# Concerning Peter Vickers' Recent Treatment of 'Paraconsistencitis'

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## 1 Introduction

Inconsistencies have long been considered one of the major challenges for the explication of scientific reasoning and rationality. Consistency has traditionally been taken to be a necessary requirement for accepted scientific theories (e.g. Popper (1959), but also more recently Douglas (2009)). In view of classical logic anything can be derived from an inconsistent set of premises by the rule *Ex Contradictione Quodlibet* (ECQ)[1]. This motivates the requirement that scientists are to reason from consistent sets of premises. Philosophers of science (especially in the post-Kuhnian time) have challenged this traditional stance by pointing out cases of inconsistencies in scientific theories, and by arguing for the importance of tolerating them in specific circumstances. As a result, many have suggested that methodological and heuristic requirements need to be weakened in order to allow for inconsistencies in, for instance, young, underdeveloped theories (e.g. Feyerabend (1975), Lakatos (1978), Nickles (2002)), and more generally that:

(a) inconsistencies commonly appear in science;

(b) scientists, as rational agents, sometimes accept and reason from inconsistencies.

In view of this, one of the major questions that attracted philosophical interest is how scientific rationality in view of inconsistencies is to be explicated.[2] This challenge has especially been interesting for logicians and it resulted in the development of non-classical logical frameworks, which prevent logical explosion in case of inconsistent premise sets. A family of such logics, known under the name of paraconsistent logics, has been developed in different schools, such as the Brazilian one (initiated by the work of Newton da Costa), the Australian one (including the school of dialethism[3], most prominently advocated by Graham Priest), the U.S. American one (around relevance logicians such as Anderson and Belnap), the Belgian one (embedded in the program of adaptive logics, pioneered by Diderik Batens), etc.

Peter Vickers' *Understanding Inconsistent Science* (Vickers (2013)) reopens the debate by shading new critical light on both claims (a) and (b). In what follows we first give a brief overview of Vickers' book, focusing on his methodology and the way in which he challenges the above two claims (a) and (b) (Section 2). We then turn to our critical commentary. We

[1] *Ex Contradictione Quodlibet*, sometimes also called *Ex Falso Quodlibet* or *the principle of explosion* is an inference rule valid in classical logic, according to which from a contradiction follows anything: $A, \sim A \vdash B$.

[2] The view that scientific rationality does not always conform to the ideal of consistency fits into the more general philosophical leaning towards *bounded rationality*, according to which rational agents reason in view of limited cognitive resources and abilities (see e.g. Doyle (1992)).

[3] Dialethism is a view that there are true contradictions, i.e. sentences which are both true and false.

start with Vickers' criteria for what is to count as a significant inconsistency (Section 3), and proceed towards his conclusions regarding the relevance of paraconsistent logics for an explication of scientific reasoning (Section 4). We show that while Vickers raises a number of important points, his attack on the paraconsistent modeling is insufficiently elaborated and leaves some fundamental questions open. In Section 5 we conclude that despite our critical points, Vickers' book is a highly valuable contribution to the literature on inconsistencies in scientific reasoning and to the philosophy of science in general.

## 2 In Search of Inconsistencies that Matter

Vickers' investigation is anchored in a thorough reexamination of classical examples of inconsistent science (Chapters 3-6): Bohr's theory of the atom, classical electrodynamics, Newtonian cosmology, and the early calculus. Another chapter (Chapter 7) is devoted to some additional examples: Aristotle's theory of motion, Olber's paradox, the case of classical electrons, and Kirchhoff's theory of diffraction. Each of the cases is examined by means of a careful and detailed historical analysis. As such, Vickers' book is a prime example of an integrated history and philosophy of science.

**Theory Eliminativism** A distinguishing feature of Vickers' approach to the case studies is his account of 'theory eliminativism' (introduced in Chapter 2). Vickers starts off by noticing that a significant part of the debates on whether a certain scientific theory is inconsistent is rooted in a disagreement over the question which propositions constitute the given theory. As a result, investigations of inconsistencies are often sidetracked by disputes on what the theory in question is or what theories in principle are. Vickers' proposal is that philosophical discussions be refocused from theories as units of philosophical and historical appraisal to analysanda which he dubs 'pointedly grouped propositions'. There are two criteria for the selection of such analysanda: first, they need to be *historically relevant* in the sense of belonging to a set of commitments of an individual scientist – a member of the relevant scientific community – at a single point in time; second, the propositions need to be *doxastically or instrumentally significant* either by belonging to the doxastic commitments of the relevant scientists or by belonging to their instrumental commitments (i.e. by having been used together for a certain epistemic purpose). By requiring from the analysis of inconsistencies in science to focus on propositions which are pointed in this sense, Vickers makes sure that inconsistencies are recognized as important if and only if they have played a certain doxastic or instrumental role in the relevant history of science.

