

# Scientific Pluralism, Consistency Preservation, and Inconsistency Toleration

Otávio Bueno<sup>†</sup>  
otaviobueno@mac.com

## ABSTRACT

Scientific (disunity) pluralism is the view according to which there is a plurality of scientific domains and of scientific theories, and these theories are empirically adequate relative to their own respective domains. Scientific monism is the view according to which there is a single domain to which all scientific theories apply. How are these views impacted by the presence of inconsistent scientific theories (in particular, theories that are inconsistent with one another)?

There are (A) *consistency-preservation* strategies and (B) *inconsistency-toleration* strategies. Among the former, two prominent strategies can be articulated: (A1) *Compartmentalization* and (A2) *Information restriction*. Among the inconsistency-toleration strategies, we have: (B1) *Paraconsistent compartmentalization* and (B2) *Dialetheism*. In this paper, I critically assess the adequacy of each of these four views.

## 1. Introduction

Scientific (disunity) pluralism is the view according to which there is a plurality of scientific domains of inquiry, a plurality of scientific theories, and these theories are empirically adequate relative to their own respective domains. Scientific monism is the view according to which there is a single domain of inquiry to which all scientific theories apply.

It seems to me that any monist view faces considerable difficulties to make sense of contemporary science, where there is extremely little in common among different domains. Just consider how diverse the following theories and domains of scientific inquiry are: quantum field theory and pediatrics, molecular biology and cultural anthropology, biochemistry and sociological social psychology. They do not share goals, methods, theories, or approaches.

<sup>†</sup> Department of Philosophy, University of Miami, USA.

In fact, the only way of lumping them together in some coherent form is to consider them at such a high level of abstraction that very little of their actual content is preserved in the end. For instance, to insist that all (or even most) of the investigation of these domains involves the formulation of empirically testable theories, and that such a test has the overall structure of a *modus tollens* (as Popper (1934) insists) is precisely to navigate at an extraordinarily high level of abstraction, and to lose the most significant, specific details of the research being conducted in each particular domain. It is very hard to see how a monist picture can accommodate this salient feature of scientific practice, while doing justice to the specificity and content of each domain of inquiry.

Given this plurality, it may happen that inconsistent theories end up being formulated within a particular domain of inquiry. The result is the occasional pursuit, articulation, and advancement of theories that are inconsistent with one another. A prominent case is, of course, the tension between quantum mechanics and relativity theory. This immediately raises the issue of how these views, scientific pluralism and scientific monism, are impacted by the presence of inconsistent scientific theories? In this paper, I provide a framework to consider this question.

The framework has the following components: First, there are (a) *consistency-preserving* strategies and (b) *inconsistency-tolerant* strategies. Among the former, two prominent maneuvers can be articulated: (a<sub>1</sub>) According to *compartmentalization*, domains of inquiry to which mutually inconsistent scientific theories apply are compartmentalized, with no overlap between them, and in this way classical logic is preserved. (This generates a consistent disunity scientific pluralism.) (a<sub>2</sub>) According to *information restriction*, a single domain is maintained throughout the sciences, and any resulting inconsistency is eliminated by (somehow) extracting conflicting information (in more or less principled ways). As a result, once again, classical logic is preserved. (This yields a consistent scientific monist view.)

Among the *inconsistency-tolerant* strategies, there are also two possibilities: (b<sub>1</sub>) According to *paraconsistent compartmentalization*, a plurality of overlapping domains is allowed for throughout the sciences. Since inconsistencies may emerge, a paraconsistent logic is then needed to avoid triviality, in light of classical logic's explosive nature; that is, on this logic, everything follows from a contradiction. (This generates inconsistent scientific disunity pluralism.) (b<sub>2</sub>) According to *dialetheism*, all inconsistencies for which there is good evidence are taken to be part of a single scientific domain (Priest

2006*a* and 2006*b*). Once again, in light of the presence of inconsistencies, a paraconsistent logic is required. (This generates inconsistent scientific monism.) In what follows, I critically assess the adequacy of each of these four approaches.

