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Can we test inconsistent empirical theories?

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Abstract

This paper discusses the logical possibility of testing inconsistent empirical theories. The main challenge for answering this affirmatively is to avoid that the inconsistent consequences of a theory both corroborate it and falsify it. I answer affirmatively by showing that we can define a class of empirical sentences whose truth would force us to abandon such inconsistent theory: the class of its potential rejecters. Despite this, I show that the observational contradictions implied by a theory could only be verified (provided we make some assumptions), but not rejected. From this, it follows that, although inconsistent theories are rejectable, they cannot be rejected *qua inconsistent*.

Keywords

logic of science; falsificationism; paraconsistency; dialetheism; contradictions; negation; negative facts; theory rejection

Sé padre de las virtudes y padrastro de los vicios. No seas siempre riguroso, ni siempre blando, y escoge el medio entre estos dos estremos, que en esto está el punto de la discreción.

—Don Quixote (Part II, Chapter LI)

1. Introduction

We usually call set of statements *inconsistent* or *contradictory* if it contains or implies a sentence α and its negation $\neg\alpha$. From the standpoint of classical logic, inconsistencies are considered unacceptable because of the *principle of explosion*, according to which any statement whatsoever is implied by a contradiction. A system of logic or a consequence relation may be called "explosive" iff it satisfies the principle of explosion, which means that classical logic is an explosive logic. Well then, it was seven decades ago that Jaśkowski published the first formal approach to handling inconsistencies and, since then, several other *non-explosive* or *paraconsistent logics* have appeared with several applications. These logics are called so (cf. Miró Quesada) because they avoid that an inconsistent set of statements immediately becomes *trivial*, which is the undesirable condition in which one such set implies absolutely any statement.[1]

In its most moderate forms, what we may call the *paraconsistent programme* aims to show that we can reason in the presence of inconsistencies or at least that we can develop formal systems of inference where we need not infer whatever we please from an inconsistent set of statements (cf. …). Although this undertaking is not short of boldness and heterodoxy, I believe it is not incompatible with the aims of classical scientists and philosophers. For if the only available theory for a given field would be an inconsistent one, the success of this programme would provide us with "some kind of crutch while we do not have something better" (Piscoya in Mosterín 1999 195). This programme, however, finds one of its more radical forms in *dialetheism*: the view that some sentences are both true and false or that there

[1] Some authors have defended that trivialism "makes perfect sense in its appropriate domain" (Estrada-González 88) or event that it is true (Kabay). I will disregard these remarks and "go about the business" (Kabay 15) of this paper since what these authors propose does not affect the substance of my work and my main interlocutors reject trivialism anyway (cf. *Ensaio* chapter I; *DTBL* chapter 3).

are true contradictions. These *dialetheias*, as Priest calls these true contradictions, need not to be what we may call observation, empirical, or data sentences; i.e. sentences we can intersubjectively decide that were verified or not by an observation. Nevertheless, the existence of what we may call *empirical dialetheias* has been suggested by da Costa (*Ensaio* chapter III) and Priest (1999; 2002), two of the greatest champions of the paraconsistent programme, which suggests that there may a place for observationally inconsistent theories in the empirical sciences.

The procedure for testing one such theory, though, has been barely discussed within and without the paraconsistent programme. In this paper, I explore the problem of *formally* defining the empirical content of inconsistent (but non-trivial) empirical theories, which for several considerations (see sections 4–5) is not equivalent to that of defining the class of potential falsifiers of such theories. *More precisely, I intend to extend Popper's concept of "potential falsifier" so that not only classical consistent theories, but also some inconsistent theories can have a class of statements that are incompatible with them, which I will more generally call "potential rejecters".*

My approach aims to be applicable both to consistent and inconsistent theories, preserving the properties of the of the classical concept of potential falsifier for observationally consistent theories. Within the domain of inconsistent theories, my approach aims to hold regardless of whether the inconsistency of our theory is regarded as a flaw to be corrected by a future consistent substitute, or as a feature that helps us describing properly inconsistent phenomena as empirical dialetheists would want it. However, we are going to see that my proposal represents some sort of Pyrrhic victory for empirical dialetheists, for although I claim that we can reject inconsistent theories—regardless of how we regard its inconsistency—we cannot reject them *qua inconsistent*. If I am right in this, then empirical dialetheism would be a dogmatic or, at least, an unscientific position, as it would be logically impossible to reject.

2. A note on falsificationism

My interest of extending Popper's concept of potential falsifier may be regarded as an uncalled-for attempt to sister paraconsistent logics with falsificationism: the latter being a philosophy of science that openly rejects non-classical logics for being "weaker" or "not critical enough" (*OK* 305–6). I do not regard this as a valid objection for two reasons.

First, a concept like potential falsifier can be used without subscribing to the whole of the falsificationist epistemology. The claim that we should reject our hypotheses given our acceptance of certain statements connected with the empirically accessible world—i.e. Popper's potential falsifiers—is independent from other falsificationist ideas like this being the only or main way of testing scientific statements. The need to define the class of potential falsifiers or rejecters of our hypotheses is related to the critical need that they can be proven wrong or, at least, less worthy of our belief. A hypothesis that cannot be tested in that way—without discarding other forms of testing it—can hardly be part of empirical science.

Second, although the first reason clearly imposes that we are critical towards our theories, it does not establish how much vulnerability to criticism is "critical enough". Whereas for Popper "[i]t doesn't matter if we are over-critical" (*OK* 305), we can choose to establish a less self-destructive standard not only for inconsistent theories, but also in general. Hence, we can work with our paraconsistent logics while handling inconsistent theories despite being a bit "under-critical", for that will be critical enough for our purposes.

