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# Can We Test Inconsistent Empirical Theories?

*I will tell you [...] about those ways of enquiry which are alone conceivable. The one, that a thing is, and that it is not for not being.* [...]. *The other, that a thing is not, and that it must needs not be, this I tell you is a path wholly without report.* The Goddess (Parmenides, Fr. 3)

**Abstract.** Despite the logical possibility and alleged existence of inconsistent empirical theories, the problem of their testability remains largely unexplored. I develop a testability criterion which makes some of these theories testable, including some observationally inconsistent ones. However, they are not rejectable qua inconsistent by this criterion. These results, while opening the domain of scientific theories to inconsistent ones, challenge the prospects of dialetheism in the philosophy of science.

**Keywords**: paraconsistency; dialetheism; trivialism; proposal; rejection

## 1. Introduction

Consistency is traditionally regarded as a non-negotiable requirement for scientific knowledge. This traditional view is justified by standard or classical logic, whose *principle of explosion* or *ex contradictione sequitur quodlibet* allows us to infer anything we want from inconsistencies. Thus, if a scientific proposal turned out inconsistent, we would be able to infer anything we want from it, rendering it useless for science.

Science is not free from inconsistencies, though. Some historical examples attest to this, like the conflicting results in the estimations made in the 19th and 20th centuries of our planet's age. Moreover, it has been argued that some established scientific theories, like classical electrodynamics [cf. 33], are internally inconsistent. However, contrary to what

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classical logic would allow, scientists do not seem to infer whatever they please from inconsistencies. Some authors have taken this as evidence that scientific reasoning is not classical, but paraconsistent, as it seems not to adhere to the principle of explosion.

This suggests that inconsistent theories might have a pathway into empirical science. However, the question of whether these theories can be tested remains underexplored. I address this gap by proposing a logical criterion of testability accommodating of inconsistent theories which extends Popper's notion of falsifiability. This will show that inconsistent theories can indeed be tested. I will nevertheless also show that these theories cannot be rejected qua inconsistent, casting doubt on the viability of dialetheism as a view in philosophy of empirical science.

I will proceed as follows. Section 2 lays out the concepts necessary for the discussion. In Section 3, I discuss my methodological approach, engaging critically with the literature on scientific inconsistencies. Section 4 presents the desiderata for the testability criterion to be proposed. Section 5 surveys the literature directly relevant to our question. My own position is articulated in Section 6, and its application is roughly exemplified in Section 7. Section 8 provides some closing reflections.

## 2. Preliminary definitions

In this paper, I will talk about scientific statements, proposals, and theories by means of a formal language **La**. We can construct **La** from a vocabulary consisting of the infinite sets: $\mathcal{V}$ of individual variables, $\mathcal{C}$ of individual constants, $\mathcal{F}_n$ of $n$-place functors, $\mathcal{P}_n$ of $n$-place predicates, $\mathcal{S}_n$ of $n$-place connectives, and $\mathcal{Q}$ of quantifiers, for $n > 0$. Thus, our sets of terms $\mathcal{T}$ and of formulae **La** are inductively defined as follows:

Definition 1 (Language).
$\mathcal{T} \overset{\text{def}}{=} v \mid c \mid f^n(t_1, ..., t_n)$ and $\mathbf{La} \overset{\text{def}}{=} P^n(t_1, ..., t_n) \mid \circ^n(\alpha_1, ..., \alpha_n) \mid \wp v\, \alpha_1$, where $v \in \mathcal{V}$, $c \in \mathcal{C}$, $f^n \in \mathcal{F}_n$, $t_i \in \mathcal{T}$, $P^n \in \mathcal{P}_n$, $\circ^n \in \mathcal{S}_n$, $\alpha_i \in \mathbf{La}$, $\wp \in \mathcal{Q}$.

Bold uppercase letters **A**, **B**, ... will stand for subsets of **La**, while Greek lowercase letters $\alpha$, $\beta$, ... will serve as meta-variables for formulae of **La**. For unary and binary connectives $\circ \in \mathcal{S}_1$ and $\bullet \in \mathcal{S}_2$, I adopt the usual notations $\circ\,\alpha$ and $(\alpha \bullet \beta)$ instead of $\circ(\alpha)$ and $\bullet(\alpha, \beta)$, respectively. For each $n \in \mathbb{N}$, the sets $\mathcal{T}_n \subset \mathcal{T}$ and $\mathbf{La}_n \subset \mathbf{La}$ will contain all terms and formulae, respectively, with exactly $n$ free variables. Note that individual constants and saturated functions are members of $\mathcal{T}_0$ and that closed formulae are members of $\mathbf{La}_0$. In what follows, $t(t_1, ..., t_n)$ and $\alpha(t_1, ..., t_n)$ denote the expressions resulting from substituting the free variables $v_1, \ldots, v_n$ of $t \in \mathcal{T}_n$ and $\alpha \in \mathbf{La}_n$, respectively, for $t_1, \ldots, t_n \in \mathcal{T}$.

There can be as many formal languages as there are logical systems and scientific proposals, each of them with their own peculiar symbols. For simplicity, we shall assume that all proposals are formulated in — or translatable into — $\mathbf{La}$, which contains standard logical symbols such as:

$$\neg \in \mathcal{S}_1; \quad \wedge, \vee, \rightarrow, \leftrightarrow \in \mathcal{S}_2; \quad \exists, \forall \in \mathcal{Q}.$$

Formulae $\alpha \in \mathbf{La}$ express *scientific statements*. We may distinguish between theoretical and observational statements, which are often associated to general laws and particular occurrences, respectively [cf. 82, p. 165]. This association is misleading, as hard-core theoretical statements are not mere generalisations of observational statements (see Section 4). Hence, I instead distinguish between *observational* and *general statements*, the latter being generalisations of the former. General statements denote laws and their logical form is that of universally quantified formulae such as $\forall v\, \alpha(v)$. Observational statements, instead, are instances of general statements denoting particular occurrences, and they are intersubjectively verifiable in the empirically accessible world.

There is no fully logical or objective way to decide whether a given statement is general or observational or neither. The classification depends on the correspondence rules $\mathsf{R}$ adopted, which allow us to identify the sets $\mathcal{O}_\mathsf{R} \subseteq \mathcal{T}$ of $\mathsf{R}$-observational terms and $\mathbf{Ob}_{1\mathsf{R}} \subseteq \mathbf{La}_1$ of $\mathsf{R}$-observational formulae with one free variable. The intended domain of $\mathcal{O}_\mathsf{R}$ comprises all entities which are observable under the correspondence rules $\mathsf{R}$. Where $t \in \mathcal{O}_\mathsf{R}$, $\mathbf{Ob}_{1\mathsf{R}}$ includes all formulae $\alpha \in \mathbf{La}_1$ such that $\alpha(t)$ is $\mathsf{R}$-observational — i.e., it denotes an observable state of affairs according to $\mathsf{R}$ — and $\forall v\, \alpha(v)$ is $\mathsf{R}$-general.

Thus, the functions $\mathbf{Ob}_\mathsf{R}$ and $\mathbf{Ge}_\mathsf{R}$ return, respectively, the subsets of $\mathsf{R}$-observational and $\mathsf{R}$-general formulae in a given set.

DEFINITION Ob. $\mathbf{Ob}_\mathsf{R}(\mathbf{A}) \stackrel{\text{def}}{=} \{\alpha(t) \in \mathbf{A} : \alpha \in \mathbf{Ob}_{1\mathsf{R}} \text{ and } t \in \mathcal{O}_\mathsf{R}\}$.

DEFINITION Ge. $\mathbf{Ge}_\mathsf{R}(\mathbf{A}) \stackrel{\text{def}}{=} \{\forall v\, \alpha(v) \in \mathbf{A} : \alpha \in \mathbf{Ob}_{1\mathsf{R}}\}$.

In the following, I fix the correspondence rules and drop the subscript $\mathsf{R}$. Whenever I refer to $\mathbf{Ob}$ and $\mathbf{Ge}$ as sets, I mean $\mathbf{Ob}(\mathbf{La})$ and $\mathbf{Ge}(\mathbf{La})$, respectively, with $\varphi$ and $\psi$ being metavariables of formulae in $\mathbf{Ob}$.

A useful notion which we can define with the help of **Ob** is that of an event. An *event* is a typical or universal aspect of an occurrence, while an *occurrence* is what is described by an observational statement, like:

STATEMENT 2. *John is older than Paul.*

Statement 2 represents an occurrence that can be expressed with a formula $O(j,p) \in \mathbf{Ob}$, where $j,p \in \mathcal{O}$ stand for John and Paul, respectively, and $O(x,y)$ for '$x$ is older than $y$'. In contrast, '$x$ is older than Paul' and 'John is older than $x$' represent events, corresponding to different general aspects of the occurrence represented by Statement 2.

In order to sharpen this notion, I define events as follows:

DEFINITION 3 (Event). $\mathbf{Ev}(\alpha) \stackrel{\text{def}}{=} \{\alpha(t) \in \mathbf{Ob} : \alpha \in \mathbf{Ob}_1\}$.

Let $O(x,p), O(j,x) \in \mathbf{Ob}_1$. Thus, $\mathbf{Ev}(O(x,p))$ represents the event *being older than Paul*, and contains all possible instances of '$x$ is older than Paul', including 'John is older than Paul', 'George is older than Paul', 'Paul is older than Paul', etc. $\mathbf{Ev}(O(j,x))$ represents, instead, the event *John being older than*, which contains 'John is older than Paul', 'John is older than George', 'John is older than John', etc.

An important consequence of Definition 3 is the following:

COROLLARY 4. *For all* $\alpha \in \mathbf{Ob}_1$, $\mathbf{Ev}(\alpha)$ *is an infinite subset of* $\mathbf{Ob}$.

This is related to the fact that general statements have infinitely many observational instances, which reflects their logical connection in the sense that the latter can be inferred from the former.

This connection can be formalised with the concept of *logical consequence*, which I define as a function $\mathbf{Cn}_\mathsf{S} : 2^{\mathbf{La}} \to 2^{\mathbf{La}}$ mapping a set $\mathbf{A}$ to its set of $\mathsf{S}$-consequences — i.e., logical consequences according to some logical system $\mathsf{S}$. Thus, the set $\mathbf{Cn}_\mathsf{S}(\mathbf{A})$ consists of all $\alpha$ which are $\mathsf{S}$-consequences of $\mathbf{A}$. For example, in most logical systems $\mathsf{S}$, $P(a) \in \mathbf{Cn}_\mathsf{S}(\mathbf{A})$ if $\mathbf{A} = \{\forall x\, P(x)\}$ or $\forall x\, P(x) \in \mathbf{A}$ or $\forall x\, P(x) \in \mathbf{Cn}_\mathsf{S}(\mathbf{A})$.

Scientific statements are commonly gathered into *scientific proposals*, which are collections of general and observational statements containing all the expected consequences of a theory we seek to formulate.

DEFINITION 5 (Proposal). $\mathbf{P}$ is a proposal iff $\mathbf{Ob}(\mathbf{P}) \neq \{\} \neq \mathbf{Ge}(\mathbf{P})$.

In what follows, $\mathbf{P}$, $\mathbf{P}'$, $\mathbf{P}''$, $\ldots$, $\mathbf{P}_0$, $\mathbf{P}_1$, $\ldots$ will stand for proposals.

A property typically deemed desirable in scientific proposals is logical integration. A set of statements is *logically integrated* iff its statements fit together in a coherent way, according to some logical principles.

Definition 6 (Logical integration). $\mathbf{A}$ is $\mathsf{S}$-integrated iff, for some $\mathbf{B}$, $\mathbf{A} = \mathbf{Cn}_{\mathsf{S}}(\mathbf{B})$. $\mathbf{A}$ is logically integrated iff, for some $\mathsf{S}$, $\mathbf{A}$ is $\mathsf{S}$-integrated. $\mathsf{S}$ is an underlying logic of $\mathbf{A}$, and $\mathbf{Cn}_{\mathsf{S}}(\mathbf{B})$ the $\mathsf{S}$-integration of $\mathbf{B}$.

A scientific proposal, as defined here, may not be logically integrated. A scientific proposal that is logically integrated is a *scientific theory*.

Definition 7 (Theory). $\mathbf{T}$ is a theory iff there is an $\mathsf{S}$ and a $\mathbf{P}$ such that $\mathbf{T}$ is the $\mathsf{S}$-integration of $\mathbf{P}$ — that is, $\mathbf{T} = \mathbf{Cn}_{\mathsf{S}}(\mathbf{P})$.