**Against 'Paraconsistencitis'** [4] In view of this methodological approach, Vickers examines whether the inconsistencies that have been identified in the philosophical literature satisfy the above two criteria. The outcome of his analysis is often striking. First of all, he shows that some of the classic examples of inconsistencies cannot be related to doxastic commitments of the relevant scientists. For example, with regard to Bohr's theory of the atom, Vickers identifies five possible inconsistencies, four of which are usually discussed in the literature (Chapter 3). He shows that none of these four can be related to doxastic commitments of the scientists, nor is it clear that scientists at all reasoned from inconsistencies in each of these cases. In contrast, the fifth one, pointed out by Pauli, is usually omitted in the literature on inconsistencies in this historical episode even though it regards the doxastic commitments of the relevant scientists. However, as Vickers shows, Pauli presented this inconsistency as a sign that the theoretical

[4] The term 'paraconsistencitis' stems from Meheus (2003) where it is used to indicate an epistemic bias: scholars suffering from it have a tendency to see inconsistencies everywhere.

system required an immediate revision (p. 68), rather than taking it as a part of the beliefs *from which* scientists are to reason on, in search for a consistent replacement. In this way Vickers challenges the received view on this case study, which interpreted it in accordance with claim (b).

The inconsistency commonly related to the early calculus is another example of what Vickers takes to be a confusion in the received view (Chapter 6). In search for possible inconsistencies Vickers shows that neither can they be found in the algorithms used to make derivations, nor in the justifications provided for those algorithms (including Newton's and Leibniz's ones). Finally, Vickers investigates whether the relevant scientists made an 'as if' commitment to inconsistent propositions, i.e. whether they reasoned 'as if' they believed in inconsistent propositions. He argues that even at this level –which in any case is not a matter of a doxastic commitment– it is not easy to find an inconsistency (except perhaps in the case of Johann Bernoulli). Instead, scientists had a consistent story to tell when interpreting their derivations, even if it seems that they reasoned 'as if' they were committed to inconsistent premises. Once again, Vickers questions the validity of claim (b).

Moreover, the variety of case studies he examines poses a challenge to claim (a). What Vickers shows is that, once we eliminate the 'theory talk', "most of the 'inconsistent theories' commonly put forward are not, on inspection, inconsistent in any significant sense after all" (p. 246). Not only that, but the diversity of historical cases shows that any attempt at constructing an overarching 'theory of inconsistent theories' is likely to fail (p. 218): while some inconsistencies were already present in the propositions used by the scientists and were only subsequently detected, others appeared only once a specific question gave rise to propositions which turned out to be inconsistent with the already accepted ones; while in some cases, the question leading to the recognition of an already existing inconsistency was asked by the relevant scientific community, in others such a question was posed only at a later point; while some concerned the idealized experimental set up, others concerned propositions which were considered to be candidates for the truth, etc. (p. 218-226).

Having questioned both claims (a) and (b), Vickers draws conclusions regarding the formal modeling of scientific reasoning. To this aspect of Vickers' work, which will, no doubt, for some (such as logicians) be the most exciting and the most controversial one, we will devote Section 4. Before we turn to it we will take a closer look at a related problem: Vickers' notion of significant inconsistencies.

## 3 A Conceptual Problem with 'Significance'

As we have already mentioned, Vickers starts his investigation by introducing two criteria for what is to count as 'significant' (sometimes also 'important', 'interesting') pointedly grouped propositions, namely historical significance on the one hand and doxastic and/*or* instrumental significance on the other hand. Accordingly, an inconsistency is significant (important, interesting) in case it occurs within such a significant pointedly grouped set of propositions.

However, in the course of the book, this characterization of significance is narrowed down in such a way that merely inconsistencies that concern doxastic commitments are counted as significant (important, interesting). For instance, in the case study of Bohrs theory of the atom Vickers concludes that, even if scientists sometimes reasoned from inconsistencies in their instrumental commitments, none of them would be important or interesting (p. 52–56). Similarly, "in the case of Kirchhoff's theory, although we certainly do have an inconsistency in 'pointedly grouped propositions', the character of the community's commitment to some of the propositions means that the inconsistency is not especially interesting or important" (p. 216-217). What we have here are inconsistencies which occur in a significant pointedly grouped set of propositions (according to the two initial criteria), but which Vickers nevertheless doesn't deem 'interesting or important'.

This has several noteworthy consequences. Following Vickers' analysis, many of the discussed historical examples (in which inconsistencies have been successfully identified) concern primarily instrumental commitments. As such they are not to be considered significant (important, interesting) according to the narrowed notion of significance, although being part of significant pointedly grouped propositions. Clearly, in order to substantiate his criticism of (a) Vickers needs to convince the reader that in the majority of his case studies there are indeed no occurrences of significant inconsistencies. In view of his initial account of significance, admitting the existence of inconsistencies that concern instrumental commitments seems to undermine his criticism of (a).

In view of this we may ask: is Vickers' criticism of (a) thus unjustified and somewhat forced? In other words, is there a good reason why the characterization of 'significant inconsistencies' should be narrowed to those that concern doxastic commitments, excluding those based on instrumental commitments? Now, in Vickers' narrowed conception, unless an inconsistency can be found in the doxastic commitments of scientists, there is nothing interesting or important about it. This is due to the fact that such inconsistencies give rise to a type of reasoning that is common for idealizations, approximations, abstractions, etc. (what he calls '*as if*-reasoning'). But, in his view, this type of reasoning is already well-understood in the literature and for its explication we do not need to take a recourse to anything exotic like a paraconsistent logic.