Now, there is the valid concern that some paraconsistent logics may be "not critical enough" even for our purposes. For instance, in the only proposal for applying Popper's principle of falsifiability to inconsistent theories, Piscoya (section II.3) demands that a theory T be non-trivial in order to be falsifiable. Moreover, a potential falsifier of T would be any sentence φ such that the set $T \cup \{\varphi\}$ results *trivial*, meaning it implies any sentence whatsoever.[2] But in the case of those logics that take great pains avoiding trivialisation, like those satisfying *strong paraconsistency* (Perzanowski 8), this would make falsification very difficult if not impossible. This spirit of *avoiding trivialisation at all costs* might seriously undermine our capacity to be critical to inconsistent theories. Notwithstanding that, I think that this problem can be overcome without disqualifying these logics for empirical science, for I will show that we still can find ways to reject a theory whose underlying logic makes it impossible to trivialise it. After all, *pace* trivialists, being trivial is not the only way in which a theory can go wrong.

Another objection may be made apropos of the *dogmatic* or *naïve* falsificationist approach some readers may perceive as the framework of this investigation. Regarding this, I will now explain how the dogmatic jargon the reader may find in these pages is linked to a naïve attitude towards the isolated theories that form these programmes, and how this latter attitude is also linked to a sophisticated conception of research programmes.

Dogmatic falsificationism—often confounded with naïve falsificationism—is the thesis that a theory can only be scientific if there is an observation statement (see section 3) whose verification would prove it false and, hence, wrong. A dogmatic falsificationist would say that a scientific theory must be able to advance a "crucial experiment" where it can be proven wrong and worthy of being discarded—but never one (nor many) from which it can be proven right or worthy enough of being considered true (not even on inductive or probabilistic grounds). It is not clear that this view was subscribed by Popper in any of his writings. Note that, already in 1934, Popper declared that:

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[2] That the class of "potential trivialisers" of a theory should be identical to its class the potential falsifiers is not clearly stated in Piscoya's quoted writing, but it was clarified to me in a personal communication.

> *a conclusive logical proof for the untenability of a system can never be given*, because one can always e.g. consider the experimental results unreliable or claim that the contradiction between them and the system is only an apparent one and will be solved with the help of new insights. (*Logik* § 9, my emphasis)[3]

What Popper advocated, instead, was for the establishment of some *methodological rules* that help us decide when a theory has received enough negative evidence to be discarded, which is what *naïve* or *methodological falsificationism* consists of. Although in different words, he accepted Neurath's criticism that, in practice, we can only "shake" (*erschüttern*) a theory but never falsify it, since he recognised that decisions and conventions play a crucial role when a theory is tested. On the other hand, when he proposed his "systematic" conception of theories—as Neurath called it—, Popper was concerned with establishing a *formal definition* of the empirical content of scientific theories, and not so much with specifying the *practical process* whereby a theory would be experimentally corroborated or falsified according to its formally defined empirical content.

The formal definition of the set of observation sentences that are relevant for testing our theories is a necessary condition of a "logical criterion" for their testability (cf. Popper 1989). However, this does not mean that "in practice" we can easily verify observation sentences or that their verification would straightforwardly force us to reject the basic principles of a whole research programme. That is, even if we do agree that a given observation sentence contradicting a theory has been verified, our logical criterion does not force us to immediately reject what Lakatos called the "hard core" of that theory.

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[3] "*…ein zwingender logischer Beweis für die Unhaltbarkeit eines Systems kann ja nie erbracht werden*, da man ja stets z. B. die experimentellen Ergebnisse als nicht zuverlässig bezeichnen oder etwa behaupten kann, der Widerspruch zwischen diesen und dem System sei nur ein scheinbarer und werde sich mit Hilfe neuer Einsichten beheben lassen."

One of the main contributions of Lakatos' *sophisticated falsificationism* was, precisely, the thesis that the basic principles or "hard core" of a theory, by itself, does not predict observable states of affairs, for "auxiliary" hypotheses must be made about our instruments and the initial conditions of the case to be tested. In this conception, scientific experiments never test the basic principles of scientific theories: those principles are actually unfalsifiable and even metaphysical (cf. Lakatos chapter 1). What we put into test, instead, is the hard core of a research programme in conjunction with the set of auxiliary hypotheses in effect. Accordingly, a testable theory T may be syntactically represented as a set of sentences closed under a consequence relation that does not only comprise the set of basic axioms of the whole programme, i.e. its hard core, but also those assumptions that let us extract observable predictions from those basic axioms, i.e. its auxiliary hypotheses. Thus, a research programme $\mathfrak{T}$ may be represented as a series $\langle T_1, T_2, \ldots, T_m \rangle$ such that each $T_i = (C \cup H_i)^{\vdash}$ stands for an *isolated* testable theory of $\mathfrak{T}$, and T_{i+1} stands for the (improved) immediate successor of T_i. The set C comprises the statements of the *hard core* of the programme, which—ideally—are the same for all the theories in the sequence. For its part, H comprises the *auxiliary hypotheses* in effect for a given isolated theory, so that there cannot be two distinct T_i and T_j such that $H_i = H_j$.

Now, what do falsificationist say when the results of an experiment go against our predictions? A dogmatic falsificationist would say that our theory (meaning the whole research programme) has been falsified and we must reject it altogether. A naïve or methodological falsificationist would say that the theory (also meaning the whole research programme) has been shaken and that we may be in the path of methodologically deciding to reject it. A sophisticated falsificationist, instead, would say that (i) it is the isolated tested theory (comprising the hard core of the programme and the auxiliary hypotheses in effect) what has been shaken and should probably be rejected (after checking that, for example, the experiment was properly conducted), and that (ii) only time will say whether this result was an anomaly solvable with the same hardcore and different auxiliary hypotheses, or if this was a step towards *replacing this programme by a better one*, i.e. a different hard core.

Notice here that sophisticated falsificationism regards the tested isolated theory as a shaken one, thus connecting itself with naïve or methodological falsificationism. Similarly, the connection between dogmatic and naïve falsificationism is given by the way in which the former's language of "outright rejection" adequately simplifies that of latter for our purposes. In the end, this work will be concerned not with the practical intricacies of when it is appropriate to replace a programme by another one, but with the observation statements that are *logically* incompatible with isolated testable theories.