In the following, $\mathbf{T}$, $\mathbf{T}'$, $\mathbf{T}''$, $\ldots$, $\mathbf{T}_0$, $\mathbf{T}_1$, $\ldots$ will stand for theories. Whenever I indicate that $\mathbf{T} = \mathbf{Cn}_{\mathsf{S}}(\mathbf{P})$, we will assume that $\mathbf{P}$ is a set of axioms sufficient to derive all the expected consequences of $\mathbf{T}$.

A *scientific prototheory* is a scientific proposal only some of whose parts are logically integrated.

Definition 8 (Prototheory). $\mathbf{P}$ is a prototheory iff (a) $\mathbf{P}$ is not a theory and (b) there is at least one theory $\mathbf{T}$ such that $\mathbf{T} \subset \mathbf{P}$.

My distinction between theories and prototheories follows Smith's characterisation of unorganised scientific claims as prototheories and of logically integrated ones as theories [77]. For him, inconsistent proposals should not be formalised as final theories to be fully endorsed, but rather examined as prototheories constituting intermediate steps in the context of discovering proper (consistent) theories. This distinction also parallels Neurath's between *model-encyclopedias* (related to proposals) and *model-systems* (related to theories). He critiqued Popper and Reichenbach for characterising scientific theories as well-defined, abstract model-systems with clear logical relationships and no ambiguities. In contrast, actual science, he argued, relies on rather indefinite terms, rendering model-systems mere abstractions [56, p. 354]. Although I do not fully disagree with Smith and Neurath, my aims require treating inconsistent proposals as (logically integrated) theories or model-systems.

Scientific proposals are also expected to be *consistent* and, more specifically, *non-trivial*. Some of the definitions that follow are given relative to a negation $\neg \in \mathcal{S}_1$ and a conjunction $\wedge \in \mathcal{S}_2$, but I will omit reference to them whenever no ambiguity arises.

Definition 9 (Consistency). $\mathbf{A}$ is $\neg$-consistent iff no $\alpha$ is such that: (a) $\alpha, \neg\alpha \in \mathbf{A}$ or (b) $\alpha \wedge \neg\alpha \in \mathbf{A}$ (for some $\wedge \in \mathcal{S}_2$); and $\neg$-inconsistent otherwise. $\mathbf{A}$ is observationally $\neg$-consistent iff $\mathbf{Ob}(\mathbf{A})$ is $\neg$-consistent, and observationally $\neg$-inconsistent otherwise.

Definition 10 (Triviality). $\mathbf{A}$ is trivial iff $\mathbf{La} \subseteq \mathbf{A}$, and non-trivial (or absolutely consistent) otherwise. $\mathbf{A}$ is observationally trivial iff $\mathbf{Ob}(\mathbf{A})$ is trivial, and observationally non-trivial otherwise.

Note that the consistency and triviality of $\mathbf{A}$ depend on $\mathbf{A}$'s members rather than on $\mathbf{A}$'s $\mathsf{S}$-consequences. The latter become relevant in case $\mathbf{A}$ is $\mathsf{S}$-integrated, as we will see in Corollaries 13 and 14.

Some authors hold that there is an 'appropriate domain' for triviality [30, p. 88] or even *trivialism*, the view 'that every proposition is true' [38, p. 10]. As my main interlocutors, da Costa [22] and Priest [72], oppose trivialism, it is tempting to disregard these views and just 'go about [my] business' [38, p. 15]. There is nevertheless a straightforward argument against trivialism in science. We do not want our proposals to subsume every other conceivable proposal in the following sense.

Definition 11 (Subsumption). $\mathbf{A}$ subsumes $\mathbf{B}$ iff $\mathbf{B} \subseteq \mathbf{A}$. $\mathbf{A}$ observationally subsumes $\mathbf{B}$ iff $\mathbf{Ob}(\mathbf{A})$ subsumes $\mathbf{Ob}(\mathbf{B})$.

However, it is clear that a trivial proposal subsumes every proposal.

Corollary 12. *If* $\mathbf{A}$ *is (observationally) trivial, then it (observationally) subsumes every proposal* $\mathbf{P}$.

This should be argument enough against adopting trivialism in the philosophy of science, but I will say more about this view in Section 5.2.

The question now arises as to why inconsistent proposals are also deemed undesirable. The answer lies in the effects of inconsistency within (logically integrated) theories. Consider the following corollaries:

Corollary 13. $\mathbf{Cn}_{\mathsf{S}}(\mathbf{A})$ *is* $\neg$*-inconsistent iff there is an* $\alpha$ *such that: (a)* $\alpha, \neg\alpha \in \mathbf{Cn}_{\mathsf{S}}(\mathbf{A})$ *or (b)* $\alpha \wedge \neg\alpha \in \mathbf{Cn}_{\mathsf{S}}(\mathbf{A})$.

Corollary 14. $\mathbf{Cn}_{\mathsf{S}}(\mathbf{A})$ *is trivial iff* $\mathbf{La} \subseteq \mathbf{Cn}_{\mathsf{S}}(\mathbf{A})$.

This means that a theory $\mathbf{T} = \mathbf{Cn}_{\mathsf{S}}(\mathbf{P})$ is inconsistent iff $\mathbf{P}$ $\mathsf{S}$-entails a contradiction, and trivial iff $\mathbf{P}$ $\mathsf{S}$-entails every formula. Now, consistency happens to be a defining feature of *classical theories*, introduced below. In what follows, $\mathsf{C}$ will stand for the classical logic.

DEFINITION 15 (Classical theory). $\mathbf{T}$ is classical iff (a) it is $\neg$-consistent and (b) it is $\mathsf{C}$-integrated, where $\neg$ is $\mathsf{C}$'s only negation.

Classical logic is associated with many properties, but in this paper, we will be concerned with only one in particular: explosiveness.

DEFINITION 16 (Explosion). $\mathsf{S}$ is $\neg$-explosive iff, for all $\mathbf{A}$, if $\mathbf{Cn}_{\mathsf{S}}(\mathbf{A})$ is $\neg$-inconsistent, then $\mathbf{Cn}_{\mathsf{S}}(\mathbf{A})$ is trivial.

For theories with explosive underlying logics, the following holds:

COROLLARY 17. *For all* $\mathbf{A}$ *and all* $\neg$*-explosive systems* $\mathsf{S}$*:* $\mathbf{Cn}_{\mathsf{S}}(\mathbf{A})$ *is* $\neg$*-inconsistent iff* $\mathbf{Cn}_{\mathsf{S}}(\mathbf{A})$ *is trivial.*

This extends Corollary 12 to inconsistent theories, which shows why inconsistency is undesirable of classical theories. However, some systems do not generally uphold explosion: paraconsistent logics.

DEFINITION 18 (Paraconsistency). $\mathsf{S}$ is $\neg$-paraconsistent iff $\mathsf{S}$ is not $\neg$-explosive. (I.e., for some $\mathbf{A}$, $\mathbf{Cn}_{\mathsf{S}}(\mathbf{A})$ is $\neg$-inconsistent but non-trivial.)

We may also define stronger paraconsistent systems [cf. 62, p. 8].

DEFINITION 19 (Strong paraconsistency). $\mathsf{S}$ is strongly $\neg$-paraconsistent iff $\mathbf{Cn}_{\mathsf{S}}(\{\alpha, \neg\alpha\}) = \mathbf{La}$ holds for no $\alpha$.

A logical system can be both explosive and paraconsistent. This is not the case of classical logic, which is explosive and non-paraconsistent regarding its only negation. In contrast, each of da Costa's $\mathsf{C}_n$ systems [20, 21] is $\neg^{(n)}$-explosive but $\neg^{(n-1)}$-paraconsistent, for $0 < n < \omega$.

Most definitions of paraconsistent logics involve restricting explosion, with no explicit restriction on the principle of non-contradiction [cf. 27, p. 259]. Notably, Priest's system features the latter as a theorem [cf. 71, sec. 5.2]. However, we must note that the term 'paraconsistent logic' was coined by Francisco Miró Quesada [cf. 53] to name Newton da Costa's $\mathsf{C}_n$ systems, in which both principles are restricted. It seems that both authors regarded the latter principle as the most significantly challenged by these systems. Although Definition 18 follows the usual convention, the importance of the principle of non-contradiction here must be stressed. Of course, this does not mean that adopting a paraconsistent logic commits us to *dialetheism*: the view that there are true contradictions or that some statements are both true and false. At most, it leads us to question that reasoning must proceed explosively.

## 3. Approaching inconsistencies in science

Section 3.1 opens our methodological discussion by identifying the nature of the investigation pursued here. Section 3.2 then surveys usual approaches for such an investigation, discussing some of their philosophical implications. Finally, in Section 3.3, I describe my methodological approach and provide a general outline of the structure of the inquiry.

### 3.1. Kinds of inconsistencies and investigations into them

In order for an inconsistency to be interesting for the philosophy of science, all the theories or statements involved in it must be accepted or entertained by relevant members of the scientific community at some point in time. These inconsistencies are typically classified into three groups: (a) *factual* or *theory-data inconsistencies*, which are inconsistencies between a theory and observations or data; (b) *external* or *inter-theoretical inconsistencies*, which take place between two or more theories that describe the same thing; and (c) *internal* or *intra-theoretical inconsistencies*, which characterise (self-)inconsistent theories.[1]

Now, following Šešelja [76], we distinguish three kinds of investigations into scientific inconsistencies. *Historical investigations* are descriptive in nature and aim to show whether inconsistencies have occurred in science, and how scientists have dealt with them [cf. 3, 28, 51, 59, 60, 77]. *Logical investigations* are normative and seek to explain how scientists should or should not reason in the presence of inconsistencies [cf. 15, 16, 23, 24, 26]. Finally, *methodological investigations* are also normative, focusing on the role that consistency plays in the appraisal of scientific proposals [cf. 9, 14, 44, 47, 48, 78]. I must nevertheless add that some — perhaps most — works on scientific inconsistencies are hybrid, often featuring both descriptive and normative perspectives [cf. 25, 32, 33, 70, 81].

The present inquiry relates to internal inconsistencies and combines elements of logical and methodological investigations.

[1] This classification is sometimes attributed to Priest [70, p. 144], though Batens [8, p. 354] and Bartelborth [3, pp. 95–6] proposed it more than a decade earlier. Even earlier, Gotesky [34] provided an extensive list of inconsistencies in which these three groups can be distinguished. Naming conventions for these groups vary across works.

### 3.2. Content-driven and logic-driven approaches

Two approaches are commonly discussed in the study of scientific inconsistencies: the logic-driven and the content-driven approaches. The *logic-driven approach* handles an inconsistent proposal **P** by adopting a paraconsistent underlying logic, thus maintaining its logical integration. In contrast, the *content-driven approach* retains classical logic, but transforms **P** into a prototheory with internally consistent but mutually inconsistent subdomains which interact with content-based constraints.[2]

Some authors argue that these approaches stem from divergent views on science. For example, Nickles [58] relates the logic-driven approach to the syntactic view of scientific theories and to a Copernican paradigm, in which theories are logically integrated, interpreted formal systems, and science is an endeavour aiming for a unified view of the universe. In contrast, he relates the content-driven approach to the semantic view and to a Ptolemaic paradigm, in which theories are treated as collections of models and local problems are solved within restricted domains. However, the distinction between these approaches has three key limitations.

First, the distinction covers a limited range of strategies for dealing with scientific inconsistencies. Thus, the two approaches can be further analysed along two criteria: their (a) intolerance or (b) tolerance of inconsistencies; and their enforcement of (i) unity or (ii) disunity within the domain. We can combine these dimensions into a framework, adapted from Bueno [17], in which the following four types of strategies toward scientific inconsistencies can be distinguished (see Table 1).

(a.i) *Inconsistency-intolerance with domain unity*: The inconsistencies of the original proposal are eliminated with some inconsistency-removal technology resulting in a consistent theory with a unified domain. Bueno calls this strategy *information restriction*.

(a.ii) *Inconsistency-intolerance with domain disunity*: The domain is compartmentalised into internally consistent subdomains to which we apply classical logic. Bueno calls it *compartmentalisation*.

[2] An alternative to these two approaches — although closer to the content-driven approach — is Vickers' *theory eliminativism*. Here, we 'eliminate theory-talk' by focusing solely on the specific propositions to which scientists commit [81, p. 29]. This would result in most scientific inconsistencies vanishing, as they would be absent in most of those cases. But theory eliminativism — as valuable a view on scientific inconsistencies it is — will not serve my purposes, as I am concerned precisely with internally inconsistent theories, and not with mutually consistent propositions.

| | | domain | |
|---|---|---|---|
| | | unity | disunity |
| inconsistency | intolerance | a.i | a.ii |
| | tolerance | b.i | b.ii |

Table 1. Types of strategies towards scientific inconsistencies.