Is Vickers' reply convincing? In order to address this problem we need to say a bit more on logical modeling. This will be done in the following section.

## 4 Classical or Paraconsistent Logic?

**Vickers' stance** According to Vickers, scientists discover an inconsistency in their doxastic commitments in one of two ways: they recognize an inconsistency that was thus far hidden in their commitments, or they come to accept a new proposition which is inconsistent with some of their prior commitments. If that happens, a scientist takes a 'stop-discharge-and-revise'-approach: "one stops what one is doing and declares that something in the original assumption set needs to be changed" (p. 69) and, if possible, one immediately engages in a belief revision process. In other words, scientists "look back over the inconsistent propositions, and consider what can be dropped, or replaced with minimal damage to accumulated scientific success" (p. 74).[5] Importantly, they do not simply accept the inconsistencies and reason from them by means of changing their logic in order to avoid logical explosion.

Moreover, often scientists engage in what Vickers calls *as if*-reasoning: they treat some of the propositions in question as idealizations, simplifications, approximations, abstractions, etc. (sometimes as a result of withdrawing their doxastic commitment). As such they reason with the propositions *as if* they have committed to them as candidates for the truth while they in fact only have an instrumental or a pragmatic commitment to them (p. 240). As a result, scientists in this case reason more carefully, never merely conjoining the inconsistent propositions to derive just anything via ECQ.

[5] See also Harman (1986) who introduces various constraints on rational belief revision, among them minimality of change. He also discusses a *Logical Inconsistency Principle* according to which "Logical inconsistency is to be avoided" (p. 11). Note though that according to Harman this principle is not without exceptions: he mentions the Liar paradox as an example.

In view of these considerations Vickers rejects the relevance of paraconsistent logics (in short, PLs) for the modeling of scientific reasoning:

> scientists don't avoid the conflict [in their commitments] by changing their *logic*. As the case studies have shown, classical deductive logic was used. Nor does it seem plausible that scientists *should* handle these cases by changing their logic. All that might be said is that these cases *can* be reconstructed by changing the logic, but it is hard to see what understanding might be gained by such a reconstruction. (p. 238, italics in original)

Interestingly, the fact that scientists do not end up with a logical explosion in face of inconsistencies is one of the main arguments used in favor of the adequacy of PLs for an explication of scientific reasoning. Thus, Vickers' conclusion may come to many as a surprise. Let us see how this argument goes and check whether Vickers' stance can withstand its force.

**Why paraconsistent logic?** Paraconsistent logicians often focus on the question which *consequence relation* adequately describes the reasoning of scientists.[6] For instance, according to Brown (2002) (p. 630–631), the consequence relation functions as a closure condition on our commitments, telling us which consequences we are committed to if we accept a set of sentences.[7] Hence, the claim that scientists sometimes accept inconsistent sets of premises (see (b)) but at the same time do not accept just any conclusion (or more carfully phrased: do not deem sensible just any conclusion or do reject at least some statements) is an argument against the adequacy of classical logic (CL) and in turn a positive argument in favor of PLs.[8]

Though Vickers does not directly engage in a discussion of this argument, we may identify two possible replies in his book.

**Reply 1** First, he notices that "explosion can only come in *via* contradictions, and reasoning will be halted as soon as contradictories are derived" (p. 104). This is somewhat puzzling: were scientists to follow CL it seems they would at least be licensed (on logical grounds) to derive just anything. However, what holds them back according to Vickers is "physical common-sense" and that "further inferences will carry zero scientific value" (ibid.).

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[6] PLs have been defended also on other grounds (see e.g., Priest et al. (2013) for an overview), for instance, on the basis of arguments that deny that Modus Ponens or Disjunctive Syllogism are truth preservational. To start off our discussion it is though sufficient to focus on the argument above. Similarly, the adequacy of classical logic has been criticized as a suitable logic for the modeling of scientific reasoning for different reasons beside its explosive character (see e.g., Weingartner (1994)).

[7] Various complications arise if we try to give a more refined account of the exact nature of the commitment in question since the reasoning capacities of agents are restricted and they may not be aware of certain consequences of a given premise set and/or they may not be able to memorize and process all the consequences. For instance, Field (2009) characterizes the commitment in terms of degrees of beliefs and 'obvious' implications:

> If it's obvious that $A1, \ldots, An$ together entail $B$, then one ought to impose the constraint that $P(B)$ is to be at least $P(A1) + \ldots + P(An) - (n-1)$, in any circumstance where $A1, \ldots, An$ and $B$ are in question. (Field, 2009, p. 260)

Beall (2013) phrases the commitment in terms of a constraint according to which one is not to reject consequences: "If $X \vdash A$, then it's irrational to accept $X$ and reject $A$." (Beall, 2013, p. 4).