Furthermore, the work here presented can be extended into a formal framework for comparing the empirical content of two (or more) research programmes. Formally defining how observation sentences are related to our theories—even if only against some "auxiliary" assumptions—could help us "compare" rival research programmes, as "sophisticated falsificationism" would want it. The only way to decide which of two or more programmes is better supposes deciding which one has "corroborated excess empirical content" over the rest (Lakatos 32). A way to measure the "corroborated empirical content" of an isolated theory depends on us being able to formally define its empirical content, which in Popper's strategy is the set of potential falsifiers of the theory: the set of all those sentences that would force us to reject it. [4] A strategy to measure this "corroborated empirical content", of course, is not to be applied to these "isolated theories" resulting from the conjunction of the "hard core" and

[4] Popper's strategy of measuring the "corroborated empirical content" of a theory can be reconstructed from his discussion on the "degree of corroboration" of a theory (cf. *Logik* chapter VIII).

the "auxiliary hypotheses", but to those "series of theories" that stand for rival research programmes (cf. Lakatos 33) through a complex process which I cannot explore here.[5]

For all that said, if the language I use throughout this paper is indistinguishable from that of "dogmatic falsificationism", the reader must be aware that (1) this language is here used within a sophisticated falsificationist framework as explained above, and that (2) my investigation is circumscribed within the search for a "logical criterion" of testability, rather than within the problem of how a theory is accepted or rejected in practice.[6]

3. Observation sentences

What makes a formal language adequate for empirical science? Well, it must include a class of sentences that, under some assumptions (cf. section 4), can be somehow connected with the empirically accessible world. The most practical way to accomplish this is by distinguishing, according to the ongoing assumptions, sentences of at least two kinds within our language: *theoretical* and *observation* sentences. The former are unverifiable in the sense that no convention is sufficient for determining directly from experience that they are true, their logical form being that of *universal* sentences, like:

Sentence 1. For all x and y, the speed of x is less or equal than the speed of light (in vacuum) at circumstance y.

Observation sentences, instead, are verifiable in the sense that experts can intersubjectively reach an agreement on whether the *possible fact* denoted by one such

[5] I plan to explain this in my working paper "A logically neutral(ish) framework for empirical testing".

[6] For a detailed discussion on the distinction between "logical" and "practical" criteria of testability, see Popper's "Falsifizierbarkeit, zwei Bedeutungen von" and my "El sentido lógico de la refutabilidad". For a broader falsificationist countercriticism, see Miller's "Errando al blanco".

sentence is an *actual fact*. Their logical form may be that of *singular* or *existential* sentences, like:

Sentence 2. For some x and y, the speed of x is less or equal than the speed of *light* at circumstance y.

Sentence 3. The speed of a is less or equal than the speed of *light* at circumstance k.

The existential sentence 2 is a logical consequence of the universal sentence 1, and so is the singular sentence 3—provided a and k are in the domain of discourse. None of this is to say that all existential and singular sentences denote observable states of affairs, but just that some of them do whereas no universal sentence can. Accordingly, the set of observation sentences is a subset of the set of singular and existential sentences of our language. Now, although we need not to restrict our observation sentences to only one of these sets, there are good reasons for doing so.

Popper, for example, prefers existential sentences in order to show the asymmetry between theoretical sentences and their *potential falsifiers*. In this convention, the negation of a potential falsifier cannot preserve its logical form, as it would be unfalsifiable.[7] But as advanced, we need to follow a convention where the negation of an observation sentence also results in an observation sentence; otherwise, the conjunction of an observation sentence and its negation could not possibly result in an observation sentence. This would be impossible with existential sentences as the negation of one such sentence like 2 is a universal one like 2'. This forces us to exclude existential sentences from our scope.

[7] "[W]ir müssen die logische Form der Basissätze so bestimmen, daß die Negation eines Basissatzes seinerseits kein Basissatz sein kann" (*Logik* § 28). "Basic sentence" (*Basissatz*) is equivalent to "potential falsifier" in this context. However, Popper was not completely clear about his since somewhere else in his *Logik* (§ 23) he suggests that their logical form should be that of singular [*besondere*] sentences: "In realistischer Ausdrucksweise kann man sagen, daß ein besonderer Satz (Basissatz) ein *Ereignis* [*occurrence*] darstellt oder beschreibt."

Sentence 1'. For some x and y, the speed of x is greater than the speed of *light* at circumstance y.

Sentence 2'. For all x and y, the speed of x is greater than the speed of *light* at circumstance k.

Sentence 3'. The speed of a is greater than the speed of *light* at circumstance k.

Now, the aforementioned presupposes that the negation of any singular sentence expressing an observable state of affairs also expresses an observable state of affairs. Letting φ and ψ always stand for arbitrary observation (singular) sentences of our language, we now turn to present and justify some theses about observability that give support to this claim. What follows should not be regarded as complete rigorous proof of this claim, but as an incomplete informal argument to be further developed in a later work.

Thesis I. That φ is *observable* does not necessarily mean that it is *currently observable*.

By "currently observable", I mean "observable with currently available instruments", and by "observable", I mean "observable with instruments that may be available or may be theoretically possible to construct". This difference manifests itself when we advance theories with observational predictions that cannot be tested with our current instruments, but that partly provide the theoretical basis for constructing the necessary instruments in the future. For instance, the existence of gravitational waves was predicted by Einstein's General Theory of Relativity already in 1916, but it was not until 2015 that they could be observed. Almost one hundred years later!

Thesis I also covers the cases where φ is currently observable, but not so $\neg\varphi$. For example, the concept of musical pitch was already implicit in Pythagoras. Hence, it is reasonable to assume that he could have understood, *mutatis mutandis*, the meaning of the following two sentences:

Sentence 4. The pitch of that whistle is B_8 (~7.9 kHz).

Sentence 5. The pitch of that whistle is B_{10} (~31.6 kHz).