(b.i) *Inconsistency-tolerance with domain unity*: The domain is not compartmentalised and inconsistencies are preserved, leading us to adopt a paraconsistent logic. Bueno calls it *dialetheism*!

(b.ii) *Inconsistency-tolerance with domain disunity*: The domain is compartmentalised into mutually (and perhaps also internally) inconsistent subdomains, whose inconsistencies interact through a paraconsistent logic. Bueno calls it *paraconsistent compartmentalisation*.

The logic-driven approach appears to align with type b.i strategies, as only a paraconsistent underlying logic would allow for a proposal's logical integration to be non-trivial. The content-driven approach, on the other hand, seems to align with type a.ii strategies, as the domain is compartmentalised in order to prevent inconsistencies from interacting with each other. Associating type a.i and type b.ii strategies to these approaches, however, proves more difficult.

On the one hand, the two approaches are incompatible with type a.i strategies. Here, we use some technology to remove the inconsistencies of a theory $\mathbf{T}$ in order to obtain consistent $\mathbf{T}'$, which would require neither paraconsistent logic nor domain compartmentalisation — as there would be no inconsistencies left to handle. Even if paraconsistent logics were used as technologies for this task, their role would be to remove inconsistencies, not to handle them.[3] On the other hand, type b.ii strategies appear to feature elements of both approaches. Thus, subdomains could be identified in consideration of the proposal's content, whose underlying logic could be classical or non-classical. These subdomains could nevertheless interact unrestrictedly through an *overlying* paraconsistent logic, as it is done in multi-deductive logics [cf. 26].

[3] Bertossi, Hunter, and Schaub [10] identify five kinds of inconsistency-tolerance technologies: consistency-checking, paraconsistent logics, argumentation systems, inconsistency analysis, and belief revision. We must nevertheless note that a technology is not equivalent to a strategy, as the former are instruments which may be used by the latter for a specific task in the handling of inconsistencies.

The second limitation of this distinction is that the logic-driven approach is not necessarily tied to the syntactic view, nor is the content-driven approach to the semantic view. In fact, what I said about logic-driven and content-driven approaches in the framework above does not align with how the syntactic and semantic views fit within it, as both views are compatible with all four types of strategies. This is more evident for the syntactic view, as all these types of strategy admit syntactic formalisation. Regarding the semantic view, its most obvious applications appear in strategies of type a.i (for the consistent theory obtained) and of type a.ii (as it is typically used in the content-driven approach). However, it can also be applied to type b.i and b.ii strategies if we recourse to a paraconsistent model theory [e.g. 1].

Finally, the distinction between logic-driven and content-driven approaches is not actually as drastic as it is often made out to be. Thus, as argued about the syntactic and semantic views regarding the analysis of scientific theories [cf. 39], logic-driven and content-driven approaches may serve different but complementary roles in the analysis of inconsistent proposals. Logic-driven approaches would be valuable for making a proposal's structure accessible to unrestricted logical analysis, while content-driven approaches would help us explain how subdomains are or can be differentiated when applying a scientific proposal.

There is nevertheless a legitimate point of contention between the proponents of the two approaches: the need of paraconsistent logics for the analysis inconsistent proposals. The logic-driven approach finds a case for this in da Costa's *principle of systematisation*, according to which '*reason is always expressed by means of a logic*' [22, p. 45]. Thus, when scientists reason about proposals which turn out inconsistent, they must do it with logical principles which do not trivialise them. Such principles would be systematised in a paraconsistent logic.

In contrast, in the content-driven approach, discovering an inconsistency in a proposal's logical integration does not mean that we have committed or must commit to its contradictions, as Smith [77] notes. Scientists, as human beings, cannot foresee all the logical consequences of their assumptions, and they can perform inferences from inconsistent proposals without knowing them to be so. Thus, logic appears to serve a corrective rather than an operative function [cf. 75, pp. 797–798]. No logic, classical or otherwise, is *operating* behind our reasoning, guiding each inference. Instead, logic functions by *correcting* errors, like when we reject a proposal after proving that it is inconsistent.[4] Such a rejection could lead to developing a possibly inconsistent prototheory based on the original inconsistent proposal, which could serve as a projection of its (consistent) substitute [77, p. 438]. Moreover, one such inconsistent proposal might even be worth accepting, according to Brown [14, p. 285], if it offers 'the best and most general' available account for its domain.

This raises the question of how the content-driven approach safeguards inconsistent proposals from trivialisation. In this regard, Frisch says that logic 'licenses' certain inferences but does not 'require' them. Hence, we do not need to resort, he says, to 'somewhat extravagant formal solutions' such as paraconsistent logics [33, p. 541]. Instead, it would suffice, according to him and Brown [14, p. 285], to place contextual limits on inference, guided by the proposal's content, in order prevent the combination of mutually inconsistent claims.

One could object here that the fact that logic does not require us to make a particular inference does not mean it is ok for us not to do it. If logic licenses us to infer both $\alpha$ and $\beta$, it would seem arbitrary to infer $\alpha$ while ignoring $\beta$. In this line, da Costa and French contend that placing these kinds of contextual limits on inference — and thus not committing to belief in the logical consequences of our premises — would put scientific knowledge 'beyond the reach of logic' [23, p. 111].

I will now outline the methodology of this paper and examine whether this point of contention is relevant to our inquiry.

### 3.3. Methodology of this paper

This paper's central question is whether inconsistent theories can, in principle, be tested. I will answer affirmatively by providing a logical reconstruction [cf. 43] of the concept of testability which applies not only to classical theories but also to some inconsistent ones. As Definition 7 construes inconsistent theories as logically integrated proposals,

[4] Incidentally, this point may serve to defend Aristotle's psychological formulation of the principle of non-contradiction against Łukasiewicz's critique [45]. In that formulation, 'it is impossible for anyone to believe that the same thing is and it is not' (*Metaphysica*, Γ 3, 1005 b 23–4). We can understand this in two ways: (i) it is impossible for anyone's beliefs to entail contradictions; and (ii) it is impossible for anyone to explicitly believe contradictions. Taking into account the point being footnoted here, Łukasiewicz's critique may not work so well against (ii). Of course, (ii) could still be challenged by pointing out that dialetheists explicitly believe in contradictory statements. But now it would be only them.

the strategy for our logical reconstruction must be inconsistency-tolerant and preserve domain unity. Thus, we need a type (b.i) strategy.

This kind of strategy presupposes that the theory is paraconsistently integrated. The reader may expect the next step to be selecting a system of paraconsistent logic and arguing why it is the best suited to the analysis of inconsistent theories. This is nevertheless unnecessary, as I do not intend to analyse the testability of any specific theory, but rather to examine the most general features an inconsistency-tolerant criterion of testability would possess. For this purpose, no assumptions about the underlying logic are needed, other than its paraconsistency.

This strategy is admittedly closer to the logic-driven. However, this should not entail an endorsement of this approach nor a defence of the need of paraconsistent logics for the analysis of scientific inconsistencies. Paraconsistent logics provide a straightforward formal means to treat inconsistent proposals as (logically integrated) theories. This suits me because I am fond of syntactic formalisations and aim to include those inconsistent theories of potential interest to dialetheists — which presumably are logically integrated. Therefore, my choice reflects practical considerations and personal taste rather than an endorsement of the logic-driven approach — let alone a commitment to a Copernican paradigm of science in opposition to a Ptolemaic one.

Furthermore, I want to avoid any connection between my chosen approach and endorsing dialetheism, as it may be suggested by Bueno's labelling of type b.i strategies as 'dialetheism'. Just as we can use paraconsistent logic without embracing dialetheism (see Section 2), we can adopt an inconsistency-tolerant approach within a unified domain without committing to this view. We only need to commit to the assumption that some proposals, despite their inconsistencies, can be analysed as (logically integrated) theories. After all, not everyone would agree that using an incomplete theory — which is often the case — entails rejecting the *tertium non datur*. Thus, my approach should be regarded as neutral with respect to dialetheism and related views.

Thus, the following sections will proceed as follows. Section 4 provides the desiderata for my testability criterion. In this context, I define a classical notion of falsifiability which will serve as a standard of classical testability, and which will accommodate some inconsistent theories. In Section 5, I review the relevant literature on the subject; particularly, I discuss previous attempts in Section 5.3. I present my own approach in Section 6 and argue that it is more successful than the previous attempts examined. I sketch an application in Section 7 using Priest's conception of motion — though it should be regarded as merely illustrative.

## 4. Desiderata for a generalised criterion of testability

A scientific proposal $\mathbf{P}$ is testable only if the empirical evidence has the potential to tip the balance towards its acceptance or rejection. On the one hand, we have the statements $\varphi \in \mathbf{Po}(\mathbf{P})$ constituting potential positive evidence for $\mathbf{P}$ — that is, whose acceptance would strengthen the case for accepting $\mathbf{P}$. On the other hand, we have the statements $\varphi \in \mathbf{Ne}(\mathbf{P})$ constituting potential negative evidence against $\mathbf{P}$ — that is, whose acceptance would strengthen the case for rejecting $\mathbf{P}$. Both kinds of statements constitute the proposal's empirical content.

Definition E (Empirical content). $\mathfrak{E}(\mathbf{A}) \overset{\text{def}}{=} \langle \mathbf{Po}(\mathbf{A}), \mathbf{Ne}(\mathbf{A}) \rangle$.

Completing this definition involves specifying $\mathbf{Po}(\mathbf{A})$ and $\mathbf{Ne}(\mathbf{A})$, which will be crucial in our discussion (see Postulates 23 and 40).

Note that I referred to the potential negative evidence as leading to *rejecting* the proposal, rather than to *falsifying* it. It would not make much sense to say that an inconsistent proposal is empirically falsifiable, since it is logically false even from some dialetheistic views. Rejection is a more appropriate term for a dialetheist like Priest, for whom a statement can be both true and false but it cannot be jointly accepted and rejected [71, p. 103; see also §19.9; cf. 72, ch. 6]. I believe this also aligns with traditional views like Popper's [cf. 65, §24] and Hempel's [35, p. 79], who were less concerned with the falsity of inconsistent theories than with the fact that their inconsistency would render them trivial. Thus, a conception of scientific testing accommodating of inconsistent theories cannot regard rejection as falsification.

Philosophical traditions diverge on how $\mathbf{Po}(\mathbf{A})$ and $\mathbf{Ne}(\mathbf{A})$ should be weighted. While falsificationists privilege the role $\mathbf{Ne}(\mathbf{A})$, inductivists give $\mathbf{Po}(\mathbf{A})$ at least as much importance. In my approach, both kinds of evidence will be important, playing different roles in some cases. I suggest six desiderata to guide our logical reconstruction of the testability notion and discuss them over the course of this section.

The first desideratum concerns the size of $\mathbf{Po}(\mathbf{A})$ and $\mathbf{Ne}(\mathbf{A})$.

Desideratum I (Richness). If $\mathbf{P}$ is a testable proposal, then the sets $\mathbf{Po}(\mathbf{P})$ and $\mathbf{Ne}(\mathbf{P})$ should be large enough.

This is a sensible requirement, as we want our proposals to anticipate and exclude numerous states of affairs. Of course, it is not immediately clear what 'large enough' should mean in this context. Popper, for example, requires a theory $\mathbf{T}$ to rule out at least one event [65, §23] — i.e., that $\mathbf{Ev}(\alpha) \subseteq \mathbf{Ne}(\mathbf{T})$ holds for some $\alpha \in \mathbf{Ob}_1$. We can generalise this idea as follows.

DEFINITION 20 (Eventfulness). $\mathbf{A}$ is positively eventful iff there is an $\alpha \in \mathbf{Ob}_1$ such that $\mathbf{Ev}(\alpha) \subseteq \mathbf{Po}(\mathbf{A})$. $\mathbf{A}$ is negatively eventful iff there is an $\alpha \in \mathbf{Ob}_1$ such that $\mathbf{Ev}(\alpha) \subseteq \mathbf{Ne}(\mathbf{A})$. $\mathbf{A}$ is eventful iff it is both positively eventful and negatively eventful.

In these terms, Popper requires negative eventfulness. However, note that all notions of eventfulness are equivalent for classical theories.

COROLLARY 21. *For all classical theories* $\mathbf{T}$: $\mathbf{T}$ *is eventful iff* $\mathbf{T}$ *is positively eventful iff* $\mathbf{T}$ *is negatively eventful.*

In what follows, eventfulness will be my standard for Desideratum I, though I concede that its adequacy may be subject to debate.

Next, we want to ensure that $\mathbf{Po}(\mathbf{A})$ and $\mathbf{Ne}(\mathbf{A})$ be kept distinct.

