechanics, classical particle electromagnetism and Bohr's quantisation postulates can be unified into a formal system despite their incompatibilities. These theories would be closed with respect to a multi-deductive relationship $\vdash_{\mathsf{S}_3}$ which, due to incompatibilities between the basic theories, must be a paraconsistent relationship. This unification is not the one aspired by physicists, but it could be represented with that scheme if "the same logic is maintained and there is no incompatibility between the theories" (de Souza 2000, p. 259).

On the other hand, Brown, Priest and others have developed a strategy called *Chunk and Permeate*, with which they have analysed the infinitesimal calculus, Bohr's atomic model and Dirac's δ function. Following this line of research Friend and Martínez-Ordaz (2018) studied the interaction between the *Liquid Drop Model* and the *Shell Model* of the atomic nucleus. Both mutually inconsistent models are used together to extract predictions that neither can achieve alone. Scientists must therefore work with a paraconsistent reasoning strategy that allows interaction between both models two while avoiding triviality. Such a strategy is called *Bundle Chunk and Permeate*, which is an extension of *Chunk and Permeate* that incorporates

Abramsky's fibre analysis.

The logic-driven strategy is implicitly guided by da Costa's *principle of systematisation*, according to which "*reason is always expressed by means of a logic*" (1994, p. 45). This principle applied to inconsistency, however, may interpreted as if the scientist and the philosopher is always rational when dealing with an inconsistency. This, of course, cannot possibly be true about each particular action and decision made by scientists.

In the *content-driven* strategy, the inconsistencies of a theory are solved in favour of the system we try to describe with our theory. In this way, if we have that $\mathbf{T} \vdash \alpha$ and $\mathbf{T} \vdash \neg\alpha$, we have to carefully select which of α and $\neg\alpha$ makes more sense about they content of the system we are aiming to describe with our theory. Needless to say, if we do not want $\vdash$ to be paraconsistent, then we must conceive $\mathbf{T}$ as a proposal rather than a theory. Within this content-driven strategy are Norton's studies on the Quantum Theory of Black-body Radiation (1987) and on the Newtonian Theory of Gravitation (2002), as well as Smith's (1988b; 1988a).

The premise of this strategy is that inconsistencies are a consequence of the axioms by which we formalise our theoretical representations of the world, and not necessarily of the representations themselves. Hence, it is in principle possible to resolve these inconsistencies by referring back to those representations. If the technological applications of two theories are mutually inconsistent in terms of accuracy, this may be easily solved by deciding how interested we are in accuracy and simplicity, for this context. The problem with this strategy, though, is that no effective method has been proposed to solve the theoretical inconsistencies. But again, had we such method, the content-driven strategy could be reduced to the logic-driven strategy, for we could build a logic where either α or $\neg\alpha$ (or non) would be a theorem of our theory.

According to the epistemological tradition, at least one of two mutually inconsistent theories has to be false. This does not go against what most of the counter-tradition defends. For example, the family of non-adjunctive approaches avoid triviality by isolating the consistent parts of a theory so that contradictions never appear simultaneously. However, there are those who defend the possibility that these inconsistencies are not a defect to be avoided. For these authors, certain (internally or mutually) inconsistent theories perhaps should not be accepted in spite of their inconsistencies, but precisely because of them. This is because they argue that the world is, in a sense, inconsistent. This thesis can be referred to as *factual dialetheism*, and is best understood in the discussion of internal inconsistencies.

4 Internal inconsistencies

Internally inconsistent theories present at least two problems that we have not discussed yet. First, does it make sense to talk ask whether there are true contradictions about the world? And, if so, how should we proceed to accept them or reject them? In what follows, I will discuss these problems.

4.1 Contradictory facts

> *Yo vi una Rueda altísima, que no estaba delante de mis ojos, ni detrás, ni a los lados, sino en todas partes, a un tiempo. Esa Rueda estaba hecha de agua, pero también de fuego, y era (aunque se veía el borde) infinita.*
>
> —Jorge Luis Borges, *La escritura del Dios* (1949)

Perhaps the first to address with logical rigour the possibility of true contradictions with logical rigour was Jan Łukasiewicz, who may be considered the spiritual father of the counter-tradition. He criticised the arguments advanced by Aristotle for, what he calls, the *logic*, *ontological* and *psychological* formulations of the *principle of (non) contradiction* Łukasiewicz (1910, §1). Łukasiewicz points out an *ignoratio elenchi* in the arguments by which the Stagirite pretended to justify that *no contradictory statement can be true*, with an argument that only establishes that *not all contradictory statements can be true* (1910, p. 28).[6]