Nevertheless, while he could have verified sentence 4 with his own ears—although within some margin of error—, he could not have possibly verified sentence 5: he lacked the proper instruments for doing so. Yet, this does not mean that what is described by sentence 5 cannot possibly be observed. In fact, we now have the proper instruments for doing so.

Thesis I is important because it helps us show that, for each observation sentence φ, there must be at least another one logically entailing $\neg\varphi$. (Notice that I am not claiming, yet, that there must be an observation sentence logically equivalent to $\neg\varphi$.) Thus, suppose that there was no ψ logically entailing $\neg\varphi$. This would mean that our language cannot express an observable state of affairs incompatible with φ either because (i) φ necessarily holds or (ii) this language is not expressive enough. Since necessary claims are not precisely observable, the former case is irrelevant. The same holds for the second, as we need sufficiently expressive languages, *unless the expressivity of our language is only limited by our current instrumental limitations*. This justifies our second thesis:

Thesis II. For all φ, there is a ψ that logically entails $\neg\varphi$.

Now we need to show that $\neg\varphi$ expresses an observable state of affairs, i.e. that it is an observation sentence. I do this in two steps. First, I will argue that some disjunctions of observation sentences are observation sentences in their own right (Thesis III) and, second, I will show that any $\neg\varphi$ is equivalent to one such observable disjunction (Thesis IV). Both steps will heavily rely on arguments previously made by Priest.

Regarding our first step, Priest argued that "inference may well play some role in rational reconstruction of how [*seeing that*] proceeds" (1999 441), which means that some logical concepts may be part of such a reconstruction. This thesis is quite justifiable for disjunction in cases where we *see that* "Ted or Ned is playing", when we do not know which of these two identical twins we are seeing (Priest 2002 121). Now, this does not mean that all disjunctions can be regarded as observable. In my rejection of (i) above, the reader may have noticed that no disjunction $\varphi \vee \neg\varphi$ would qualify to me as an observation sentence. It does not make sense, in my consideration, to say of a necessary claim that is observable, i.e. testably by observation, for such a claim would be *a priori* verified—unless we consider that the *tertium non datur* does not necessarily hold for observation sentences. For all that said, I think it is safe to say that any disjunction of observation sentences of the form $\varphi_1(x) \vee \varphi_n(x)$ ..., provided it is neither a logical truth nor a logical absurdity, expresses an observable state of affairs in a similar way that "Ted or Ned is playing" does. This sufficiently justifies the following thesis:

Thesis III. Let Φ be a set of monadic predicates standing for observational properties that are mutually exclusive and exhaustive for any given object in its domain. Then for all $\varphi \in \Phi$, not only $\varphi(x)$ is an observation sentence, but so is the disjunction:

$$\bigvee_{\psi \in \Phi - \{\varphi\}} \psi(x)$$

In this framework, $\neg\varphi(x)$ may defined as the formula above, which is the (neither tautological nor logically absurd) disjunction according to which x has at least one of the properties that is incompatible with φ; which means that verifying any of its disjuncts would imply verifying $\neg\varphi$. For instance, if φ stands for "it is 0:00", then $\neg\varphi$ would stand for "it is 0:01, or it is 0:02, …, or it is 23:59". Hence, $\neg\varphi$ would be equivalent to an observation sentence ψ expressing the (positive) fact that it is either 0:01, or 0:02, …, or 23:59, or…", which means that $\neg\varphi$ is an observation sentence in its own right as I wanted to show.

Thesis VI. For all φ, $\neg\varphi$ expresses an observation sentence.

Somehow paradoxically, none of this is completely at odds with Bobenrieth's remarks against the observability "negative facts". According to him, negation "does not reflect or represent something in reality but something that we do with reality" (2007 508) and, hence, "there is no perception of negative facts", for "negation is an operation given by virtue of our category schemes" (1996 407). But Priest's point on "inference playing a role" in observation is precisely that "what we do with reality" is part of the process of *seeing that*: we can *see that* something is "not red" by *seeing that* it has some property incompatible with "redness" according to our theories or definitions.[8] It is in this sense that he states that although "the world as such is not the *kind* of thing that can be consistent or inconsistent"—as "[c]onsistency is a property of sentences"—, it is possible to extend the domain of these properties by specifying that a world is consistent if "any true purely descriptive sentence about [it] is consistent", and inconsistent otherwise (*IC* 159).

Hence, Bobenrieth's remarks may even be taken as part of the foundations of a "semantic dialetheism", as Priest characterised Bobenrieth's position (*IC* 302 footnote 32), although this was totally against the former's intention (cf. Bobenrieth 2019). What is more, Priest himself at one time remarked that "the observable world is consistent" since "our perceptions of the world are entirely consistent", except for "odd visual illusion[s]" (1999 463). It is not certain that he still believes this but, under the semantic dialetheism he ascribes to Bobenrieth, one might even joke that you can verify that it is both 1:34 and 9:41 (hence, not 1:31) if the screen of your phone is like the one in Figure 1.[9]

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[8] According to Priest, inference would play no role of we *see that* "Pierre is *not* in the room" when seeing an empty room (2002 120). It may be argued that the extension of "empty room" cannot include a room where Pierre (or anyone) is present, which would mean that "this room being empty" would be incompatible by definition with "this room having Pierre". But since Priest does not explain how we could in this sense *see that* Pierre both is and is not in the room, I will add no further comment.

[9] Bobenrieth's alleged semantic dialetheism would perhaps be one where a contradiction, as in Isaac Newton's words, "implies no more than an impropriety of speech", for the "things which men understand by improper and contradictious phrases may be sometimes really in nature without any contradiction at all" (*Letters to Bentley* 212). Would not this be indistinguishable from anti-dialetheism? Yes and no, may answer the dialetheist.

Fig. 1. We can *see that* this is a "dialetheic" phone by comparing the mutually contradictory times it shows at the centre and at the upper-left corner of its screen.