DEFINITION 22 (Well-defined empirical content). $\mathfrak{E}(\mathbf{A})$ is well-defined iff $\mathbf{Po}(\mathbf{A}) \cap \mathbf{Ne}(\mathbf{A}) = \{\}$, and ill-defined otherwise.

DESIDERATUM II (Coherence). The empirical content of a testable proposal should be well-defined.

The reason for this requirement is the potential ambiguity that arises in case $\varphi \in \mathbf{Po}(\mathbf{A}) \cap \mathbf{Ne}(\mathbf{A})$. In such cases, verifying $\varphi$ would support both the acceptance and the rejection of $\mathbf{A}$, thereby generating ambiguity about which epistemic attitude towards $\mathbf{A}$ would be more justified.

The following desideratum is crucial, as meeting it would support the non-arbitrariness of our definition by showing its alignment with orthodox standards of testability for classical theories.

DESIDERATUM III (Conservatism). A classical theory should be testable in the extended sense iff it is testable in the classical sense.

A well-known classical account of testability is Popper's falsifiability criterion, which I will now discuss. In order to properly understand it, we must identify three forms of falsificationism: dogmatic, methodological, and sophisticated. The differences between them are better understood if we distinguish, in a particular theory $\mathbf{T} = \mathbf{Cn}_{\mathsf{S}}(\mathbf{C} \cup \mathbf{H})$, its hard-core hypotheses in $\mathbf{C}$, its auxiliary hypotheses in $\mathbf{H}$, and its underlying logic $\mathsf{S}$. As advanced in Section 2, hard-core hypotheses are not mere generalisations, as only when combined with auxiliary hypotheses can $\mathbf{T}$ entail observational predictions.

In *dogmatic falsificationism*, if $\varphi \in \mathbf{Ne}(\mathbf{T})$ and $\varphi$ is verified, then we must conclusively reject $\mathbf{C}$. Popper never endorsed this view. Notably, as early as 1934, he argued that no 'logical proof' can conclusively show a theory's failure, since experimental results may be unreliable or insights to resolve the contradiction between the results and the theory may be lacking [65, §9]. He argued that, since $\varphi$ is logically incompatible with $\mathbf{T}$, verifying $\varphi$ falsifies $\mathbf{T}$. However, we cannot know from this which specific assumption in $\mathbf{C} \cup \mathbf{H}$ was false. We cannot even identify whether it was a hard-core hypothesis or an auxiliary one. Thus, we lack a logical means to direct the *modus tollens* at a specific assumption in $\mathbf{C} \cup \mathbf{H}$ [cf. 65, §18]. Moreover, from certain anti-exceptionalist positions about logic [cf. 36], the *modus tollens* could be directed at the very underlying logic $\mathsf{S}$ [see 73, pp. xiii–xiv; 22, *passim*; 9, pp. 171–2].

Popper, however, defended *methodological* (aka *naïve*) *falsificationism*, whereby the accumulation of negative evidence must at some point lead to the decision by scientists to reject $\mathbf{C}$. This is a decision for which some methodological rules but no conclusive logical criterion can be provided. Thus, this version of falsificationism acknowledges Neurath's [56] critique that, in practice, we can only 'shake' (*erschüttern*) $\mathbf{C}$ but never falsify it. But methodological falsificationism is also flawed, as well-corroborated hard-core hypotheses are seldom abandoned, even in the face of robust negative evidence. Such evidence is rather treated as an anomaly to be explained away or even deferred for future resolution.

Finally, in *sophisticated falsificationism*, advanced by Lakatos [42], we shift the focus from isolated theories $\mathbf{T}$ to *research programmes* $\mathfrak{R} = \langle \mathbf{T}_0, \ldots, \mathbf{T}_n \rangle$, i.e., series of theories. Here, when counter-evidence arises against an isolated theory $\mathbf{T}_i = \mathbf{Cn}_{\mathsf{S}}(\mathbf{C} \cup \mathbf{H}_i)$, we do not discard $\mathbf{C}$. Instead, we replace $\mathbf{T}_i$ with a new theory $\mathbf{T}_{i+1} = \mathbf{Cn}_{\mathsf{S}}(\mathbf{C} \cup \mathbf{H}_{i+1})$, in which the hard-core hypotheses $\mathbf{C}$ are ideally not changed but the auxiliary hypotheses $\mathbf{H}$ are. These isolated theories are not strictly falsified by experience, as even observational statements cannot be conclusively verified or falsified. However, they are falsifiable in the methodological sense outlined above, as our judgment on the truth or falsity of relevant observational statements may lead us to falsify an isolated theory. The programme $\mathfrak{R}$ and its core assumptions $\mathbf{C}$ will nevertheless persist unless a rival programme $\mathfrak{R}'$, with a totally different hard-core $\mathbf{C}'$, proves more successful. This occurs because, although an isolated theory may be incompatible with some data, hard-core hypotheses are so abstract that they can often be made compatible with any data.

Despite these differences, these three forms of falsificationism can be connected so that a dogmatic jargon can be used without endorsing dogmatic falsificationism. First, both sophisticated and methodological falsificationism regard isolated theories as shakeable. Second, the language of 'outright rejection' of dogmatic falsificationism simplifies the language of 'shaking' of methodological falsificationism well enough for our purposes. This is convenient since my theory testing discussion will concern isolated theories, which are *logically* incompatible with certain observational statements. So if this paper's jargon seems somewhat dogmatic, remember that it is embedded within a sophisticated framework, and that its aim is to provide a *logical criterion of testability* rather than describing how testability works in concrete scientific practice.[5]

That said, here is a key insight on which falsificationism is based: the logical properties of universal statements give rise to an asymmetry between the possibilities of verifying and falsifying general statements. Since no universal statement like $\forall x\, P(x)$ can be inferred from a finite amount of particular statements like $P(a_1), \ldots, P(a_n)$, no general statement can be inferred from a finite amount of observational statements. However, a general statement can be contradicted by a single observational statement, like $\neg P(a)$ contradicts $\forall x\, P(x)$. Therefore, general statements and, hence, scientific theories are only partially decidable: they can be falsified but cannot be verified [64, p. 426; cf. 65, §6].

Accordingly, a general statement $\alpha$ has many *potential falsifiers*—observational statements whose truth would imply $\alpha$'s falsity—but no *potential verifiers*—observational statements whose truth would imply $\alpha$'s truth. The closest $\alpha$ has to potential verifiers are *potential corroborators*: observational statements which logically follow from $\alpha$. Thus,

[5] For Popper's take on the logical and practical senses of falsifiability, see [67; cf. 52]. It is worth noting here that sophisticated falsificationism can address Feyerabend's claim that theories not only conceive new data but also transform previous ones [31; cf. 19]. This view suggests that we do not test a theory against theory-neutral data, but rather organise data with the help of our theory, making them theory-laden from the outset. However, nothing prevents the falsificationist from testing isolated theories against theory-laden, intersubjectively interpreted data.

a potential corroborator $\varphi \in \mathbf{Co}(\mathbf{T})$ of $\mathbf{T}$ is an observational statement which logically follows from $\mathbf{T}$; while a potential falsifier $\varphi \in \mathbf{Fa}(\mathbf{T})$ of $\mathbf{T}$ is any observational statement contradicting of $\mathbf{T}$. Thus, the classical notion of empirical content may be specified as follows:

Definition Co. $\mathbf{Co}(\mathbf{A}) \stackrel{\text{def}}{=} \mathbf{Ob}(\mathbf{A})$.

Definition Fa. $\mathbf{Fa}(\mathbf{A}) \stackrel{\text{def}}{=} \{\varphi : \neg\varphi \in \mathbf{Co}(\mathbf{A})\} \cup \{\neg\varphi : \varphi \in \mathbf{Co}(\mathbf{A})\}$.

Postulate 23 (Classical empirical content).

$$\mathbf{Po}(\mathbf{A}) \stackrel{\text{def}}{=} \mathbf{Co}(\mathbf{A}) \quad \text{and} \quad \mathbf{Ne}(\mathbf{A}) \stackrel{\text{def}}{=} \mathbf{Fa}(\mathbf{A}).$$

For example, if $\forall x\, P(x) \in \mathbf{T} = \mathbf{Cn}_{\mathsf{C}}(\mathbf{P})$ and $P(a) \in \mathbf{Ob}$, then $P(a) \in \mathbf{T}, P(a) \in \mathbf{Co}(\mathbf{T})$, and $\neg P(a) \in \mathbf{Fa}(\mathbf{T})$.

Popper's account of the empirical content relies exclusively on $\mathbf{Fa}(\mathbf{A})$. However, $\mathbf{Co}(\mathbf{A})$ is useful for formulating the following result:

Corollary 24. *For all classical theories* $\mathbf{T}$*:* $\mathbf{T}$ *is consistent iff* $\mathfrak{E}(\mathbf{A})$ *(as per Postulate 23) is well-defined.*

Now, in his *Logik*, Popper states two conditions for falsifiability: (a) negative eventfulness (see Definition 20) and (b) consistency [65, §24]. In a later work, Popper discussed the prospects of using non-classical logics in empirical science and argued that such a move would diminish a theory's possibilities of falsification, rendering us 'not critical enough' [66, p. 305; cf 79]. Thus, a third Popperian condition for falsifiability is that (c) classical integration. In sum, Popper's testability criterion, which I call *P-falsifiability*, may be defined as follows:

Definition P (P-falsifiability). $\mathbf{T}$ is P-falsifiable iff (a) it is negatively eventful, (b) it is consistent, and (c) it is $\mathsf{C}$-integrated.

Note that, Definition P satisfies Desideratum I, since negative eventfulness is equivalent to eventfulness given Corollary 21. It also satisfies Desideratum II, since, given Corollary 24, condition (b) is equivalent to requiring that $\mathfrak{E}(\mathbf{T})$ be well-defined. It would nevertheless be incorrect to claim that it satisfies Desideratum III. Rather, it serves to specify it. Thus, a testability notion defined for inconsistent proposals would satisfy Desideratum III provided its notion of rejectability is coextensive with P-falsifiability in the domain of classical theories.

The question now arises as to why extending P-falsifiability serves our aim of devising a testability criterion for inconsistent theories. The main reason is that, if successful, the promised extension would have emerged from a framework highly intolerant of inconsistent theories, excluding them from the outset. Thus, the way would be paved for alternative criteria which may be more inconsistency-tolerant. The next desideratum is precisely the applicability of this notion to inconsistent theories:

DESIDERATUM IV (Inconsistency tolerance). Some inconsistent proposals should be testable, including some observationally inconsistent ones.

While a positive answer to our question does not require that our testability notion fulfils the clause on observationally inconsistent theories, it would be more inconsistency-tolerant if it did. In this regard, Corollary 24 suggests a minor amendment to Definition P that would yield this positive answer, though still failing to satisfy this clause. Accordingly, my definition of falsifiability will replace Popper's consistency requirement with that of having a well-defined empirical content [cf. 4, 5].

DEFINITION F (Classical falsifiability). $\mathbf{A}$ is (classically) falsifiable iff (a) it is eventful and (b) $\mathfrak{E}(\mathbf{A})$ (as per Postulate 23) is well-defined.

COROLLARY 25. *For all classical* $\mathbf{T}$*:* $\mathbf{T}$ *is falsifiable iff* $\mathbf{T}$ *is P-falsifiable.*

Corollary 25 means that, when it comes to classical theories, Definition F satisfies Desiderata I and II to the same degree that Definition P does. Therefore, it also satisfies Desideratum III, which is why I will adopt Definition F as my classical definition of falsifiability.

With regard to Desideratum IV, note the following result.

COROLLARY 26. $\mathbf{A}$ *is observationally consistent iff* $\mathfrak{E}(\mathbf{A})$ *is well-defined.*

This means that it is observationally inconsistent proposals — not all inconsistent proposals — that have ill-defined empirical contents.

This finding invites us to consider inconsistent theories which are observationally consistent. At first glance, one might regard such theories as irrelevant to our inquiry. However, this would be a mistake. In a key paper on inconsistency in empirical science, Priest states that 'there certainly have been inconsistent theories where the inconsistencies are located internally, away from observational consequences' [70, 122]. This focus is justified also from a traditional view. As Hempel notes, the problem with inconsistent theories lies in their ability to imply 'any conceivable *observational* prediction' [35, p. 79, my emphasis].

The next result concerns the conditions under which observationally consistent proposals would be falsifiable.