## 2. Inconsistent Scientific Theories

When considering the issue regarding inconsistent scientific theories, four issues need to be addressed: (1) Are there, and can there be, such things as inconsistent scientific theories? (2) If so, how should they be accommodated? In particular, we should be able to understand the *styles of reasoning* involved in dealing with inconsistencies. (3) What are the sources of the inconsistencies? Do they emerge from empirical reasons, theoretical reasons, or by mistake? (4) Given the crucial role of mathematics in applications, how should we deal with inconsistent *applied* mathematical theories? What is the status of these theories? Which commitments (if any) do they bring?

It may be argued that there cannot to be inconsistent (scientific or mathematical) theories. Or, at least, if inconsistent scientific theories existed, they could not be accommodated by classical philosophical conceptions of science. On the *syntactic* approach to scientific theories, a theory is a set of sentences closed under logical consequence (for a thorough discussion and references, see Suppe (1977). If the logic is classical, an inconsistent theory would be identical with the set of sentences of the language. Triviality then emerges.

On the *semantic* approach to scientific theories, a theory is presented as a family of models (see, for instance, van Fraassen 1980 and 1989). If the logic is classical, an inconsistent theory would have no models. So, there would not be models of the theory in terms of which the theory could be presented. Again, there is no room for inconsistency here.

In response, it can be argued that if inconsistent scientific theories cannot be accommodated by classical philosophical conceptions of science, this just shows that these conceptions are inadequate. We should change these conceptions so that they are able to accommodate the data. But *which data*? It has been assumed that there are inconsistent scientific theories. But are there in fact any? (See Vickers (2013) for a critical response.) We typically do not have scientific theories that are internally inconsistent. At best, we may have cases of theories that are inconsistent with other theories. But, at this point, the issue

becomes one of *theory choice*: which of the two theories is better supported by the evidence? Let us consider some well-known cases that have been suggested as providing support for inconsistent scientific theories.

### 3. Bohr's atomic model

In 1913, Niels Bohr advanced his celebrated atomic model: the planetary model of the atom. According to this model, an atom has a nucleus around which electrons orbit, similarly to the configuration of a planetary system. From the start, the concern emerged as to whether such a model would be stable or whether the electrons orbiting in the atom would eventually lose their energy and crash into the nucleus.

According to electrodynamics, the model is fundamentally unstable: the electrons would eventually collapse into the nucleus in a matter of seconds. Bohr was, of course, perfectly aware of this issue. In order to circumvent the difficulty, he introduced a postulate to the effect that *the collapse would not happen*:

Energy radiation (within the atom) is not emitted (or absorbed) in the continuous way assumed in the ordinary electrodynamics, but only during the passing of the systems between different “stationary” states (Bohr (1913), p. 874).

Of course, the matter here is not just to insist, in more or less *ad hoc* terms, that the system would remain stable. An effort was made to suggest a reason why the stability of the atom was not an issue. By invoking the notion of a “stationary” state, in which the continuous emission and absorption of energy radiation in the atom does not occur in the ordinary way described by electrodynamics, Bohr was able to insist that energy radiation takes place just between different “stationary” states.

In the end, electrodynamics *does* apply, but only *partially*, that is, when the systems pass through various “stationary” states. At the “stationary” states themselves, however, energy radiation within the atom is not emitted (or absorbed) in the usual electrodynamic way. In fact, electrodynamics does not apply to these states.

In light of these remarks, it could be argued that Bohr's atomic model, strictly speaking, does not constitute a case of an internally inconsistent model, but at best it provides an instance of an inconsistency between two theories (broadly construed): Bohr's model and electrodynamics. The issue then

becomes which of these theories should be preferred?

One may wonder whether the matter is really one of *theory choice*. Each of the two theories (or, more precisely, both electrodynamic theory and Bohr's model) had its own proper domain of application. Electrodynamics was unable to explain the wavelength of the hydrogen's line emission spectrum, which, in turn, was beautifully accounted for by Bohr's model. Moreover, Bohr does invoke electrodynamics to describe the shifts among different "stationary" states. Bohr's innovation was to *limit the domain of applicability* of electrodynamics, emphasizing that the theory does *not* describe the behavior of "stationary" states. Bohr's own model would account for that. In the end, we have here some division of explanatory labor.