Priest argument for observable contradictions, depends on the further assumption that some conjunctions of observation sentences—including some contradictory ones—can also be regarded as observation sentences. Since I do not necessarily subscribe this, I will just enunciate it as a postulate and refer the reader to Priest's writings for the justification (i.e. 1999; 2002).

Postulate V. For some φ, $\varphi \wedge \neg\varphi$ is an observation sentence.

We are can now ready to tackle the problem of the empirical content of scientific theories, to which I turn in the following three sections.

4. The empirical content of classical scientific theories

In the syntactic view, a theory $T^{\vdash}$ can be represented as a set of sentences or formulae (from our formal language) closed with respect to some consequence relation. My main reason for preferring a syntactic treatment over a semantic one is that the latter would require fully specifying (i) the set of logical symbols to be used, (ii) the semantics of this symbols, and (iii) the properties of our consequence relation. This would force me to choose a particular paraconsistent semantics for my treatment, which would go against my interest of *extending* Popper's view rather than *adapting* it to some specific view of the handling of inconsistencies—as it is done, for instance, in the *partial structures approach* (cf. da Costa and French; Martínez-Ordaz). Hence, we need not specify the properties of $\vdash$ in advance as that should be done depending on which are more suitable to the theory at hand; particularly, on whether our theory is consistent or not (see sections 5–6). Hence, $\vdash$ will denote an arbitrary consequence relation in what follows.

Now, observation sentences are crucial for understanding the empirical content of T. In the terms of dogmatic falsificationism, these sentences can either corroborate it or falsify it. I will call a *potential corroborator* of T to any observation (singular) sentence expressing a case where T holds. Hence, the set of potential corroborators of T, or $Co(T)$, can be defined as follows:

Definition Co. $Co(T) = \{\varphi \mid T \vdash \varphi\}$.

For instance, if T entails sentence 1, then sentence 3 would be a potential corroborator of T, since sentence 1 entails 3, which would be ultimately entailed by T.

As previously advanced, though, theories can never be verified; they can only be almost ultimately falsified, to put it in the terms of naïve falsificationism. This is why it is more important to identify the sentences contradicting T, like 1'–3', than those simply corroborating it. Any observation sentence contradicting T falls within the following variation of Popper's definition of the class of *potential falsifiers* of T, or $Fa(T)$:

Definition Fa. $Fa(T) = \{\varphi \mid T \vdash \neg\varphi\}$.

Sentence 3' clearly fulfils this definition. Provided circumstance k is at our reach and we have the proper instruments, it is possible to observe that the speed of a is greater than the speed of light at k. That this was never observed, despite multiple tests, is not due to sentence 3' being untestable, but arguably to T being true.[10]

[10] That T is arguably true on these grounds, of course, does not "justify" that it is true, let alone from the standpoint of falsificationism, which opposes all forms of "justificationism" (cf. Lakatos chapter 1; Miller sections 6–7).

With these conventions at hand, we can formally define $\mathfrak{E}'(T)$, which stands for the empirical content of T, as the ordered pair comprising the sets of potential corroborators and falsifiers of T, in that order.

Definition E'. $\mathfrak{E}'(T) = \langle Co(T), Fa(T) \rangle$.

Incidentally, a formal definition of the empirical content of a research programme can be constructed from definition E'. The overall strategy would be quite simple: the empirical content of a research programme $\mathfrak{T} = \langle T_1, \ldots, T_n \rangle$ would be the n-tuple $\mathfrak{E}'(\mathfrak{T}) = \langle \mathfrak{E}'(T_1), \ldots, \mathfrak{E}'(T_n) \rangle$. The full strategy, however, supposes modifying the programme as to be compared with another one since a research programme cannot be evaluated in isolation. However, such a formulation requires much more space and, since it is not necessary for the purposes of this work, I will leave it for a future one.[11]

Now, before moving on, we must remark some things regarding the formal apparatus presented so far. First, it is not necessary that all observation sentences be part of the empirical content of T; it is perfectly possible that there be some φ be *neutral* with respect to T, which means that it would be neither its potential corroborator nor falsifier. Second, nothing in these definitions prevents the intersection $Co(T) \cap Fa(T)$ from being empty. In fact, it cannot be so if T is observationally inconsistent (assuming some laws of negation). In such a scenario, any T entailing two mutually contradictory sentences φ and $\neg\varphi$ would have these two sentences as both potential corroborators and falsifiers. Third, as a consequence of our second remark, nothing guarantees that the sets of potential falsifiers and corroborators of a theory be well-defined in classical terms, i.e. consistent. After all, if $\varphi, \neg\varphi \in Fa(T)$, any test performed regarding φ would falsify T, which is almost like saying that T is *a priori* false, rather than falsifi*able*. Fourth, the same applies to the formally defined empirical

[11] "A logically neutral(ish) framework for empirical testing" (Working paper).

content of a theory. We say that the empirical content of T is well-defined when both $Co(T)$ and $Fa(T)$ are well-defined and their intersection is empty; otherwise, the verification of any sentence in such intersection would leave us uncertain as to the epistemic status of our theory (cf. Author 2019). Fifth, these definitions already make it possible that some inconsistent theories have a well-defined empirical content: those that are observationally consistent.

All of this prepares the ground for my last remark, which concerns the difference in how our apparatus applies to observationally inconsistent theories in contrast to observationally consistent ones. In the latter case, we can reduce the empirical content to either of $Co(T)$ or $Fa(T)$. For example, we could define $Fa(T)$ with the equivalence $Fa(T) = \{\varphi \mid \neg\varphi \in Co(T)\}$. However, this would not be advisable for observationally inconsistent theories. As I have shown in my previous remarks, the empirical content of an observationally inconsistent theory T would be such that some φ and its negation $\neg\varphi$ would be in both $Co(T)$ and $Fa(T)$. This is inconvenient for two reasons. First, in line with the stated above, T would have to be *a priori* rejected given our assumption that at least one of φ or $\neg\varphi$ must hold. But even if we discard this assumption, this just would mean that, if both (or any) are observed, we would have to reject T. This is not only is the opposite to what a dialetheist would expect from T, but it would practically make us reject T *a priori*.