Theorem 27. *If* **T** *is negatively eventful and observationally consistent, then* **T** *is falsifiable — regardless of whether* **T** *is consistent or not.*

This theorem shows that Definition F successfully extends Definition P to certain inconsistent theories. Thus, assuming that some inconsistent theories are both negatively eventful and observationally consistent — which is perfectly possible if the underlying logic is paraconsistent — results in a positive answer to the paper's question.

Another important result is that, if we accept some inconsistent theories as falsifiable, then some non-classically integrated theories will also be falsifiable: notably, theories which are paraconsistently integrated.

Corollary 28. *For all inconsistent* **T***: if* **T** *is falsifiable, then it is* $\mathsf{P}$*-integrated, where* $\mathsf{P}$ *is paraconsistent.*

This relates to another desideratum that may be put forward:

Desideratum V (Logical tolerance). The broader the range of admissible underlying logics for testable theories, the better.

Classical falsifiability clearly satisfies Desideratum V much better than P-falsifiability. It does not, by definition, exclude theories that cannot be classically integrated and, as implied by Corollary 28, it admits theories with paraconsistent underlying logics. However, as the following result shows, classical falsifiability fails to fully satisfy Desideratum IV.

Theorem 29. *All observationally inconsistent* **T** *are unfalsifiable.*

This theorem shows the limits of classical falsifiability in relation to inconsistency-tolerance: it cannot serve as a testability criterion for observationally inconsistent theories. I will address this issue in Section 6.

Finally, an additional desideratum may be put forward concerning the possibility of testing inconsistent proposals *qua inconsistent*. Testing **P** qua inconsistent means testing its inconsistent consequences, i.e., those statements $\alpha$ such that $\alpha, \neg\alpha \in \mathbf{P}$. If there is potential evidence providing ground for accepting or rejecting those consequences, then **P** is said to be testable qua inconsistent.

Desideratum VI (Dialetheic sensitiveness). Some inconsistent proposals should be testable qua inconsistent.

This desideratum becomes relevant only if we adopt such views as factual or observational dialetheism, which I discuss in Section 5.

## 5. Previous approaches to testing inconsistencies

It will be helpful to name a couple of philosophical views related to our discussion. Let us call *factual dialetheism* the view that there are true contradictions about the world and *observational dialetheism* the view that there are true contradictory observational statements. Both da Costa and Priest have defended the possibility of at least one of these views, but it seems that neither committed to them.

While our enquiry requires no commitment to these views, they may be presupposed depending on how we regard the inconsistencies of the theories tested. If we regard them as flaws to be removed when we find its consistent substitute, these views are not presupposed. However, if we regard them as features reflecting true contradictions in the world, we are presupposing factual dialetheism; and also observational dialetheism if those contradictions are expressed by observational statements. This difference in how we regard the theory's inconsistency affects how our enquiry applies in both scenarios: while testing the theory qua inconsistent would be required in the latter case, this would not be necessary in the former. Thus, corroborating an inconsistent empirical theory qua inconsistent would provide support to factual dialetheism, while rejecting it qua inconsistent would undermine it.

I will proceed as follows. Section 5.1 presents Priest's argument for the possibility of observational dialetheism, that is, for the observability of some contradictions. Then, Section 5.2 discusses da Costa's argument for the possibility of factual dialetheism, that is, his hypothesis that this view is verifiable. Finally, Section 5.3 reconstructs some previous attempts to define the empirical content of inconsistent theories.

### 5.1. On observational dialetheism

In what follows, I freely reconstruct Priest's [69] argument that there might be observable contradictions or, in our terms, contradictory observational statements, though without necessarily endorsing his views.[6] To this end, I will assume certain theses about observability — some of which Priest may not share. The first such thesis is the following:

Thesis I. *Not all $\varphi \in$ **Ob** are currently observable.*

[6] See [29] for a more extensive take on the observability of contradictions, featuring a discussion on the possibility of depicting impossible states of affairs.

While *currently observable* means 'observable with currently available instruments', *observable* means 'observable with instruments that are theoretically possible to construct but may not be presently available'. What an observational statement denotes is observable in this latter sense, as theories usually predict phenomena beyond current testing capabilities — though often providing a basis for developing the necessary instruments in the future. For instance, the existence of gravitational waves was predicted by relativity theory already in 1916, but they were not actually observed until 2015 — almost a century later!

Another example is related to the concept of musical pitch, which was already implicit in Pythagoras. We may assume that, *mutatis mutandis*, he would have been able to understand the following two statements:

STATEMENT 30. *The pitch of this whistle is* $C_5$ *(≈ 523 Hz).*

STATEMENT 31. *The pitch of this whistle is* $C_9$ *(≈ 8372 Hz).*

However, while he could have verified Statement 30 by ear — within some margin of error — he had no chance to do so with Statement 31: his ears could not capture a pitch that high and he lacked additional instruments to indirectly do it. Yet, what Statement 31 describes is, in principle, observable and we now have the instruments to do so.

Thesis I helps us show that, for all $\varphi \in \mathbf{Ob}$, there must be some $\psi \in \mathbf{Ob}$ logically entailing $\neg\varphi$. (Note that I am not claiming, yet, that $\neg\varphi$ itself is observational.) For suppose no $\psi$ logically entails $\neg\varphi$. Therefore, **La** cannot express an observable state of affairs incompatible with $\varphi$ either because: (i) $\varphi$ necessarily holds or (ii) **La** is not expressive enough. Since necessary claims are not precisely observ*able*, case (i) is irrelevant. And so is case (ii), since we want **La** to express all observational statements in the sense of Thesis I. Hence our second thesis:

THESIS II. *For all* $\varphi \in \mathbf{Ob}$*, there is a* $\psi \in \mathbf{Ob}$ *such that* $\neg\varphi \in \mathbf{Cn}_{\mathsf{S}}(\{\psi\})$.

To argue that $\neg\varphi$ itself is observational, let us turn to Priest's remark that 'inference may well play some role in the rational reconstruction of how [*seeing that*] proceeds' [69, p. 441], which means that some logical notions may be involved in such a reconstruction. To illustrate, assume that Ted and Ned are identical twins and consider the disjunction:

STATEMENT 32. *Ted is playing or Ned is playing.*

We can observe what Statement 32 describes by observing one of these identical twins playing, even without knowing which one [70, p. 121]. Of course, this does not mean that all disjunctions of observational statements are also observational. It is at least dubious that a logically true statement like $\varphi \vee \neg\varphi$ denotes an observ*able* states of affairs, as such a statement would be *a priori* verified — unless we think that the *tertium non datur* may not hold for some observational statements.

However, it seems safe to say that any disjunction of observational statements $\varphi_1 \vee \cdots \vee \varphi_n$, which it is neither a logical truth nor a logical absurdity expresses an observable state of affairs in a similar way that Statement 32 does. A noteworthy kind of observational disjunction involves cases like the one described in the following thesis:

THESIS III. *Let $\mathcal{P}' \subseteq \mathcal{P}_1$ be a finite set of predicates standing for observable properties that are mutually exclusive and exhaustive for any observable object in our domain. Hence, for all $\mathcal{P}'' \subset \mathcal{P}'$ and $t \in \mathcal{O}$:*

$$P_1(t) \vee \cdots \vee P_n(t) \in \mathbf{Ob},$$

*where $P_i \in \mathcal{P}''$ and $n$ is the cardinality of $\mathcal{P}''$.*

Now, suppose $\mathcal{P}'$ and $\mathcal{P}''$ above are such that, for exactly one $Q \in \mathcal{P}$:

$$\mathcal{P}' - \mathcal{P}'' = \{Q\}.$$

In this case, the following equivalence intuitively holds for all $t \in \mathcal{O}$:

$$\neg Q(t) = P_1(t) \vee \cdots \vee P_n(t).$$

Here, $\neg Q(t)$ is equivalent to a disjunction stating that $t$ has at least one of the properties incompatible with $Q$. Consequently, verifying any disjunct $P_i(t)$ would entail verifying $\neg Q(t)$, which makes $\neg Q(t)$ observational in its own right.

This analysis of negated formulae $\neg\alpha$ is limited to specific cases where $\alpha$ is of the form $P(t)$. However, the results can be generalised to all observational formulae as follows:

THESIS IV. *If $\varphi \in \mathbf{Ob}$, then $\neg\varphi \in \mathbf{Ob}$.*

I do not have the space to develop a full argument for this thesis, but the reader may extrapolate it from the previous analysis.

Now, we may think that, in line with what I said about disjunctions, a conjunction of observational statements $\varphi_1 \wedge \cdots \wedge \varphi_n$ is observational provided it is neither a logical truth nor a logical absurdity. But it would be highly controversial to assert this in general. First, there is arguably a limit to how large $n$ can be for $\varphi_1 \wedge \cdots \wedge \varphi_n$ to be observed by someone. Second, we must also consider Heisenberg's indeterminacy principle. However, the last thesis we need is not that all conjunctions of observational statements are observational, but only that some conjunctions of mutually contradictory observational statements are also observational. Since I do not endorse this thesis, I will just concede it and refer to Priest's writings [e.g., 69, 70] for its justification:

Thesis V. *For some* $\varphi \in \mathbf{Ob}$, $\varphi \wedge \neg\varphi \in \mathbf{Ob}$.

The argument presented so far is intended to support the theoretical possibility of observable contradictions. Nonetheless, there are some reasons to doubt its relevance to observational dialetheism. Thus, some have challenged this view by noting the absence of perceptions directly corresponding to what is described by negative statements. The argument dates back to Vasil'ev [80], who in the early 20th century argued that we know that a ball is *not red* based not on a direct 'negative perception' of its non-redness, but on a perception that it is, say, green all over, which is incompatible it being red anywhere. Although da Costa [22, pp. 127–129] noted this remark without critique, he put forward an argument for the possibility of observational dialetheism [22, §3.3], which I will discuss in Section 5.2. Bobenrieth critiqued that argument, asserting that '[n]egation does not reflect or represent something in reality but something that we do with reality' [12, p. 508] and that there are no 'negative facts' to be experienced, as negation is 'an operation given by virtue of our category schemes' [11, p. 407].

In defence of Priest and da Costa, we may argue that the former'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*. Thus, there is no problem with saying that we see that something is not red by seeing that it is green all over. Moreover, Priest emphasises the role of category schemes in our perception of Escherian visual illusions as if they represented inconsistent states of affairs. In doing so, however, he does not address how we would tell whether an apparently inconsistent state of affairs corresponds to a genuinely inconsistent one.

The above raises the question of whether Priest's case for (the possibility of) observational dialetheism is semantic or metaphysical [cf. 46, p. 265]. *Metaphysical dialetheism* posits that certain aspects of reality are themselves inconsistent, whereas *semantic dialetheism* locates inconsistencies in the relationship between language and the world. Curiously enough, Priest characterised Bobenrieth as a semantic dialetheist [71, p. 302, fn. 32], contrary to the latter's intended view.

It is not clear whether Priest is a semantic or a metaphysical dialetheist himself. At one time he said that 'the world as such is not the *kind* of thing that can be consistent or inconsistent' because '[c]onsistency is a property of statements', but that we can extend the domain of these properties by calling the world consistent iff 'any true purely descriptive statement about [it] is consistent', and inconsistent otherwise [71, p. 159]. Moreover, against Mares' presupposition that he is a metaphysical dialetheist, he objects that his book *In Contradiction* is 'largely neutral on most relevant issues', as his account of truth 'is not a correspondence theory' [71, p. 302]. At another time, however, Priest said that 'the observable world is consistent' since 'our perceptions of the world are entirely consistent' [69, p. 463]. Thus, it seems that his position is compatible with the two aforementioned kinds of dialetheism. Nonetheless, the last quote suggests that, if he were a factual or observational dialetheist, he would be so semantically rather than metaphysically.

Now, it seems to me that a true contradiction within semantic dialetheism involves, to quote Isaac Newton, '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' [57, p. 212]. I cannot delve into this point here, but I fail to see how semantic dialetheism challenges scientific or philosophical orthodoxy other than through ultimately terminological disputes. But it is up to dialetheists to judge whether this aligns with their views.

I have presented and examined an argument supporting the possibility of observational dialetheism. Whether or not we accept this argument, it provides a foundation for admitting contradictory observational statements. That said, it does not establish that there are true contradictions about the world. In the next section, I discuss an argument suggesting that this hypothesis is more plausible than we may think.

### 5.2. On factual dialetheism

In the early 1980s, before the term 'dialetheism' was coined, da Costa put forward a case for the possibility of true contradictions about the world, i.e., factual dialetheism [22, sec. 3.3]. Building on Łukasiewicz's critique of the principle of non-contradiction [45], he argued that factual dialetheism can only be established a posteriori. He made two claims that are worth distinguishing, and which I will phrase as follows:

da Costa's weak claim about factual dialetheism.
It is possible to verify factual dialetheism.

da Costa's strong claim about factual dialetheism.
It is easier to verify than to reject factual dialetheism.