[8] Meheus (2002a) offers a more fine-grained analysis distinguishing between *sensible* and *acceptable* consequences. The latter are those statements that also hold according to the intended consistent replacement of a theory, while the former are the ones provided by the consequence relation of a logic. She argues that sometimes inferences are sensible though not acceptable (e.g., they may serve a heuristic value when searching for a consistent replacement). Moreover, for acceptance, derivability is not sufficient but sometimes extra-logical criteria have to be considered. When modeling the reasoning of scientists a focus on acceptability is fruitless since the intended consistent replacement may not be yet available but rather be the outcome of both analyzing the given propositions and of extra-logical considerations.

**Some Concerns** But doesn't that mean that CL simply licenses too many inferences and thus cannot be considered to be an adequate deductive standard? A first rejoinder may be given by the fact that it is not expected nor demanded that a rational reasoner actually infers every proposition in the consequence set. For instance, it doesn't make much sense to apply weakening again and again (from $A$ to derive $A \vee B$) although it is licensed by CL. According to Harman (1986), there are overriding principles that govern the economy of reasoning (such as Clutter Avoidance) given the limitations of (human) reasoners. We may phrase this as follows: there is no need to clutter our minds with every logical consequence that we are aware of or that is obvious to us. So couldn't Vickers just argue that the consequences of ECQ fall under Clutter Avoidance and are thus no danger to the reasoner who employs CL? It is important though that every proposition that *can* be logically inferred (and the reasoner is aware of this) is to be considered a valid or sensible consequence *as long as we have reasons to accept our premises* (but see Reply 2 below): we cannot (at least on logical grounds) reject its status as a consequence. In other words, the logic provides us with a reason to accept it as logically implied by the premises. Thus, Reply 1, according to which inferences by ECQ carry zero value and are thus always rejected, seems to be rather playing into the hands of the paraconsistent logicians.

**Reply 2** There is a second, stronger reply. Vickers' 'stop-discharge-and-revise' consideration (in view of which scientists who face contradictions stop reasoning from the inconsistent propositions, withdraw their doxastic commitment in the given premises, and start revising their beliefs) seems to undercut the basic assumption underlying the above argument in favor of PLs. According to Vickers, scientists (doxastically) reject premises as soon as a contradiction arises from them. By "physical common-sense" the contradiction is unacceptable. But then, at least one of the premises has to be rejected since they together imply the contradiction. Hence, the fact that a contradiction has been derived gives scientists a strong reason to doxasically reject their premises. This way the danger of logical explosion is immediately avoided and we need not assume that scientists (need to) change their logic (in order to restrict their inferences).

**To infer classically or not to infer classically?** One worry is whether from this observation it really follows that scientists employ CL or whether Vickers' 'stop-discharge-and-revise' consideration is also compatible with assuming that scientists use a PL. We can reconstruct Vickers' stance in terms of three phases. Phase 1 in which scientists reason from their premises $A1, \ldots, An$ to a contradiction $B$. Phase 2 in which they reason that $B$ is unacceptable. Phase 3 in which they resolve the situation by dropping their doxastic commitment to $A1, \ldots, An$. According to Vickers, the reasoning in Phase 1 is governed by CL. Phase 2 is governed by extra-logical considerations. Phase 3 is a belief revision process.

Concerning Phase 1 we have to critically ask for a convincing reason to suppose that scientists arrived at the contradiction by means of CL. The inference steps they employed may be supported by a PL as well. To illustrate the point take a simple premise set $\Gamma = \{p \vee q, {\sim}q, {\sim}p\}$. Via disjunctive syllogism we get from $p \vee q$ and ${\sim}q$ to $p$. With ${\sim}p$ we have the contradiction $p \wedge {\sim}p$. However many PLs also allow one to derive this contradiction: e.g., scientists could have employed quasi-classical logic (Hunter (2000)), **CL**$-$ (Batens & Provijn (2001)), or **AN** (Meheus (2000)).

Now it is true that many well-known PLs do not license this inference. Take Priest's **LP** (Priest (2006), previously and independently introduced in Asenjo et al. (1966)): it does not validate Disjunctive Syllogism and Modus Ponens and hence it does not license the inference from $p \vee q$ and ${\sim}q$ to $p$. The reason is that in **LP** $\vee$-addition is (unrestrictedly) valid. Were **LP** to allow for Disjunctive Syllogism or Modus Ponens, it would be explosive in view of contradictions. Indeed, in our example we could derive ${\sim}p \vee r$ (for some arbitrary $r$) by $\vee$-addition from ${\sim}p$

and use Disjunctive Syllogism with the previously obtained $p$ to gain the arbitrary $r$.[9] However, as has been pointed out, for instance, by Beall (2013), despite their inferential weakness PLs such as **LP** constrain and guide the reasoning process in a useful way by offering to the reasoner choices. For example, in **LP** we can derive $p \vee (q \wedge \sim q)$ from $p \vee q$ and $\sim q$, offering the choice between $p$ and the contradiction $q \wedge \sim q$. Moreover, paraconsistent logicians will not deny that there are rational reasons to avoid contradictions. This may narrow down the choice in favor of $p$ so that $q \wedge \sim q$ is avoided.[10]

Summing up, we are confronted with a difficult methodological problem: how to decide whether the reasoning in view of which $p$ was obtained from $p \vee q$ and $\sim q$ is based on disjunctive syllogism, or whether it is based on deriving $p \vee (q \wedge \sim q)$ from $p \vee q$ and $\sim q$ and an additional constraint that narrows down the choice between $p$ and $q \wedge \sim q$ to the non-contradictory disjunct $p$? In a sense both interpretations only offer rational reconstructions but it is in no way clear how to establish the bridge between these reconstructions and the question what logic in fact governed the reasoning process of the given agents (if this is at all a sensible question![11]).