Hence, although our concept of potential falsifier can be useful for inconsistent theories that have a well-defined empirical content, it is useless with respect to observationally inconsistent theories. I address this issue in the next two sections by reworking our apparatus so that it can accommodate observationally inconsistent theories.

5. Issues regarding the paraconsistent case

The remarks of the previous section can broaden our understanding of why Priest proposed that a theory such that $T \vdash \varphi \land \neg\varphi$ be rejected if we just fail to observe $\varphi \land \neg\varphi$ (2002 125). Since neither φ nor $\neg\varphi$ would be incompatible with T, the route of observing a state of affairs would seems useless for rejecting it and, hence, it seems that we can only recourse to non-observing the state of affairs denoted by $\varphi \wedge \neg\varphi$. However, although Priest codifies this proposal in quasi-falsificationist or, rather, rejectionist terms[12], his seems to be a more verificationist tentative. All our observations can do here is either verify or fail to verify, but we cannot observe something that would make us reject T. In sum, Priest's proposal urges us to reject T in the absence of (positive) evidence, but not because of *counter*evidence.

But even if we accept this form of verificationism, things do not add up. Suppose $T \vdash \varphi \wedge \psi$, where φ and ψ do not contradict in any way. It can be the case that we observe φ, but we do not observe any of ψ or $\neg\psi$. In such case, no scientist would say that $\varphi \wedge \psi$ is to be rejected, for this might be due to limitations in our *current* instruments. Thesis I shows precisely why this does not have to be different if $T \vdash \varphi \wedge \neg\varphi$. Even if we were not able to observe $\neg\varphi$—that is, even if we were not able to see any ψ logically entailing $\neg\varphi$—, our most reasonable presumption would be that $\neg\varphi$ is not currently observable. But we need more than this to reject $\neg\varphi$. We need to observe something incompatible with $\neg\varphi$ being the case: some ψ expressing a state of affairs that, according to inference, would force us to reject $\neg\varphi$. Henceforth, the reasoning we used for $\varphi \wedge \psi$ can also be applied to $\varphi \wedge \neg\varphi$. Suggesting that it should be otherwise looks more like an attempt by the dialetheist of "dictating science from the armchair" (Becker 20).

My objection above is closely related to Kabay's claim that it is impossible to deny trivialism (2008 chapter 2). In his conception, the denial of a statement requires the assertion

[12] This was not the only place where Priest subscribed a falisificationist thesis. For example, compare his statement that "the 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)" (*IC* 84) with Popper's claim that in "the demonstrative science logic is used in the main for proofs—for the transmission of truth—while in the empirical sciences it is almost exclusively used critically—for the retransmission of falsity" (*OK* 305).

of an *alternative statement*. Although he does not fully state what are the necessary conditions for a given sentence to be alternative to another one, he states the thesis that "an alternative proposition to a conjunction cannot be a conjunct of that conjunction" (Kabay 77). This makes the denial of trivialism impossible since the assertion of trivialism can be conceived—as Kabay does—as the assertion of a conjunction of all the sentences of the language with respect to which one is a trivialist. In such a case, no sentence would be an alternative to trivialism, for all are conjuncts of the statement whereby trivialism is asserted. Similarly, it would be impossible to deny/reject a contradictory statement $\varphi \wedge \neg\varphi$ by asserting/accepting any of $\neg\varphi$ and φ because they are the conjuncts of that contradictory conjunction. Now, I have to issues with this thesis of Kabay. First, it seems that it would do away with the concept of self-defeating statement, i.e. statements that deny/reject themselves. For instance, if we let α be a deniable/rejectable statement and β be its alternative, one might say that the conjunction $\alpha \wedge \beta$ should be self-defeating. However, this may no longer be the case under Kabay's thesis. Otherwise, he would have to state that trivialism is self-defeating, that is, that by "asserting dialetheism" we are also "deny it", and he devotes a whole chapter to justify "the impossibility to deny trivialism" (chapter 3). What is more, if we agree with Priest that a statement φ and its negation $\neg\varphi$ are mutually exhaustive alternatives, any contradictory conjunction $\varphi \wedge \neg\varphi$ would be simply undeniable/unrejectable. Now, one might be attracted to dialetheism and even trivialism on philosophical or metaphysical grounds, but doing away with self-defeating statements and bringing some that are undeniable and unrejectable would be, in my consideration, too high a price to pay for a theory of science and reasoning in general.

Malgrado tutto, Priest's proposal has the strength that, if we were to observe both φ and $\neg\varphi$, the improbability of such state of affairs (cf. *IC* section 8.4) would arguably give T a great support *qua inconsistent*. After all, although Priest's proposal for rejecting observable contradictions is quite objectionable, his argument for the observability of some contradictions is at least worthy of consideration—even if only in a semantic conception such as the one he attributes to Bobernieth.

Another important takeaway from Priest's work is his use of the term "reject" instead of "falsify" to express our dismissal of a scientific statement. From his conception, it would not make much sense to say that a contradiction $\varphi \wedge \neg\varphi$ was empirically falsified as it was already false according to his semantics of logical connectives (*IC* chapter 5). For Priest, it is not the same rejecting a statement and accepting that it is false (cf. *DTBL* chapter 6). Although he considers it possible to accept that a given statement is both true and false, he deems it "impossible jointly to accept and reject the same thing" (*IC* 103; but see section 19.9). Hence, a broader conception of scientific testing—one which concedes a place to contradictory statements—cannot conceive the act of dismissing a given theory or statement simply as an act of falsification. In fact, I believe this is also closer to Popper's conception since, even when arguing against inconsistent theories, he was not so much concerned with them being false, but with them being trivial (cf. *Logik* § 24) given the principle of explosion he clearly subscribed (cf. 1943).