I will discuss each of these claims in turn.

In favour of the weak claim, da Costa argues that, in order to verify what we have called factual dialetheism, it would be enough to verify only one contradiction about the world [22, p. 208]. But if this is so, the question naturally arises: why have scientists not accepted the double-slit experiment as verification of one such contradiction? The results of this experiment are easily so interpreted: light is both a wave and a particle and, given the well-known theoretical duality, also not a wave and not a particle. But this *verified contradiction* was rejected by scientists precisely because it was inconsistent according to the theoretical assumptions in place. Something *being* both a wave and not a wave is not an observable phenomenon under these assumptions — it is a sign that some assumption is wrong [cf. 2]. Thus, instead of accepting this contradiction about the world, scientists developed quantum theory, in which the results of this experiment are perfectly consistent.

Part of the problem is that da Costa phrases his remarks in a dogmatic jargon, as presupposing that we test statements in isolation. We nevertheless know that verifying a statement presupposes its coherence within some theoretical framework. Hence, verifying a factual contradiction would presuppose its coherence within a given research programme — but we know of no research programme in empirical science coherent with contradictions. This does not mean that da Costa's weak claim cannot be reformulated within a sophisticated understanding of empirical testing. As explained in Section 4, I myself use a dogmatic jargon in order to avoid a more convoluted sophisticated language.

Such a formulation can be borrowed from Priest [70, pp. 124–5]. He argues, probably following Kuhn [40, pp. 205–6], that a research programme is evaluated based on it possessing or lacking certain *good marks*, including consistency, simplicity, explanatory power, etc. Given the diversity of good marks, we should not necessarily establish some of those as non-negotiable requirements, but ponder them together when choosing among rival programmes. For instance, let $\mathfrak{R} = \langle \mathbf{T}_1, \ldots, \mathbf{T}_n \rangle$ be a programme where each $\mathbf{T}_i$ is inconsistent. Thus, if $\mathfrak{R}$ has more good marks than its consistent rival programmes, its contradictory hypotheses may be regarded as scientifically accepted contradictions about the world, thus corroborating factual dialetheism. Moreover, let $\varphi, \neg\varphi \in \mathbf{Co}(\mathbf{T}_i)$ for each $\mathbf{T}_i$ and some $\varphi$. In that case, verifying $\varphi$ and $\neg\varphi$ would mean verifying an observable contradiction and, therefore, observational dialetheism. This expresses da Costa's weak hypothesis in more sophisticated terms, making it more plausible than it initially appeared to be.

However, even if we accept this version of the weak claim, there are issues related to the strong claim. In its support, da Costa points out an asymmetry about the testability of factual dialetheism, opposite to the one Popper pointed out about general statements: verifying factual dialetheism requires verifying a single factual contradiction, whereas rejecting it demands rejecting infinitely many of them. Thus, it is easier to verify factual dialetheism than to reject it [22, p. 208].

The asymmetry, nonetheless, may be stronger than da Costa wants. For it is not clear what kind of statement $\beta$ would qualify as an *alternative* to a contradiction $\alpha \wedge \neg\alpha$, in the sense that accepting $\beta$ would commit us to reject $\alpha \wedge \neg\alpha$. Neither $\alpha$ nor $\neg\alpha$ would qualify, since they follow from $\alpha \wedge \neg\alpha$ in most paraconsistent logics. The next candidate is $\neg(\alpha \wedge \neg\alpha)$. This probably would not be a candidate for Priest, as it is a tautology in his system [71]. But it may be for da Costa, since, although $\alpha \wedge \neg\alpha$ is logically acceptable in his $\mathsf{C}_1$ system, the conjunction:

$$(\alpha \wedge \neg\alpha) \wedge \neg(\alpha \wedge \neg\alpha)$$

is logically absurd. This very formula is nevertheless acceptable in his $\mathsf{C}_2$, though the conjunction with its negation is absurd. This latter conjunction, however, is acceptable in his $\mathsf{C}_3$, and so forth.

This raises the question of which $\mathsf{C}_n$ system should we use in order to determine an alternative statement to a factual contradiction. As long as we do not have good answers for this, it would be difficult to tell whether $\neg(\alpha \wedge \neg\alpha)$ or any other formula would force us to reject $\alpha \wedge \neg\alpha$. Hence, it seems that once a contradictory statement is granted logical acceptability, it becomes immune to rejection.

We may gain further insight into this issue by examining Kabay's argument on the impossibility to deny trivialism [38, ch. 3]. He argues that denying $\alpha$ requires asserting some $\beta$ alternative to $\alpha$, with the following being a necessary condition for alternative statements:

Postulate 33 (Kabay's condition). No conjunct $\alpha_i$ of a conjunction $\alpha_1 \cdots \wedge \alpha_n$ is an alternative to it.

This condition seems sensible, but it immediately leads to Kabay's desired conclusion. Since trivialism can be construed as the conjunction of all the statements of the language **La** one is a trivialist about, no $\alpha \in \mathbf{La}$ can be an alternative to it. Ergo, trivialism is undeniable.

There is nevertheless one reason why Kabay's condition is not acceptable in science: it allows for the generation of *absurdity shields* around any proposal. The more absurd a proposal is — i.e., the more mutually inconsistent information it incorporates — the more immune to criticism and rejection it becomes. For instance, let $\alpha$ be a deniable/rejectable statement and $\beta$ an alternative to $\alpha$ (which may be $\neg\alpha$). The conjunction $\alpha \wedge \beta$ is presumably absurd and self-defeating, but not if we accept Kabay's condition. Otherwise, trivialism would be self-defeating, since, by asserting trivialism we are also denying it.[7]

Now, one might be attracted to dialetheism and even to trivialism on philosophical grounds, but it is quite clear that absurdity shields should have no place in empirical science. Counter-evidence is crucial in empirical science, and there is no counter-evidence against a proposal that can assimilate any claim without withdrawing any of its assumptions. One thing is to be not critical enough [cf. 66, p. 305], and quite another to render criticism impossible. Trivialism and factual dialetheism both seem to fail in this respect — though the former to a far greater extent.

The above seems to undermine the viability of factual and observational dialetheism in the philosophy of science. However, this is of no consequence for our study, as it does not rely on endorsing these views.

### 5.3. On the empirical content of inconsistent theories

Not much has been said about the testability of inconsistent theories. The traditional position on the matter was probably first stated by Popper, who discarded these theories from the realm of falsifiable ideas because they allow for the derivation of 'any conclusion we please' [68, p. 72]. Hempel was more specific, in the sense that those inconsistencies entail 'any conceivable observational prediction' [35, p. 79]. Perhaps the

[7] We may amend Kabay's condition retaining its intuitive appeal, while avoiding the undeniability of trivialism, a follows: if the conjunction $\alpha_1 \wedge \cdots \wedge \alpha_n$ is logically acceptable, then none of its conjuncts $\alpha_i$ is an alternative to it.

nicest thing that has been said about inconsistencies in science from a traditional perspective is Mosterin's claim that 'the uneasiness with [...] inconsistency led to a great theoretical progress' [55, p. 299].

In this section, I examine the only two attempts I know to define the empirical content of inconsistent theories from an inconsistency-tolerant or *counter-traditional* [cf. 25] perspective. Despite its merits, I am deliberately disregarding Frisch's [33] acceptability conditions for inconsistent theories in this review. This is because it differs from my aims in not treating inconsistent proposals as (logically integrated) theories.

The first attempt belongs to Piscoya [63], who proposes a testability criterion by which inconsistent theories are refutable iff they are absolutely consistent, that is, non-trivial [63, p. 67]. Piscoya's approach provides no clear definition of the empirical content of inconsistent theories. First, he said that a statement $\varphi$ such that $\varphi \notin \mathbf{T}$ would be incompatible with $\mathbf{T}$ and, hence, it would be reasonable to interpret that $\varphi \in \mathbf{Ne}(\mathbf{T})$ [63, p. 67]. Later — in a personal communication — he clarified that $\mathbf{Ne}(\mathbf{T})$ should consist of $\mathbf{T}$'s *potential trivialisers*, i.e., those $\varphi$ which, conjoined with $\mathbf{T}$, entail every $\alpha$. In both cases, the underlying logic chosen for $\mathbf{T}$ has a role in the determination of $\mathbf{Ne}(\mathbf{T})$.

Piscoya's first suggestion entails that an incomplete theory $\mathbf{T}$ such that $\varphi, \neg\varphi \notin \mathbf{T}$ would be refuted by either $\varphi$ or $\neg\varphi$, which is counter-intuitive. Hence, I will reconstruct his second suggestion.

DEFINITION 34. $\mathbf{Tr}_{\mathsf{S}}(\mathbf{A}) \overset{\text{def}}{=} \{\varphi \in \mathbf{Ob} : \mathbf{La} \subseteq \mathbf{Cn}_{\mathsf{S}}(\mathbf{A} \cup \{\varphi\})\}$.

POSTULATE 35 (Absolute empirical content).

$$\mathbf{Po}(\mathbf{Cn}_{\mathsf{S}}(\mathbf{A})) \overset{\text{def}}{=} \mathbf{Co}(\mathbf{Cn}_{\mathsf{S}}(\mathbf{A})) \quad \text{and} \quad \mathbf{Ne}(\mathbf{Cn}_{\mathsf{S}}(\mathbf{A})) \overset{\text{def}}{=} \mathbf{Tr}_{\mathsf{S}}(\mathbf{A}).$$

A testability definition based on Postulate 35 promises to satisfy Desideratum III, given the following result:

COROLLARY 36. *For all classical* $\mathbf{T} = \mathbf{Cn}_{\mathsf{C}}(\mathbf{P})$*:* $\mathbf{Tr}_{\mathsf{C}}(\mathbf{P}) = \mathbf{Fa}(\mathbf{T})$.

There are nevertheless a couple of issues with this approach.

First, Definition 34 has a much narrower scope than Definition Fa, as it is defined only for logically integrated sets. In fact, we need to explicitly indicate the underlying logic $\mathsf{S}$, as it is in principle possible for a theory $\mathbf{T}$ that there be $\mathbf{A} \neq \mathbf{B}$ and $\mathsf{S}_1 \neq \mathsf{S}_2$ such that:

$$\mathbf{Cn}_{\mathsf{S}_1}(\mathbf{A}) = \mathbf{T} = \mathbf{Cn}_{\mathsf{S}_2}(\mathbf{B}) \quad \text{but} \quad \mathbf{Tr}_{\mathsf{S}_1}(\mathbf{A}) \neq \mathbf{Tr}_{\mathsf{S}_2}(\mathbf{B}).$$

This renders it impossible to determine the empirical content of a prototheory — should we wish to do so — as it would be undefined.

Second, Postulate 35 has counter-intuitive consequences. For instance, let $\mathbf{T} = \mathbf{Cn}_{\mathsf{S}}(\mathbf{P})$ and $\varphi$ stand in the following relations:

$$\varphi \in \mathbf{Co}(\mathbf{T}), \quad \neg\varphi \notin \mathbf{Co}(\mathbf{T}), \quad \neg\varphi \in \mathbf{Fa}(\mathbf{T}), \quad \neg\varphi \notin \mathbf{Tr}_{\mathsf{S}}(\mathbf{P}).$$

If we were to verify $\neg\varphi$, $\mathbf{T}$ would not be refuted under this approach, although intuitively it should be. Hence, this approach would result in theories with underlying logics highly resistant to trivialisation being shielded from rejection, as they would have few potential trivialisers.

Of course, not all paraconsistent logics are so resistant to trivialisation. For instance, in da Costa's $\mathsf{C}_1$ system, if $\varphi \in \mathbf{T} = \mathbf{Cn}_{\mathsf{C}_1}(\mathbf{P})$, then $\neg{*}\varphi \in \mathbf{Tr}_{\mathsf{C}_1}(\mathbf{P})$. But other systems, like Jaśkowski's non-adjunctive logic $\mathsf{J}$ [37], go as far as blocking the rule of conjunction introduction. Thus, even if $\varphi, \neg\varphi \in \mathbf{T} = \mathbf{Cn}_{\mathsf{J}}(\mathbf{P})$, it may be that $\varphi \wedge \neg\varphi \notin \mathbf{T}$. As a result, the rule $\mathbf{Cn}_{\mathsf{J}}(\{\alpha \wedge \neg\alpha\}) = \mathbf{La}$, which in $\mathsf{J}$ corresponds to explosion, would be inapplicable for $\alpha = \varphi$. The situation would be more severe if the underlying logic were strongly paraconsistent (see Definition 19).