So we face the question whether the reasoning in Phase 1 is most adequately captured by CL or by a PL. We may ask what 'adequacy' means in this context (both normatively and descriptively), or more precisely: what kind of criteria for adequacy can we at all use to evaluate possible candidate logics? Without discussing and explicating criteria of this kind, statements such as "the case studies have shown [that] classical logic was used" (p. 238) seem rather unwarranted. From a formal point of view, there is an abundance of logics that are compatible with the historical data. Hence, without more refined criteria than mere 'compatibility' to determine when a logic offers an adequate explication of an observed reasoning process, the question seems underdetermined. It seems these more foundational questions need to be addressed first, before we can give more satisfactory answers to the main question. Moreover, the problem seems to involve further questions that go beyond the CL-vs.-PLs debate. For instance, one may ask what kind of dynamics (e.g., non-monotonicity, defeasible assumptions, etc.) are involved and to what extent is formal logic apt to model these dynamics?

A systematic study of the kind outlined in the previous paragraph can, for instance, be found in Stenning & Van Lambalgen (2008). There the idea is defended that logic is not "topic neutral", but it is "very much domaindependent in the sense that the valid schemata depend on the domain in which one reasons, *with what purpose*" (ibid., p. 20, italics in the original). The specific context an agent finds herself in requires the setting of various appropriate parameters such as the choice of a formal language, the choice of a semantics and the choice of a definition of valid arguments: she needs to reason *to* an interpretation. This gives rise to the pluralistic view that different logical forms are employed in different contexts and by different agents which enables them to reason *from* an interpretation according to formal laws. Note that CL is only one among many possible choices. This explains why different agents often draw very different conclusions when given one and the same task in the logic lab. The authors give specific importance to nonmonotonic logic in form of closed-world reasoning.[12] If we take with Stenning and Van

[9] Often paraconsistent logicians argue that in view of such examples and the fact that sometimes it is rational to accept contradictions, Modus Ponens and similar rules are not truth preserving. A case in point where it is rational to accept a contradiction is the Liar paradox (even non-paraconsistent logicians argue for acceptance of the contradiction, see e.g., Harman (1986)).

[10] More generally, it can be shown that if we pair **LP** with a principle that rejects any contradictory disjuncts from disjunctive consequences, we get the full derivative strength of CL for consistent premise sets (see (Priest, 2006, Chapter 8) and for an interesting multi-conclusion variant Beall (2011, 2013)). A similar point can be made for many other PLs. Adaptive logics integrate this in a dynamic proof theory with the effect that e.g. most inconsistency-adaptive logics are equivalent to CL for (classically) consistent premise sets.

[11] E.g., Harman (1986) takes the radical position that logic has no significant role in reasoning.

[12] For an in-depth analysis of empirical studies such as the Selection Task and the Suppression Task and other relevant literature the reader is referred to Stenning & Van Lambalgen (2008). For another example of a study

Lambalgen the domaindependence of logic as our starting point, the claim that CL is the logic of scientific reasoning implies the claim that the most adequate parameters to be set in the reasoning to an interpretation within scientific domains are the parameters that characterize CL (e.g., a first order language, truth-functionality for the implication, an extensional and bivalent semantics, monotonicity, etc.).

The reader may still have a worry. When we showed in the previous paragraph that apparently classical arguments can also be viewed as arguments based on 'weak' paraconsistent logics such as **LP**, we needed to supplement these logics with —what some may consider extra-logical— constraints according to which contradictions are to be avoided. Some comments are in place. First, one may take contradiction-avoidance as a logical principle. For instance, it may be thought of in terms of a closed-world assumption according to which contradictions are in the scope of negations as failure. This means that contradictions are considered false unless we can derive them. So-called inconsistency-adaptive logics can be interpreted as giving a formal explication of a similar idea. Nevertheless, extra-logical considerations (such as preferences, values, etc.) may still override this principle (e.g., we may want to work with a contradiction at least within a young theory) or provide us with a choice among the disjuncts within a derived disjunction of contradictions. Second and irrespective whether we consider contradiction-avoidance as extra-logical, note that Vickers does not oppose the idea that extra-logical considerations play an important role in the reasoning process. In fact, he also calls upon them. In Vickers' analysis the extra-logical considerations pro belief revision are indeed strong enough to override the normative directive of CL which advises the reasoner to accept a derived contradiction such as $p \wedge {\sim}p$ in the example above.[13]