Now, there is an important logical consideration regarding any possible extension of falsificationism, or rejectionism as I will now call it, to inconsistent theories. This is not so much related to the fact that the logic assumed by Popper was explosive, since this can be easily solvable by letting $\vdash$ be paraconsistent in the case of those inconsistent theories. Instead, it is related with a fundamental difference that exists between classical and paraconsistent logic. In classical logic, α is true iff $\neg\alpha$ is false, which means that the truth conditions of a sentence depend on its falsity conditions, and vice versa. This is why, when we define the semantics of some logical functor $\circ$ we state that $\alpha \circ \beta$ is true in such and such conditions, *and false otherwise* (or, alternatively, that $\alpha \circ \beta$ is false in such and such conditions, *and true otherwise*). But this is not the case for paraconsistent logics as it may be that both α and $\neg\alpha$ are true, or false, or both. In such logics, truth conditions are (at least partly) *independent* from falsity conditions as, for instance, Priest's semantics of propositional connectives show (*IC* 75). Hence, it is no wonder that there is no straightforward way to reject a contradictory sentence from accepting another one under paraconsistent logics.

For all that said, if we want to make an inconsistent theory T "prove its mettle", we need to make it undergo a two-fold testing process. First, we have to fail to *reject* some "regular" observational consequences of T; i.e. its non-contradictory theorems, those observation sentences that are implied by T, but not their negations. Second, we need to succeed to *verify* some of its unfalsifiable, i.e. unrejectable, observational consequences: notably, its contradictory observational consequences. While the former will test T in a Popperian way, the latter will hopefully reveal whether some of its observable contradictions hold. Given the characteristics of paraconsistent logics described above, there is no way reduce both tests to only one. But how are we to define the class of sentences that can it possible to reject T?

The rationale for this is very straightforward. The set $Re(T)$ of *potential rejecters* of T is just the set of those potential falsifiers of T that are not also its potential corroborators.

Definition Re. $Re(T) = Fa(T) - Co(T)$.

For instance, a theory T entailing some φ and its negation $\neg\varphi$ would have both as potential falsifiers (and corroborators), but neither as potential rejecters by definition. This means that, regardless of whether we observe φ, $\neg\varphi$, or both, T will not be rejected, for no rejection would possible regarding φ (or its negation). Given this new definition, the empirical content of a theory has to be accordingly redefined as follows:

Definition E. $\mathfrak{E}(T) = \langle Co(T),\ Re(T)\rangle$.

This concludes the extension of the formal definition of the empirical basis, which answers affirmatively the question of whether we can test an observationally inconsistent theory. However, I have not fully established here what does it mean for a theory to be "rejectable". Doing so would require much more space as I would have to introduce concepts like "occurrence" and "event", and carefully adapt them to the special characteristics of inconsistent theories. I do this in sections 2.2 and 4.3 of my Master thesis (2020) and I will update this work for my future paper, "A logically neutral(ish) framework for empirical testing"[13]. Now it corresponds to discuss how these concepts should be applied.

6. Rejecting inconsistent theories

Suppose we want to take advantage of a very promising theory $T^{\vdash}$, where $\vdash$ is explosive, that happens to be inconsistent and, thence, trivial. The first thing to do is *detrivialise* this theory replacing $\vdash$ by some paraconsistent relation. It cannot be any paraconsistent relation as not all of them would guarantee that the resulting theory will be non-trivial. We therefore need a consequence relation $\vdash$ that is observationally *appropriate* for T; that is, one that makes $T^{\vdash}$ non-trivial observationally.

If we find a $\vdash$ empirically appropriate for T, and $T^{\vdash}$ happens to be observationally consistent, then we can basically work like in the normal falsificationist framework. In fact, definitions Fa and E' are demonstrably special cases of definitions Re and E: whenever $\vdash$ is classical and T is consistent, it all comes to the sentences contradicting T. Hence, the equations $Re(T) = Fa(T)$ and $\mathfrak{E}(T) = \mathfrak{E}'(T)$ hold for all observationally consistent theories.[14] This all means that a relatively conservative reform of the falsificationist programme can accommodate inconsistent theories—with their special and somewhat limited

[13] In my dissertation, I used the term "refutation" instead of "rejection", as then I did not see all the advantages of the latter.

[14] The proofs of these assertions can be found in section 4.3 of my dissertation (2020).

logical properties—while preserving all its original properties for the case of those classical consistent theories that Popper and most philosophers and scientists aim at (cf. Mosterín 2011).

What happens then if $T^{\vdash}$ is not observationally consistent? In that case, determining the members of $Re(T)$ will be a much less straightforward task. The previous case was easy because, as I advanced in section 4, $Fa(T)$ can be defined just as the set comprising the negations of the formulae in $Co(T)$. But with observationally inconsistent theories this would no longer be the case as we must prove for all $\varphi \in Re(T)$ that $T \nvdash \varphi$, which is normally more difficult than proving that $T \vdash \neg\varphi$. Now, if after trying all the paraconsistent consequence relations initially considered we do not obtain an observationally non-trivial theory, then we must either reconsider our selection or, otherwise, discard the theory without empirical test as we may have reached a point where logic should no longer be modified for the sake of a theory—unless we do not think such a point exists at all.

Now, although I have not aimed to apply this proposal to the analysis of a specific inconsistent theory, it is possible to give a rough example by the hand of Priest's conception of change and motion (*IC* sections 11–2). This conception is based upon a principle he calls Leibniz Continuity Condition (LCC), according to which "any state of affairs that holds at any continuous set of times holds at any temporal limit of those times" (*IC* 166). This principle, which Priest finds "clearly" innocuous in the context where he asserts it, seems hold in cases where we have a time interval from t_1 to t_n such that at, each $t_i < t_n$, some given object a has a property like "being red" or "being in motion", from which it would follow that a has those properties also at t_n. Unfortunately, we do not find a thorough justification of this principle by Priest, but just his (and possibly Leibniz's) "feeling that, if something violated this principle, the behaviour at the limit would be, in some sense, capricious" (*IC* 166).