This leads da Costa (1994) to conclude that the existence of true factual contradictions can only be established a posteriori. He also argues that it is easier to verify the hypothesis that there are true contradictions than to falsify it. Whereas verifying it would only require to verify the existence of a single factual contradiction, falsifying it would require to falsify infinitely many cases.[7]

Becker objects to this that scientific practice in the face of inconsistencies has always been to try to eliminate them; which has always been achieved for internal theoretical inconsistencies. To assume that one should proceed otherwise implies that scientists have "made some wrong moves in provid-

[6] Mignucci (1996) presents a more formalised version of this criticism.

[7] "O que se pode dizer, no tanto, é que *a priori*, especialmente apelando para a lógica, não se justifica nem se podem banir as contradições. A existência ou não de contradições reais só se estabelecerá *a posteriori* pela ciência. E, como tudo sugere, afigura-se mais fácil provar a verdade da tese de Hegel, do que sua falsidade; com efeito, uma constatação, apenas, de contradição real, comprovaria a tese de Hegel, ao passo que nenhum número finito de constatações seria suficiente para falsificá-la." (da Costa 1994, p. 208)

ing for such an elimination, so that the contradictions should be allowed to stay" (2018, pp. 20–1). All of which would result in the speculation that some internally inconsistent theories were wrongly replaced for a consistent substitute, in spite of the fact that there was nothing wrong with them. In this way we "would end up hanging to the science that could have been, instead of the science that actually is" (2018, p. 21).

4.2 Observing inconsistencies

> A contradiction *in terminis* implies no more than an impropriety of speech. Those things which men understand by improper and contradictious phrases may be sometimes really in nature without any contradiction at all.
>
> —Isaac Newton, *Letter III* (1693)

What we may call factual dialetheism does not require empirical dialetheism. That is, that there are contradictory facts does mean that we can experience them directly. In science, we test the existence of unobservable entities by means of observable ones, where the existence of the latter is deduced from that of the former. Similarly, unverifiable statements are tested through verifiable statements, where the truth of the former implies the truth of the latter. This is why an inconsistent theory could have consistent sets of potential corroborators and falsifiers.

However, it would not make sense for the dialetheist programme to accept an inconsistent theory if it is not corroborated qua inconsistent. This would not be necessary if its inconsistency was considered a defect to be corrected, but this is not what dialetheism states. But then we run into the problem of whether it is possible to observe or experience an inconsistent state of affairs.

In this respect Gotesky remarked that even if, by some law of nature, it was impossible for something to have incompatible properties, "such a law would not be able to inform us, *in advance*, of the properties which are, in any given case, incompatible or contradictory" (1968, p. 473). This idea is specified by Bobenrieth, for whom negation "does not reflect or represent something in reality but something that we do with reality" (2007, p. 508) because "negation is an operation that occurs by virtue of our category schemes" (1996, p. 407).

Nevertheless, in spite of Bobenrieth's argument against this, some possible observations can still be interpreted as incompatible according to our assumptions for interpreting our observations. In fact, according to Priest's empirical dialetheism "*seeing that* is always an interpretative process, and inference may well play some role in a rational reconstruction of how it proceeds" (2006a, p. 59) — and the like can be argued about other forms of experience and data recollection. This is why it is, in principle, possible to see that something is not red: we only need to see that it is green, which is incompatible with being red according to the theory of colour.

Priest's argument for this is vision-centred, but it can easily be translated into other forms of experience data recollection. His first step is to assert that it is possible to see what is described by an atomic (observational) statement, such as, for example, that a flower is red. From this follows the following thesis.

Thesis 7. It is possible to observe that x is A, for some x and A.

The second step is to argue that we can also observe that something is *not* the case. This should be the case when, for example, observing a yellow rose we also somehow observe that is not red. To this we can object that what we observe is only that the flower is yellow and that only by inference do we know that the it is not red. But Priest offers another example where we would not need this kind of inference for observing that something is not the case. Consider, for example, Mario's room thought experiment. His whole life, Mario has been locked into a room where he could see the whole chromatic spectrum with exception of the colour yellow, and she could only see red flowers. However, one day Mario leaves this room and the first thing she sees is a yellow flower, so he will observe that it is *not* red. With a similar argument, Priest considers the following thesis to be justified:[8]

Thesis 8. It is possible to observe that x is *not* A, for some x and A..