**A non-falsifiable hypothesis?** This brings us to another critical point. What would be a possible falsification of Vickers' stance that Phase 1 is governed by CL? Given that we can formally model Phase 1 also via PLs (as argued above) the only distinctive property of CL compared to PLs is that it leads to logical explosion in view of inconsistent premise sets. However, according to Vickers, whenever scientists face a contradiction, extra-logical considerations override the inference rule ECQ resulting in a belief revision process (Phase 3). But this means that right from the start, every possible instance of a falsification of Vickers' thesis is deprived of its falsificatory potential.[14] As an empirical descriptive thesis it is thus questionable on methodological

that investigates the bridge between logical implication and actual reasoning take Benferhat et al. (2005). The authors investigated the descriptive adequacy of specific inference rules in the context of default logic. Subjects were confronted with premises and questions such as "Given this information, do you expect ...", to which they were to reply with "Yes", "No", or "There is no way to tell". The authors then evaluated how much the recorded data conforms to or violates the respective reasoning patterns under investigation. In line with the method employed in such tests one may be inclined to take the fact that (with Vickers) scientists refuse to apply ECQ, to be indirect evidence for them to take ECQ to be invalid and, in turn, to be indirect evidence for them to reason according to a paraconsistent logic.

[13] A difference is that in Vickers' analysis these extra-logical considerations play only a role when revising beliefs, while in our illustration above they also play an 'accumulative role' when making choices among the disjuncts the logic offers. We have two more remarks. First, also when using **LP** the extra-logical consideration to avoid contradictions can become decisive for revising beliefs. In the example above we can derive $(p \wedge {\sim}p) \vee (q \wedge {\sim}q)$ from $\Gamma$ via **LP**. Thus, we have the choice between accepting $p \wedge {\sim}p$, accepting $q \wedge {\sim}q$, or rejecting our premise set. The latter choice is given by an extra-logical consideration which advises the rejection of contradictions in favor of revising beliefs. Second, it is likely that also in a reasoning process governed by CL extra-logical considerations are used by rational reasoners to make choices. Suppose we can derive (via CL) $p \vee q$ from some premise set, but neither $p$ nor $q$ is derivable. For instance, if $p$ is significantly better supported by our evidence (or we have other reasons to reject $q$ such as ethical or political reasons) we may base our further reasoning process on $p$ rather than $q$.

[14] One may want to defend Vickers from this criticism by conjecturing that –in principle– he leaves open the possibility of a (rational) scientist applying ECQ. However, given his insistence of the non-existent danger of ECQ this seems about as likely as the spontaneous materialization of a dog on my desk due to quantum effects. For

grounds. Also, one may ask what normative force *–if any!–* ECQ has if any possible instance of it is overridden by another (extra-logical) normative principle. Given that its normative force is (factually) nullified in this way, Occam's Razor advises us to get rid of it which is a direct path towards PLs.[15]

**Sticking to your (inconsistent) guns?** It is true that the natural reaction to the discovery of an inconsistency in our belief base is along the lines of "there must be something wrong". Indeed, this is not even questioned by proponents of PLs, many (maybe even most) of whom are not committed to the metaphysical stance that there are true contradictions in the natural sciences. However, it is often argued that not every such discovery need to lead to a withdrawal of our commitment in the propositions in question: sometimes it may at best cause us to withdraw our commitment in the full aggregation of said propositions but nevertheless to keep our commitments to the individual propositions in place. In the epistemological literature such cases are frequently discussed in the context of scenarios similar to the so-called lottery paradox (Kyburg (1961)). Indeed, the situation of a scientist may be compared to the situation discussed as the preface paradox (Makinson (1965)). There, we have a scholar who wrote a rather complex book which she thoroughly checked for mistakes. Hence, for each argument in the book (think, for instance, of complicated mathematical proofs) she has good reasons to suppose that they are correct. On the other hand, experience teaches her that for a book of that complexity it is reasonable to assume that there are some mistakes left. Not knowing where the mistakes are, our scholar still has good reasons to believe in the arguments in her book (and to base her further reasoning on them, though in a careful scholarly manner) although this is inconsistent with her belief that there is a mistake.

In the same manner, a scientist may have good reasons to consider each of the propositions she is reasoning with as reasonably well-confirmed or plausible to have a justified commitment to take them as the basis of her reasoning process, although she knows that something must be wrong. (Actually, any not extremely optimistic scientist who is aware of the dynamic history of the sciences will suppose that at least some apparently well-confirmed bits in the scientific corpus will be eventually replaced.) Similarly, but with a more methodological (as opposed to epistemological) flavor, some paraconsistent logicians argue that at least when it is not yet clear which assumption to reject in a problematic theory and when reasoning towards its revision, it is useful to retain the given inconsistencies in order to find an apt consistent replacement of the theory (e.g., Batens (2002)).

Vickers' response is striking, simple, and will hopefully inspire some fruitful discussions: the situation in which "(i) one cannot sensibly use classical logic, and (ii) one doesn't know which assumption to eject [...] does not turn up in the real history of science, and is instead a philosophers reconstruction of a hypothetical science that does not exist" (p. 241). The closest scientists come to reason with inconsistencies is in the *as if*-way which, in Vickers opinion (but see below), is not governed by a PL. They draw inferences in a careful and cautious way, being

instance, Vickers argues that inferring anything via ECQ is "quite incredible" concluding that "any claim that one should be wary of one's inferences because of ECQ is just a mistake" (p. 54).