We may regard this as evidence that certain logics are simply ill-suited for empirical science. By preventing trivialisation, regardless of how inconsistent or absurd a proposal may be, these systems can be seen as enabling the absurdity shields I warned about in Section 5.2. However, in the spirit of Desideratum V, it is better not to exclude a theory solely on the basis of its underlying logic.

The second attempt, put forward by Priest, is more successful in this regard, even advancing conditions under which inconsistent theories would be rejectable qua inconsistent. In his account, an inconsistent theory such that $\varphi \wedge \neg\varphi \in \mathbf{Co}(\mathbf{T})$ is to be rejected if do not observe $\varphi \wedge \neg\varphi$ [70, p. 125]. But since $\varphi, \neg\varphi$, and $\neg(\varphi \wedge \neg\varphi)$ are logically compatible with $\varphi \wedge \neg\varphi$ and $\mathbf{T}$ in Priest's account, it is not clear what would constitute an alternative to $\varphi \wedge \neg\varphi$. Consequently, observing a state of affairs incompatible with $\varphi \wedge \neg\varphi$ would seem useless for rejecting $\mathbf{T}$ and, hence, Priest proposes to reject $\mathbf{T}$ if we fail to observe $\varphi \wedge \neg\varphi$.

Priest's approach may be formalised introducing additional symbols $\mathsf{O}, \sim \in \mathcal{S}_1$, where $\sim$ is a negation which may differ in properties from $\neg$. $\mathsf{O}\varphi$ means that $\varphi$ was observed, while $\sim\mathsf{O}\varphi$ means that $\varphi$ was not observed — in both cases, under conditions in which $\varphi$'s observation was expected. Thus, Priest's view of the empirical content of inconsistent theories may be reconstructed as follows:

DEFINITION 37. $\mathbf{Co}'(\mathbf{A}) \stackrel{\text{def}}{=} \{\mathsf{O}\varphi : \varphi \in \mathbf{Ob}(\mathbf{A})\}$.

DEFINITION 38. $\mathbf{Re}'(\mathbf{A}) \stackrel{\text{def}}{=} \{\sim\mathsf{O}\varphi : \varphi \in \mathbf{Ob}(\mathbf{A})\}$.

POSTULATE 39 (Dialetheic empirical content).

$$\mathbf{Po}(\mathbf{A}) \stackrel{\text{def}}{=} \mathbf{Co}'(\mathbf{A}) \quad \text{and} \quad \mathbf{Ne}(\mathbf{A}) \stackrel{\text{def}}{=} \mathbf{Re}'(\mathbf{A}).$$

It is not clear whether a testability criterion grounded in Postulate 39 would satisfy Desideratum III. It seems that not, since the identities:

$$\mathbf{Co}'(\mathbf{A}) = \mathbf{Co}(\mathbf{A}) \quad \text{and} \quad \mathbf{Re}'(\mathbf{A}) = \mathbf{Fa}(\mathbf{A}),$$

do not hold for classical theories — or any theory — as $\mathsf{O}$ and $\sim$ are not defined in $\mathsf{C}$. Nonetheless, it may be easy to reconstruct $\mathbf{Co}(\mathbf{A})$ and $\mathbf{Fa}(\mathbf{A})$ from $\mathbf{Co}'(\mathbf{A})$ and $\mathbf{Re}'(\mathbf{A})$, respectively — though doing so may complicate the overall definition. Setting this concern aside, there seems to be no formal issues with this approach, provided $\mathsf{O}\neg\varphi$ ($\neg\varphi$ is observed) and $\sim\mathsf{O}\varphi$ ($\varphi$ is not observed) are not logically equivalent.

The problem with this approach is that it is either unnecessary or inapplicable, depending on whether we verify $\mathsf{O}\neg\varphi$ (i) by observing some $\psi$ incompatible with $\varphi$ or (ii) by observing $\neg\varphi$ directly.

In case (i), we do not need to interpret our results as an observation of both $\varphi$ and $\neg\varphi$ in order to corroborate an inconsistent theory containing them, but just to observing $\varphi$ and $\psi$. But such an observation could be managed like the double-slit experiment, by reconstructing our assumptions so that $\varphi$ and $\psi$ are no longer incompatible. Thus, the empirical content of the resulting theory could be defined classically, without complicating our language with additional symbols.

In case (ii), we need an account on how $\neg\varphi$ could be non-observed without inference playing a role. Priest proposed an example of this: we see that 'Pierre is *not* in the room' by failing to see him in an empty room [70, p. 120]. I find this example unsatisfactory, as we do not merely fail to see Pierre in the room: we see *an empty room*. No room where Pierre is present can be in the extension of 'empty room'. Hence, when we are seeing an empty room, we are actually seeing something incompatible with Pierre begin in such a room. Similarly, a non-observation of $\varphi$ would be inferred from a report containing raw data interpreted as incompatible with $\varphi$. However, even if we accept this as an example of seeing without inference playing a role, we still need to explain how, in this sense, we could see that Pierre both is and is not in the room. Unfortunately, Priest offers no argument in this regard, which leaves us with no account on how we would observe or fail to observe both $\varphi$ and $\neg\varphi$ directly.

All that said, I admit that Priest's account has at least one strength: if we were to observe $\varphi \wedge \neg\varphi$, the low probability he admits for such a state of affairs [cf. 71, §8.4] would arguably give $\mathbf{T}$ a great support qua inconsistent. Notwithstanding this concession, the two approaches to the empirical content of inconsistent theories reviewed here have proven unsatisfactory. In the next section, I shall introduce my own approach.

## 6. Generalised concept of testability

Devising a testability criterion for an observationally inconsistent $\mathbf{T}$ presupposes defining, first, its empirical content. No problem seems to result from defining $\mathbf{Po}(\mathbf{T}) = \mathbf{Co}(\mathbf{T})$, as in Postulate 23. On the contrary, it would be convenient, as there would be some $\varphi$ such that $\varphi, \neg\varphi \in \mathbf{Co}(\mathbf{T})$, which would give us the chance to corroborate $\mathbf{T}$ qua inconsistent, as an observational dialetheist would want.

$\mathbf{Ne}(\mathbf{T})$, however, requires a different approach. The cases in which $\varphi \in \mathbf{Fa}(\mathbf{T})$ is such that $\varphi \notin \mathbf{Co}(\mathbf{T})$ are unproblematic, as it makes perfect sense that $\varphi \in \mathbf{Ne}(\mathbf{T})$. The complication arises in case $\varphi \in \mathbf{Co}(\mathbf{T}) \cap \mathbf{Fa}(\mathbf{T})$, whose verification would support both the acceptance and rejection of $\mathbf{T}$ from a classical perspective. To resolve these cases, I will invoke Don Quixote's *principle of mercy*:

> *In cases where the reasons to save or condemn something or someone are in one rank, the more commendable action is to save them, for it is ever more praiseworthy to do good than to do ill.*[8]

Faced with a paradoxical law — remarkably akin to the Liar's Sentence — which entails a man's execution and acquittal, Sancho Panza uses this principle to resolve the contradiction by sparing the man's life.

A similar approach, I propose, may be applied to theory testing: when confronted with a verified $\varphi \in \mathbf{Co}(\mathbf{T}) \cap \mathbf{Fa}(\mathbf{T})$, we are in case where $\mathbf{T}$ seems to be worthy of both acceptance and rejection. Hence,

[8] 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* II.51).

following the principle of mercy, we should not reject $\mathbf{T}$ in this case, that is, $\varphi \notin \mathbf{Ne}(\mathbf{T})$. Accordingly, where $\mathbf{Re}(\mathbf{T})$ is the set of *potential rejectors* of $\mathbf{T}$, the following specification of empirical content appears justified.

DEFINITION Re. $\mathbf{Re}(\mathbf{T}) \stackrel{\text{def}}{=} \mathbf{Fa}(\mathbf{T}) - \mathbf{Co}(\mathbf{T})$.

POSTULATE 40 (Rejectionist empirical content).

$$\mathbf{Po}(\mathbf{A}) \stackrel{\text{def}}{=} \mathbf{Co}(\mathbf{A}) \quad \text{and} \quad \mathbf{Ne}(\mathbf{A}) \stackrel{\text{def}}{=} \mathbf{Re}(\mathbf{A}).$$

For instance, if $\varphi, \neg\varphi \in \mathbf{T}$, then we have $\varphi, \neg\varphi \in \mathbf{Co}(\mathbf{T}) \cap \mathbf{Fa}(\mathbf{T})$ but $\varphi, \neg\varphi \notin \mathbf{Re}(\mathbf{T})$. Hence, verifying $\varphi, \neg\varphi$, or both will not result in rejecting $\mathbf{T}$, which is consistent with the following result:

COROLLARY 41. *For all* $\mathbf{A}$, $\mathfrak{E}(\mathbf{A})$ *(as per Postulate 40) is well-defined.*

The concept of rejectability can thus be defined as follows:

DEFINITION R (Rejectability). $\mathbf{A}$ is rejectable iff (a) $\mathbf{A}$ is eventful and (b) $\mathfrak{E}(\mathbf{A})$ (as per Postulate 40) is well-defined.

Note that Definitions F and Fa, and Postulate 23 are the special cases of Definitions R and Re, and Postulate 40, respectively, where $\mathbf{T}$ is observationally consistent. Thus, as the following theorem shows, Corollaries 25 and 26, and Theorem 27 are recovered, *mutatis mutandis*.

THEOREM 42. *If* $\mathbf{A}$ *is observationally consistent, then* (a) $\mathbf{Re}(\mathbf{A}) = \mathbf{Fa}(\mathbf{A})$ *and* (b) $\mathbf{A}$ *is rejectable iff* $\mathbf{A}$ *is falsifiable.*

This means that rejectability satisfies Desiderata I–III. It also fully satisfies Desideratum IV, as it admits the testability of certain observationally inconsistent theories. Moreover, Corollary 28 can also be recovered, and it is clear that, overall, rejectability meets Desideratum V much better than classical falsifiability.

The question now arises as to whether or to what extent does rejectability satisfy Desideratum VI. Since we have allowed for there to be $\varphi$ such that $\varphi, \neg\varphi \in \mathbf{Co}(\mathbf{T}) = \mathbf{Po}(\mathbf{T})$, it is clear that $\mathbf{T}$ can be corroborated qua inconsistent. However, it cannot be rejected qua inconsistent in our framework, as there can be no $\varphi$ such that $\varphi, \neg\varphi \in \mathbf{Re}(\mathbf{T}) = \mathbf{Ne}(\mathbf{T})$ — that is, no inconsistent consequence of $\mathbf{T}$ can be its potential rejector. Thus, Desideratum VI is only partially fulfilled. I nevertheless argue that this limitation is not due to a limitation of our framework, but to the logical limitation that inconsistent theories are only partially testable qua inconsistent: we may corroborate them but not reject them qua inconsistent. I will return to this point in the concluding remarks.

Now, the eventfulness requirement may be too strong for some inconsistent theories, as inconsistencies may arise among certain occurrences within an event. If so, we may discard inconsistent proposals with otherwise significant empirical contents, but which are not fully eventful. We may address this by defining weaker concepts of rejectability requiring weaker kinds of eventfulness relative to the notions of pseudo-event or quasi-event, which I will outline informally. A *pseudo-event* results from subtracting some occurrences from an event, provided it contains infinitely many occurrences. A *quasi-event* is a kind of pseudo-event which results from subtracting a *finite* set of occurrences from an event. Thus, a theory is *pseudo-rejectable* or *quasi-rejectable* iff it has a well-defined empirical content and it is pseudo-eventful or quasi-eventful, respectively.

With our testability criterion established, the following algorithm explains its implementation for testing proposals and theories.