Furthermore, Bohr's model is not internally inconsistent: it does not invoke any principle for which both its statement and its negation hold. In a sense, the model is not even inconsistent with electrodynamics; it just restricts the domain of application of that theory. If electrodynamics applied to the description of "stationary" states, Bohr's model would be inconsistent with it. After all, the two offer incompatible accounts of the stability of the atom. However, if electrodynamics *does not* apply to the domain where the two would diverge, and it does *not* according to Bohr's model, then the model will *not* be inconsistent with electrodynamic theory. After all, no inconsistency could be derived at this point.

In other words, it seems that the inconsistency between electrodynamics and Bohr's atomic model is only potential: had the application of the former not been restricted (thus leading to an unstable atomic structure), the theory and the model would be inconsistent with one another. As it turns out, however, in light of the way in which Bohr had set up his atomic model, the inconsistency apparently can be blocked (see also Vickers (2013), Chapter 3, for additional discussion).

#### 4. Dirac's Delta Function

Bohr's maneuver of specifying a suitable domain of application for his atomic model *vis-à-vis* electrodynamics is analogous to a move that can be made to block the inconsistency of another alleged inconsistent object: Paul Dirac's delta function. This is an "improper" function that Dirac (1958) employed to good effect in his work in the foundations of quantum mechanics in order to allow for the proper formulation of the position and momentum operators. It is a real-

valued function  $\delta(x)$  such that:

$$\int \delta(x) dx = 1$$

$$\delta(x) = 0, \text{ for } x \neq 0.$$

One can think of it as a function that has value 0 throughout the real numbers, with the exception of an arbitrarily small domain around the origin; in that domain, the function's value is so large that the integral over the domain is 1. In Dirac's own words:

To get a picture of  $\delta(x)$ , take a function of the real variable  $x$  which vanishes everywhere except inside a small domain, of length  $\varepsilon$  say, surrounding the origin  $x=0$ , and which is so large inside this domain that its integral over this domain is unity (Dirac (1958), p. 58).

The concern is that the delta function seems to violate a well-known result in analysis to the effect that any real-valued function whose value is 0 throughout its domain except for a single point has total integral also 0. Dirac was well aware that some care was needed when dealing with the delta function to prevent inconsistencies from emerging. As he insisted:

$\delta(x)$  is not a quantity which can be generally used in mathematical analysis like an ordinary function, but its use must be confined to certain simple types of expression for which it is obvious that no inconsistency can arise (Dirac (1958), p. 58).

The “types of expression” Dirac is considering here are integrals. As long as the function is used within an integral, the potential difficulties will not emerge. This results from a crucial property of the delta function:

$$\int f(x) \delta(x) dx = f(0),$$

where  $f(x)$  is any continuous function of  $x$ . In light of this property, it is not difficult to understand why the delta function has the features it has. Dirac is very clear about this point:

We can easily see the validity of this equation from the above picture of  $\delta(x)$ .

The left-hand side of (the equation above) can depend only on the values of  $f(x)$  very close to the origin, so that we may replace  $f(x)$  by its value at the origin,  $f(0)$ , without essential error (Dirac (1958), p. 59).

The result is that a suitable elimination condition for the delta function is then devised. In this way, the function can be dispensed with as long as it is used under the proper conditions. Similarly to what goes on with Bohr's atomic model, in which domain specification played a key role, the decisive feature to ensure the reliability of the delta function is the specification of the proper domain in which it can be used: inside integrals (for additional discussion and references, see Bueno (2005)).

### 5. The Early Differential and Integral Calculus: Infinitesimals

The early formulation of the differential and integral calculus in terms of infinitesimals offers an additional case of a potentially inconsistent theory. Infinitesimals are positive numbers that are smaller than any number. They were very useful in the original articulation of the calculus. But due to their potentially inconsistent behavior, it is not surprising that their introduction has been controversial from the start. Even Leibniz, who introduced them, considered infinitesimals to be just "useful fictions" (Leibniz 1716): entities introduced for heuristic purposes and eventually eliminated (see Bueno (2007) for additional discussion).