Priest applies this principle to understand what happens at the "instant of change". The instant of change is exemplified by Priest as the time t that is (a) the limit, from the past, of the series of instants when a pen is on a desk (hence, not being in motion), and (b) the limit, from the future, of those instants when it is lifted by someone (hence, being in motion). If we accept LCC, our pen should necessarily be both in motion and not in motion at t, which would be the instant of change between these two states of affairs. Now, let P be an isolated theory Priest's programme (which includes appropriate auxiliary hypotheses) and φ the observation statement "a is in motion at t_n", so that both φ and $\neg\varphi$ are entailed by P. How do we test P in terms of φ? Following our previous definitions, both φ and $\neg\varphi$ would be potential corroborators and potential falsifiers of P, but precisely because of this is that neither of them could be its potential rejecter. As explained, we simply cannot reject P regardless of whether we observe that a is or is not in motion at t. Since both observations would be compatible with P, any of them would just corroborate it as any other of its instances would. However, P could be corroborated *qua inconsistent* if we observe both conditions holding of a simultaneously at t.

Something interesting would happen if we assume the *spread hypothesis*, according to which no body can "be localised to a point it is occupying at an instant of time, but only to those points it occupies in a small neighbourhoud of that time" (*ICC* 177). In this case, it may not be approprirate to test our hypothesis at exactly t_n, but we would need to do it with respect to a time interval around t_n. That is, we would have to observe if a is and/or is not in motion at times near enough t_n. In this case, even if we do not observe a being simultaneously in motion and not in motion at any time, it may be possible to argue that P was verified *qua inconsistent* if the following conditions hold: (i) a is consistently observed to be *not in motion* before the time that opens that interval, (ii) a is consistently observed to be *in motion* after the time that ends that interval, and (iii) we observe, within the interval, a time at which a is in motion, and a future time at which a is not in motion. It is obvious, though, that this could also be evidence of performing a bad measurement or perhaps of backwards causation (cf. *IC* 179).

Before ending this section, let me shortly explain why I do not think that LCC holds in general through the following example. Let t_n be defined as the exact instant where a given decelerating object a reaches the speed s. In that case, we can say that at each time $t_i < t_n$ (of the time interval in which our object is decelerating) we had a state of affairs where the speed of a was less than s, but this would no longer be the case at t_n. Here, there would be no need for conceptualising a particular "instant" of change different from the first instant where a has changed. My position could be objected, in consideration of the practical limitations or finitude of our instruments, as follows. For any speedometer capable of detecting the speed s, there would exist a speed s' it cannot detect precisely enough so that, although the time at which the speed of a equals s' may theoretically exist, we would never be able to detect it. Consequently, the best we could hope for is to find a time t_m that represents the limit of two states of affairs: (i) that of a having a speed less than s' and (ii) that of a having a speed greater than s'. The dialetheist may interpret this time t_m as the instant of change where the speed of a is both higher and lower than s'. However, I do not think this would be a valid objection as it would only show the limitations of our instruments, which would at best make the speed at t_m empirically *undecidable*. As we know, scientists are used to work with imprecisions in their measurements and they *estimate* the right value from several tests, instead of assuming that inconsistencies hold at the exact instant or place they are targeting. As it happens with formal languages, any instrument has limitations that makes it impossible for them to *decide* some exact values.[15] Notwithstanding all that, if we

[15] For a more thorough criticism of Priest's conception of motion, see the paper by Boccardi and Macías-Bustos.

were to assume this principle and Priest's conception of motion and change, the previous two paragraphs would explain *in abstracto* the way to test it.

7. Coda

The fertility of my proposal (or the lack thereof) cannot be decided in a logical investigation such as this. It is necessary to test some inconsistent empirical theory with it. Since I am no dialetheist, I am not compelled to carry out such investigation. All I aimed at was at providing a tool for that purpose: the formal concept of potential rejecter. Thus, the task of testing inconsistent theories is no longer mine, but of the *empirical dialetheist*, if there really is any.

But even if they succeed in testing an inconsistent theory as I am suggesting here, empirical dialetheists still have to deal with the undesirable consequence that inconsistent empirical theories cannot be rejected *qua inconsistent*, they can at best be corroborated as such (provided we accept the arguments of section 3). This would make empirical dialetheism a dogmatic position; contrary to a scientific spirit that seeks not only to corroborate, but also the opportunity to reject its hypotheses—or at least shake them. It is not possible to object—in the style of Reichenbach, Neurath, and Kuhn—that falsificationism proposes an oversimplified model of scientific practice. We have shown that empirical dialetheism results dogmatic and unscientific even in this oversimplified model and, hence, it cannot be expected to be undogmatic and properly scientific in a more sophisticated one.

I want to end by stating what I think is the most important takeaway of this work in Cervantes' words. When testing one of its inconsistent observational consequences, an inconsistent theory is somehow in a circumstance where "the reasons to save or condemn it are in one rank", so that it is better to "let it pass freely, for it is ever more praiseworthy to do good than to do ill"[16]. Here, we have decided to save inconsistent theories not for the sake of "doing good" to them, but because that is their only way into the realm of empirical science. But even if we stick to these ethical terms, we must be warned that as noble as mercy can be it can also "let pass" some (or all) of the vice we are trying to prevent, in this case, in science. Hence, as Don Quixote says in our epigraph, the wisest thing is "not being always cruel, nor always merciful, but choosing a mean betwixt these two extremes, for this is a point of discretion." By providing here the bases for a tool for criticism to inconsistent theories I hope to have gotten close to this "point of discretion", I hope I am following Don Quixote's advice to be "a father of virtue, but a stepfather of vice."

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[16] I am paraphrasing Shelton's translation. Here is the original quote, which is said by Sancho Panza: "Soy de parecer que digáis a esos señores que a mí os enviaron que, pues están en un fil las razones de condenarle o asolverle, que le dejen pasar libremente, pues siempre es alabado más el hacer bien que mal" (*Don Quixote* Part II, chapter LI).

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