**Testing algorithm.**

1. Start with a $\mathbf{P}$ (of our expected theorems), an ordered list of systems $\langle \mathsf{S}_0, \ldots, \mathsf{S}_n \rangle$, and $\mathbf{Da} \stackrel{\text{def}}{=} \{\}$. Let $\mathbf{T}_i \stackrel{\text{def}}{=} \mathbf{Cn}_{\mathsf{S}_i}(\mathbf{P})$ and set $i = 0$.
2. Check if $i > n$.
   2.1. If so, reject $\mathbf{P}$ and stop — we have run out of logics.
   2.2. Else, go to Step 3.
3. Check if (a) $\mathbf{T}_i$ is rejectable and (b) $\mathbf{Re}(\mathbf{T}_i) \cap \mathbf{Da} = \{\}$.
   3.1. If so, go to Step 4 – in order to start testing $\mathbf{T}_i$.
   3.2. Else, set $i = i + 1$ and go back to Step 2.
4. Check if $\mathbf{T}_i$ is observationally inconsistent.
   4.1. If so, go to Step 5.
   4.2. Else, go to Step 6.
5. Conduct a test on yet untested $\varphi, \neg\varphi \in \mathbf{Co}(\mathbf{T}_i)$.
   5.1. If both $\varphi$ and $\neg\varphi$ are verified, note that $\mathbf{T}_i$ was corroborated qua inconsistent and go to Step 6.
   5.2. Else, note nothing and go to Step 6.
6. Conduct a test on a yet untested $\varphi \in \mathbf{Re}(\mathbf{T}_i)$.
   6.1. If $\varphi$ is verified, let $\mathbf{Da} \stackrel{\text{def}}{=} \mathbf{Da} \cup \{\varphi\}$, reject $\mathbf{T}_i$, set $i = i + 1$, and go to back Step 2.
   6.2. Else, go back to Step 4 — i.e., keep testing $\mathbf{T}_i$ until rejected.

Some remarks are required to clarify the rationale of this algorithm.

First, we start in Step 1 with a proposal $\mathbf{P}$, which remains stable, and a list of logical systems, which could be exhausted at some point if $n \in \mathbb{N}$, as anticipated in Step 2.2. The ideal ordering of the list should follow our preference of logic, with $\mathsf{S}_i$ being preferred to $\mathsf{S}_j$ for all $i < j$. Thus, $\mathsf{S}_0 = \mathsf{C}$ would normally be the case.

Second, note that the algorithm stops when $\mathbf{P}$ is rejected. If internal inconsistency is not reason enough to reject $\mathbf{T}_i$, then the inconsistency of $\mathbf{T}_i$ with the data is probably not reason enough to reject $\mathbf{P}$. Now, if $\mathbf{P}$ gets finally rejected, we may consider retrying the process with a subproposal $\mathbf{P}' \subseteq \mathbf{P}$ or add new logical systems to our list. In the latter case, we should probably judge at some point that logic should no longer be revised for the sake of $\mathbf{P}$ — unless we do not think such a point exists.

Third, $\mathbf{Da}$ records the verified $\varphi \in \mathbf{Re}(\mathbf{T}_i)$ which led to rejecting previous versions of $\mathbf{T}_i$. The reason $\mathbf{Re}(\mathbf{T}_i) \cap \mathbf{Da} = \{\}$ is required for testing $\mathbf{T}_i$ is that we do not want to test a theory we already know should be rejected — as it has verified potential rejectors.

Fourth, if $\mathbf{T}_i$ is observationally consistent, Step 5 will never be used. Hence, as justified by Theorem 42, we may proceed using Definition F, corresponding to classical falsifiability and just test those $\varphi$ in $\mathbf{Fa}(\mathbf{T}_i)$. In fact, classical testing is the special case of this algorithm where $n = 0$ and $\mathsf{S}_0 = \mathsf{C}$. However, if $\mathbf{T}_i$ is observationally inconsistent, then determining the members of $\mathbf{Re}(\mathbf{T}_i)$ will be a less straightforward task. The previous case was easy, as $\mathbf{Fa}(\mathbf{T}_i)$ is, by Definition Fa, the set of the contradictories of the formulae in $\mathbf{Co}(\mathbf{T}_i)$. But in the case of observationally inconsistent theories, proving that $\varphi \in \mathbf{Re}(\mathbf{T}_i)$ requires proving (a) $\varphi \in \mathbf{Fa}(\mathbf{T}_i)$ and (b) $\varphi \notin \mathbf{Co}(\mathbf{T}_i)$. The latter is usually more difficult, as it involves proving that $\varphi$ is not an $\mathsf{S}_i$-consequence of $\mathbf{P}$.

Finally, note that Steps 5 and 6 are written as if statements were tested one by one and, once tested, they cannot be reconsidered. However, in practice, we test multiple observational consequences of a theory simultaneously, and we may revisit them multiple times. Moreover, we may try $\mathbf{T}_{i+1}$ before rejecting $\mathbf{T}_i$ or even abandon $\mathbf{P}$ before rejecting all $\mathbf{T}_i$, as we may consider other proposals or research programmes. Hence, the algorithm is not meant to be taken literally. It simply offers a one-dimensional approximation of how the actual process would work.

## 7. Testing inconsistent motion

I will now examine how an inconsistent theory would be tested, using Hegel-Priest's conception of motion and change [71, §§11–2]. This is no proper scientific theory, but it will work as an example.

This conception is based upon a principle that Priest 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' [71, p. 166]. This principle — which Priest finds 'clearly' innocuous — seems to apply in cases where we have a time interval $(t_0, t_n)$ such that, at each $t_i < t_n$, a 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, Priest provides no justification of this principle other than his "feeling that, if something violated [it], the behaviour at the limit would be, in some sense, capricious" [71, p. 166].[9]

Priest applies this principle to understand what happens at the *instant of change*. This concept is illustrated by Priest as the time $t_n$ 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 must be both in motion and not in motion at $t_n$, the instant of change between these two situations.

Now, let $\mathbf{M}$ be a proposal containing the expected consequences of this conception, and let $\varphi$ satisfy Thesis V and represent the observational statement that '$a$ is in motion at $t_n$'. Moreover, let $\langle \mathsf{S}_0, \ldots, \mathsf{S}_4 \rangle$ be an ordered list of logical systems, where $\mathsf{S}_0 = \mathsf{C}$ and $\mathsf{S}_1, \ldots, \mathsf{S}_4$ are paraconsistent. Finally, assume that $\varphi, \neg\varphi \in \mathbf{Cn}_{\mathsf{S}_i}(\mathbf{M})$, for $i = 0, \ldots, 4$, and that $\mathbf{Cn}_{\mathsf{S}_i}(\mathbf{M})$ is rejectable, for $i = 1, \ldots, 4$. Note that $\varphi, \neg\varphi \in \mathbf{Fa}(\mathbf{T}_i) \cap \mathbf{Co}(\mathbf{T}_i)$ and, hence, $\varphi, \neg\varphi \notin \mathbf{Re}(\mathbf{T}_i)$, for $i = 0, \ldots, 4$.

Let us apply our algorithm to $\mathbf{M}$. By Step 1, we let $\mathbf{Da} \overset{\text{def}}{=} \{\}, \mathbf{T}_i \overset{\text{def}}{=} \mathbf{Cn}_{\mathsf{S}_i}(\mathbf{M})$, and set $i = 0$. Thus, we start with $\mathbf{T}_0 = \mathbf{Cn}_{\mathsf{S}_0}(\mathbf{M})$. Since

[9] Priest seems to misapply LCC, as becomes clear when considering a key part of L'Huilier's formulation, which he neglects: "And if a variable quantity, susceptible of limit, increasingly tends towards possessing a certain property, the closer it comes to its limit, so that there is no limit to its capacity to enjoy this property, its limit enjoys this property." [41, p. 167]. Here, continuity is not about retaining a property at the limit, but about the limit inheriting a property from the approach towards it. This makes it natural, within the LCC, for an object to be in motion throughout an interval, yet be at rest at the limit [cf. 6, 13].

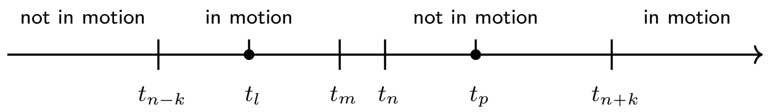


Figure 1. Observations verifying that $a$ is and is not in motion at $t_n$.

$i = 0 \leqslant 4$, we go from Step 2 to Step 3. Now, as each $\mathbf{T}_i$ is inconsistent by assumption and $\mathsf{S}_0 = \mathsf{C}$, we have that $\mathbf{T}_0$ is not rejectable. Hence, as indicated by Step 3, we set $i = 1$, obtaining $\mathbf{T}_i = \mathbf{T}_1 = \mathbf{Cn}_{\mathsf{S}_1}(\mathbf{M})$, and go back to Step 2. Again, since $i = 1 \leqslant 4$, we go from Step 2 to Step 3. Now, since $\mathbf{Da} \stackrel{\text{def}}{=} \{\}$ was not modified, we have that $\mathbf{T}_1 \cap \mathbf{Da} = \{\}$, and since $\mathbf{T}_1$ is rejectable by assumption, we must go to Step 4. Since $\mathbf{T}_1$ is observationally inconsistent by assumption, we go to Step 5. Now, we test $\varphi, \neg\varphi \in \mathbf{Co}(\mathbf{T}_1)$: if both are verified, we note that $\mathbf{T}_1$ was corroborated qua inconsistent and go to Step 6; else, we also go to Step 6. Now we test some $\psi \in \mathbf{Re}(\mathbf{T}_1)$: if verified, we let $\mathbf{Da} \stackrel{\text{def}}{=} \{\psi\}$, reject $\mathbf{T}_1$, set $i = 2$, and go back to Step 2 to retry the process with $\mathbf{T}_2$ (if necessary, we keep retrying with $\mathbf{T}_3$ and $\mathbf{T}_4$, but rejecting $\mathbf{T}_4$ would entail rejecting $\mathbf{M}$ altogether); else, we go back to Step 4 to keep testing $\mathbf{T}_1$.

Now, something interesting would happen if we adopt the *spread hypothesis*, entertained by Priest, in this testing process. This hypothesis posits that no body can be localised to just one point at a specific instant, but rather to the points it occupies in its vicinity [71, p. 177]. We would then need to test our hypothesis not at exactly $t_n$ but within an interval $(t_{n-k}, t_{n+k})$, where $k > 0$. In other words, we would need to observe whether $a$ is and/or is not in motion at times near enough $t_n$. In this case, we may argue that $\varphi$ was verified and, hence, that $\mathbf{T}$ was corroborated qua inconsistent given the following conditions (see Figure 1):

(i) $a$ is consistently observed to be *not in motion* before $t_{n-k}$;
(ii) $a$ is consistently observed to be *in motion* after $t_{n+k}$; and
(iii) there is a time $t_m \in (t_{n-k}, t_{n+k})$, such that:
  (a) $a$ is observed to be *in motion* at a time $t_l \in (t_{n-k}, t_m)$, and
  (b) $a$ is observed to be *not in motion* at a time $t_p \in (t_m, t_{n+k})$.

It is nevertheless clear that these observations could also suggest a bad measurement or even backward causation [cf. 69, p. 179]. Regarding the former, we know that scientists are used to work with imprecisions in their measurements and they *estimate* the right (consistent) value from several tests, instead of assuming that inconsistencies hold at the locus of measurement [cf. 2]. Regarding the latter, it would be interesting to ask physicists whether they would be more willing to believe that there is backward causation or that the world is inconsistent.

Notwithstanding all that, if we were interested in Priest's conception of motion and change as an inconsistent scientific theory, this paper would offer a logical criterion of testability for its full formalisation.

## 8. Concluding remarks

I showed that a logical criterion of testability can be formulated which applies to inconsistent theories as well as to consistent ones. This criterion can be applied regardless of whether we regard our theory's inconsistencies as flaws to be removed or as features reflecting inconsistencies in the world. In the former case, we may use it as 'some kind of crutch' for examining the empirical content of a fruitful theory that turned out inconsistent, while we do not have a consistent substitute [Piscoya in 54, p. 195]. In the latter case, however, I argued that inconsistent theories can only be corroborated but not rejected qua inconsistent.

This portrays factual dialetheism as a view that can only be corroborated, but not rejected or even shaken, thereby placing it outside the realm of science. It cannot be objected here — drawing on Reichenbach [74], Neurath [56], and Kuhn [40] — that falsificationism proposes an oversimplified account of science. If observational dialetheism is unscientific in this oversimplified account, it can hardly be expected to do better in a more sophisticated one. The challenge is nevertheless open.

The central question of this paper stems from the potential of paraconsistent logics to broaden the scope of scientific theories to include inconsistent ones. I have refrained from taking a stance on the existence or scientific value of such theories, on the alleged paraconsistency of scientific reasoning, and on the importance of the consistency requirement in science. Pursuing this *way of enquiry* presupposes no particular stance on these issues, as it may yield valuable insights for both traditional and counter-traditional views within the philosophy of science.

To the former group, I would remind the importance of developing broader notions that can work with diverse logical systems. Such a flexibility must always be welcome. To the latter group, I would argue that, if inconsistent theories are to have a place in science, they must still meet some form of testability criterion. An empirical theory that cannot be tested is like a map of nowhere — potentially interesting but useless for orientation.

In any case, I pursued here a way of enquiry that is conceivable — so the Goddess said. Even if this way is without report — as the Goddess also remarked — that has never stopped a philosopher from writing.

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