[15]Take e.g., the characterizations of what it means that an agent employs a logic in (Field, 2009, p. 263):

I The way to characterize what it is for a person *to employ* a logic is in terms of *norms the person follows*, norms that govern the person's degrees of belief by directing that those degrees of belief accord with the rules licensed by that logic.

More concretely in terms of belief functions this means:

E Employing a logic $L$ involves it being one's practice that when simple inferences $A1, \ldots, An \vdash B$ licensed by the logic are brought to one's attention, one will normally impose the constraint that $P(B)$ is to be at least $P(A1) + \ldots + P(An) - (n-1)$.

Clearly, in Vickers' analysis ECQ is in no way a norm that 'governs' a person's beliefs as in $I$ or $E$.

aware that their foundation is unsafe, with the goal of arriving at a (at least temporarily safe) foundation again. (Neuraths ship comes to mind.) Let us take a closer look at this.

**Cautious reasoning and logic** Note that the fact that "one cannot have full confidence in any inference one makes in such circumstances, so one is forced to judge one's inferences one at a time, empirically and/or conceptually" (p. 63) seems to be rather an argument for a defeasible and/or non-monotonic logic than an argument against PLs. These logics offer a formal explication of dynamic aspects of defeasible reasoning. Sometimes we retract previous inferences either due to insights gained by further analyzing our premises or due to insights gained by new external inputs (such as experiments, etc.).[16] Note that scientific reasoning is rich with defeasible inferences: for instance, when interpreting an experiment scientists usually make (often implicitly) assumptions (e.g. that the measuring devices and that the used computer algorithms work correctly and are fine-grained enough to detect analysanda[17], that specific exceptional circumstances do not occur, etc.) some of which may turn out to be ill-founded later on. As a consequence, inferences based on such assumptions may have to be retracted. In view of this, defeasible logics give a formally precise account of a 'careful' reasoning process in which we cannot have 'full confidence' in our inferences but have to treat them in a defeasible manner.[18]

**Idealizations and making inferences** Furthermore, what seems of prior importance in cases in which scientists "reasoned with the propositions *as if* they committed to them as candidates for the truth" (p. 240) is that these scientists had the commitment to take said propositions as premises for drawing inferences. What does follow from the fact that they interpreted them in an *as if*-mode (as idealizations, approximations, etc.)? First and foremost, this implies that whatever is derived from them is taken to be of approximative and thus defeasible character as well. Again, the most we get from this seems to be an argument for a (defeasible or non-monotonic) logic for reasoning on the basis of uncertainty. Moreover, we would be ill-advised were we willing to accept just any consequence (even if only interpreted as an approximation) in face of conflicting approximations. Hence, in view of this PLs seem to be unavoidable.[19] Note that since Vickers seems to concede that scientists sometimes reason 'carefully' on the basis of inconsistent instrumental commitments, his reply that scientists withdraw their doxastic commitments in view of a contradiction is not available in this context (since there are no relevant doxastic commitments to start with). To save the day Vickers would need to make recourse to

[16]While so-called non-monotonic logics usually only deal with the latter, 'external' dynamics (retraction in view of new external inputs), logics such as adaptive logics (Batens (2007); Straßer (2014)) and Pollock's OSCAR-system (Pollock (1995)) deal also with the former, 'internal' dynamics.

[17]An interesting case in this respect concerns the history of the research on peptic ulcer disease (PUD). From the second half of the 19th century to 1954 there were two competing hypothesis concerning the cause of PUD: the bacterial and the acidity hypothesis. In 1954 E. Palmer published a study in view of which, according to a number of authors, the bacterial hypothesis was abandoned. He took gastric mucosal biopsies from 1,180 patients and did not find any evidence in support of the bacterial hypothesis. Relying on his identification method –hematoxylin and eosin staining– he concluded that he refuted the bacterial hypothesis. However, as it turns out, his staining method was not reliable for the identification of the bacterial agent, namely Helicobacter pylori, which was in the 1980s established by Marshall & Warren as the major cause of PUD (see Šešelja & Straßer (2014)).

[18]Meheus (2003) distinguishes between reasoning from inconsistent information and reasoning ampliatively from incomplete information (e.g., by means of abduction, induction, default reasoning, etc.). She argues that also in the latter case scientists sometimes arrive at inconsistent hypotheses and continue to reason with them in a heuristically fruitful way. A paraconsistent logic for abductive reasoning can, for instance, be found in Provijn (2012).

[19]The argument also holds if one objects to the notion of non-monotonic *logics* by distinguishing sharply between implication/deduction/argument and belief revision where only the former is governed by logics. According to this view, logics constrain a reasoner by providing rational choices, while e.g. evidence weighing determines that choice (see e.g. Beall (2013); Knorpp (1997)).

the rather extreme stance according to which the only propositional attitudes that may serve as inputs in a logical reasoning process are strict beliefs (as opposed to *as if* ones) in the truth of statements.