The next step is to realise certain cognitive functions are involved in vision, including *categories of understanding* such as the conjunction and disjunction logical functions. For example, it is possible to see that a certain photograph is of Ned or Ted, if they are identical twins and we see a photograph of one of them without knowing which one is it. In other words, it is possible to see that "this photograph is of Ted *or* this photograph is of Ned." In the case of the conjunction, by looking at the yellow rose we can see that "the rose is yellow *and* the rose is *not* red." Therefore, the following thesis seems justified.

Thesis 9. If ϕ and ψ can be observed, then $\phi \wedge \psi$ can be observed, for some ϕ and ψ.

[8]Priest's original thought experiment was: "[Y]ou enter a room: the whole room is visible from where you stand; there is no one there. You can see that Pierre is not in the room." (Priest 2006a, p. 143)

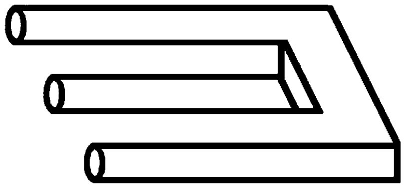

Figure 1: *Impossible trident*, by D. H. Schuster (1964).

However, it does not seem sensible to also assert that a contradictory conjunction is observable. How would it be possible to see at the same time that something is and is not? Priest tries to convince even those who consider that any situation described by an inconsistency is impossible as follows:

> Seeing impossible situations is quite possible. This is what we perceive in various visual illusions. ... [T]here are many well known impossible figures (of the kind, for example, employed by Escher in his drawings); there are perceptual sets where people report seeing things as simultaneously red and green; there are situations where things appear to be moving and not moving. (2006a, p. 121, see Fig. 1)

Which justifies the main and most interesting thesis of his essay.

Thesis 10. If ϕ can be observed, then $\phi \wedge \neg\phi$ can be observed, for some ϕ.

If it is possible to observe what is expressed by a contradiction, then it may be possible to test inconsistent theories *qua inconsistent*. In the next subsection we will examine the few proposals made for the testing of inconsistent theories.

4.3 Testing inconsistent theories

There is an incompatibility between the principle of falsifiability and inconsistent empirical theories. Since an inconsistent theory classically implies all the statements of its language, it would be "compatible with all propositions [and] there would not be even one capable of falsify it" (Piscoya 1995, p. 60). Since paraconsistent logics allow to modify this condition, Piscoya proposes to redefine the principle of falsifiability in a more general way.

> Indeed, the requirement of falsifiability for scientific-empirical theory demands that such a theory does not imply at least one proposition. Certainly the truth of such a proposition, not being implied by the theory, would be *incompatible* with it and [...]

> may be interpreted as *a falsifying one*. [...] Consequently, we will reformulate more precisely the requirement of falsifiability for scientific-empirical theories by stating that they are falsifiable if and only if they are absolutely consistent. (1995, p. 67, my emphases)

This proposal, however, has some shortcomings. First, as we saw in section 1, a theory can be absolutely consistent but empirically trivial. In such a case, it makes little sense to say that the theory is falsifiable because it still "implies any conceivable observational prediction [...] and thus tells us nothing about the world" (Hempel 2000, p. 79).

However, even if absolute consistency were required at the level of observational statements, this definition does not clearly define the class of potential falsifiers of an inconsistent theory. Piscoya's proposal defines the falsifiability of a theory not in terms of its potential falsifiers, but in terms of its logical properties. A necessary condition for defining the falsifiability of a theory is to clarify how this theory stands with respect to verifiable observational statements. Piscoya's proposal may be specified with a that he suggested to me in a personal communication.

Definition 11 (Absolute falsifier)**.** A sentence ϕ is an *absolute* (*potential*) *falsifier* of a theory $\mathbf{T}$ iff any observational sentence follows from $\mathbf{T} \cup \{\phi\}$.

This definition allows us to establish a logical criterion for falsifying a theory. However, it excludes theories formalised in logics that are very adamant to trivialisation and that, therefore, would not admit many absolute falsifiers; e.g. the family of strongly paraconsistent logics from definition **??**. A paraconsistent logic does not rule out the existence of certain statements that trivialise the theory. For example, in the calculus C_1 of da Costa (1963), da Costa (1974), and da Costa (1993), a theory $\mathbf{T}$ such that $\mathbf{T} \vdash *\neg\phi$ would be trivialised by $\mathbf{T} \vdash \phi$, which means that ϕ is a potential falsifier of $\mathbf{T}^{\vdash}$.

As a counterpart we have Jaśkowski's non-adjunctive approach (Jaśkowski 1999; Vasyukov 2001), where the conjunction introduction rule does not hold for each pair of statements. In this way, if we have $\mathbf{T} \vdash \alpha$ and $\mathbf{T} \vdash \neg\alpha$, we cannot infer $\mathbf{T} \vdash \alpha \wedge \neg\alpha$, which means that the we cannot use the principle of explosion in the form $\alpha \wedge \neg\alpha \vdash \beta$. A similar premiss is followed by the *chunk and permeate* strategy (Brown and Priest 2004).

It seems then that, in these cases, any contradiction can be handled in such a way that the theory is not trivialised. Therefore, a theory whose underlying logic is that of Jaśkowski or that uses the *chunk and permeate* strategy will have very few absolute falsifiers. This could be seen as evidence that certain logics are simply not suitable for empirical science. However, in order not to discard a theory just because of its underlying logic, it is worth redefining the class of potential falsifier so that it does not only include trivialising statements.

One such proposal is done by Priest, who start disclaiming to know of any empirical theory with contradictory observational consequences. The contradictions involved in inconsistent theories, such as Bohr's atomic theory, would be theoretical rather than observational. The following proposal, then, must be seen as potential approach to be taken if we ever were to test an inconsistent empirical theory. Let $\mathbf{T}$ be a theory such that $\mathbf{T} \vdash \phi \wedge \neg\phi$, where ϕ, $\neg\phi$ and $\phi \wedge \neg\phi$ describe observable situations. $\mathbf{T}$ is rejected or falsified if the situation expressed by $\phi \wedge \neg\phi$ is not observed.[9]

This proposal presents at least two issues. First, if we stick to the falsificationist programme, when a theory $\mathbf{T}$ implies an empirically verifiable statement ϕ, its corresponding potential falsifier would be $\neg\phi$. That's why we're more interested in observing $\neg\phi$ than ϕ. If we take $\phi \wedge \neg\phi$ as a testable consequence of $\mathbf{T}$, then its negation $\neg(\phi \wedge \neg\phi)$ would be a potential falsifier of $\mathbf{T}$. In most paraconsistent logics, including that of Priest (2006b, sec. 5), this formula is logically equivalent to $\phi \vee \neg\phi$, which means that observing either ϕ or $\neg\phi$ would falsify $\mathbf{T}$. It follows then that it is not possible to falsify an inconsistent theory from its contradictory statements.

But even if we accept the verificationist paradigm, implicit in Priest, we have problems. As we saw in 2.1, the logical empiricists did not ignore that there is some arbitrariness to induction. Verifying a theory, in this sense, partly depends on a convention since no finite number of statements can definitively justify a theory. Such a theory can only be more and more corroborated or, otherwise, falsified by experience. Thus, certain observational consequences of the theory cannot be tested because we do not yet have the necessary instruments to carry out such observations. If a theory $\mathbf{T}$ has two observational consequences ϕ and ψ, then we have that $\mathbf{T} \vdash \phi \wedge \psi$. But it is possible that ϕ has been observed, but neither ψ nor $\neg\psi$. Following Priest, we would have that $\phi \wedge \psi$ has not been observed and, hence, we would have to reject $\mathbf{T}$, which is does not seem sensible.

Malgrado tutto, Priest's proposal has the strength that, if we were to actually observe $\phi \wedge \neg\phi$, the improbability of such an observation (cf. Priest 2006b, sec. 8.4) would give unprecedented support to $\mathbf{T}$ qua inconsistent. But, in any case, it does not offer a solution for those who see only practical

[9] "If a theory entails an observable consequence α and α is not perceived, something is wrong, either with our theory or with our perceptions; something needs to be fixed. In particular, then, if a theory entails $\beta \wedge \neg\beta$, where β is some observation statement, then if such contradiction is not observed, something is wrong. As I have already argued, $\beta \wedge \neg\beta$ is a perfectly observable state of affairs." (Priest 2006a, p. 148)

use in inconsistent theories. It doesn't work for those who believe that the world is consistent, and inconsistent theories should only be used if we lack a consistent substitute.

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