Sometimes Vickers remarks that the inconsistencies in instrumental commitments qua being idealizations, approximations, etc., have no ontic bite, they "never get off the ground" (p. 232) since they can "be put down as artifacts of the idealizations in play" (p. 240). For example, although in Kirchhoff's case "the de-idealization cannot be carried out due to computational intractability, [...] it is reasonable to assume that *if* the de-idealization were possible, the inconsistencies would disappear" (p. 231). Again, this seems not to imply an argument against PLs. What we are interested in is the actual reasoning process a scientist is engaged in. In view of this we seem to have a classical case of bounded rationality where a reasoner is forced by practical limitations to reason by means of inconsistent idealizations. To have a possible though inaccessible world in mind where the inconsistencies –qua de-idealization– disappear seems not of much help in the process of drawing inferences on the basis of said propositions.

Note that this point concerns the issue of what is to count as a 'significant' inconsistency that we discussed in Section 3. In view of the above reasons, it is not clear why Vickers' narrowing down of significant inconsistencies to those in doxastic commitments of scientists (leaving out those in their instrumental commitments) is warranted.

**Blurring the demarcation line** Another question that arises is whether Vickers' dictum 'reason classically unless you arrive at a contradiction, then revise' is really that different from some proposals by paraconsistent logicians. Vickers is right to notice that many PLs weaken or abandon certain inference rules (such as Disjunctive Syllogism). However, some PLs –typically non-monotonic ones– do not abandon rules but rather manage their application. Case in point are so-called inconsistency-adaptive logics: for example, in the logic **CLuNr** the inference rule Disjunctive Syllogism besides all other rules of CL can be used in proofs. However, in cases in which it is applied to inconsistent parts of the premise set it is discharged (see Batens (1999)). This way, the logic identifies and validates inferences that do not rely on inconsistent parts of the premise set while it at the same time isolates exactly those inferences based on inconsistencies for which Vickers concedes that no scientist would be willing to make them. In a sense this logic models a reasoning process that uses the inference rules of classical logic in a cautious way akin to the approach advocated by Vickers, though the logic is inconsistency-tolerant.

Another example are the consequence relations phrased in terms of deductive argumentation frameworks (see Besnard & Hunter (2009); Arieli & Straßer (2014)). Underlying is the idea that the (defeasible) reasoning of an agent (e.g. a scientist) is to be modeled as a unilateral dialogue this agent has with herself.[20] Full classical logic is available to build arguments on the basis of a premise set $\Sigma$. The latter are pairs $(\Gamma, A)$ (given $A$ follows classically from $\Gamma$ and $\Gamma \subseteq \Sigma$). In case $\Sigma$ is inconsistent this will give rise to conflicting arguments that attack each other in terms of various forms of attack such as rebuttals or undercuts. Finally, arguments are selected that are defended from their attackers: $A$ follows from $\Sigma$ if there is a selected argument $(\Gamma, A)$.

In fact, the latter observations seem to blur the lines altogether: in both mentioned approaches the consequence relation is clearly paraconsistent in the usual understanding of invalidating ECQ, though the standard of deduction is classical logic.[21] This brings us back to the question: what does it really mean that the reasoning of an agent is governed by CL, or any logic for that matter?

[20] It is interesting to notice that one of the first paraconsistent formal logics, Jaskowski's **D2** from 1948 (for the English translation see Jaśkowski (1969)), has a 'discussive' interpretation.

[21] The same holds for the consequence relations proposed in Rescher & Manor (1970). Another approach that falls in the gray zone between CL and non-classical approaches is the idea to 'filter' classical logic which has been developed e.g. by Schurz and Weingartner (see e.g., Weingartner (1994)).

In conclusion, it can be said that Vickers' argument against PLs and in favor of CL, which is substantiated by thorough and highly welcome historical investigations into various case studies, is nevertheless in need of further elaborations in view of the above raised questions. More foundational conceptual clarification is still needed to make proper sense of questions such as the one in the previous paragraph and to understand what shall be counted as positive or negative historical evidence in answering such questions. Vickers' investigations may help to prevent or even heal some scholars of 'paraconsistencitis'. However, they do not (yet?) provide sufficient support for a convincing argument against the adequacy of PLs in the explication of scientific reasoning where this explication is not just a post-hoc rationalization.

# 5 Conclusion

In this article we have offered some critical remarks on Peter Vickers' "Understanding Inconsistent Science". Nevertheless, this should not lead to the conclusion that the significance of Vickers' book is overshadowed by these issues. On the contrary, Vickers has opened a debate on central problems regarding inconsistencies in scientific reasoning and their explication. As such, his book brings a whole variety of challenges for both historians and philosophers of science. In addition, Vickers' method of theory eliminativism is an excellent methodological tool for an investigation of inconsistencies, since it requires from philosophers to situate them in a concrete historical and doxastic, resp. instrumental context. Given the historical rigor with which his case studies are examined, as well as the methodological novelty of his approach, Vickers' work is certain to trigger a number of debates in the philosophical community, starting from those on historical details of the presented episodes, to discussions on formal logic and its application to scientific reasoning. For these reasons alone it is safe to say that Vickers' book is from now on a necessary reference for any scholar of these problem fields. Moreover, given the complexity of the topic and its relevance for other philosophical disciplines, the book is a highly welcome contribution not only to the literature on inconsistencies in science, but to logic, epistemology and methodology of science in general.

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