Infinitesimals can be put to some quite useful work in derivations (see Colyvan 2008). Consider, for instance, the differentiation of a polynomial of the form:  $f(x) = ax^2 + bx + c$ . We have, by definition, of differentiation (with ' $\delta$ ' taken as an infinitesimal):

$$\text{(Diff.) } f'(x) = \frac{f(x+\delta) - f(x)}{\delta}$$

By applying (Diff.) to the polynomial  $f$  above, we obtain:

$$f'(x) = \frac{a(x + \delta)^2 + b(x + \delta) + c - (ax^2 + bx + c)}{\delta}$$

And *via* simple algebraic transformations, we then derive:

$$f'(x) = \frac{2ax\delta + \delta^2 + b\delta}{\delta}$$

Now, since the infinitesimal  $\delta$  is strictly positive, we can divide by it. Thus, we obtain:

$$f'(x) = 2ax + b + \delta$$

However, given that the value of  $\delta$  is arbitrarily small, it can be taken to be zero. As a result, we get:

$$f'(x) = 2ax + b$$

And this is, precisely, the correct result!

So, in the same derivation, the infinitesimal was taken to be both different from zero and not different from zero, which is not consistent. Given how delicate such a kind of reasoning is, it is not surprising that Bishop Berkeley was so deeply puzzled by it in the *Analyst* (Berkeley 1734). Berkeley insisted that the case was hopeless, and in the *Analyst* bitterly complained against the introduction of infinitesimals precisely on these grounds. (For a contemporary response by paraconsistent logicians, see, for instance, Carnielli and Coniglio (2013).)

Clearly, some care is needed here. If an infinitesimal  $\delta$  can be taken as zero in some contexts, we could have that:

$$1 \times \delta = 2 \times \delta$$

And if we assume that  $\delta$  is non-zero, we could then divide by  $\delta$ , and “establish” that:

$$1 = 2 (!).$$

Thus, there is no doubt that some rules are needed to manipulate infinitesimals in order to avoid triviality.

When Leibniz introduced, in 1684, the differential calculus in his *Nova Methodus*, he formulated the concept of differential without mentioning infinitesimals (for detailed discussions, see Bell (2005) and (2014), which I

follow here). In this way, he could try to avoid, at least in principle, foundational worries associated with these objects. His strategy was to devise explicit rules of differentiation, and implement what can be considered an “algebraic” approach to the subject. Interestingly enough, he never offered a proof of the rules, although in order to obtain these rules and eventually establish their validity, it is very likely that infinitesimals would have been invoked. The rules are:

$$\begin{aligned}
 da &= && 0, \text{ where } a \text{ is a constant;} \\
 d(ax) &= && a \, dx, \text{ where } a \text{ is a constant;} \\
 d(x + y - z) &= && dx + dy - dz \\
 d(xy) &= && x \, dy + y \, dx \\
 d(x/y) &= && \frac{x \, dy - y \, dx}{y^2} \\
 d(x^p) &= && px^{p-1}dx, \text{ also for fractional } p.
 \end{aligned}$$

These are, of course, precisely the familiar rules for the derivative, and it is striking that Leibniz managed to obtain them operating in a context that may have included inconsistencies (to the extent that infinitesimals were involved).

On the surface, the rules may seem to be entirely infinitesimal-free. But it turns out that infinitesimals were still presupposed, given the particular interpretation of the formalism offered by Leibniz in terms of suitable curves. In fact, if a curve is determined by the variables  $x$  and  $y$ , then Leibniz took  $dx$  and  $dy$  to be infinitesimal differences, or differentials, between the values  $x$  and  $y$ . In this case,  $dx/dy$  was taken as the ratio of the two, and for Leibniz this is the slope of the curve at the corresponding point. Moreover, Leibniz’s definition of tangent uses infinitely small distances, which, once again, also presupposes infinitesimals (see Bell 2005 and 2014). However, in the end, by thinking about potentially inconsistent objects, Leibniz was eventually able not only to devise the correct rules for the differential calculus, but also to formulate them in a way that would, in principle, allow him to (attempt to) dispense with infinitesimals altogether.

He was not alone in invoking these objects and eventually trying to dispense with them. In Section I of Book I of the *Principia*, Newton considers the status of quantities that differ by an infinitesimal; interestingly, he notes, they are

ultimately the same. (How can quantities that differ by anything, even an infinitesimal, be the same is, obviously, a delicate matter.) As he points out:

Quantities, and the ratios of quantities, which in any finite time converge continually to equality, and before the end of that time approach nearer to each other than by any given difference, become ultimately equal (Newton (1687/1729), p. 42, Book I, Section I, Lemma I).

Of course, there is a dynamical feature in this description. The quantities are converging and their rate of change is being considered: it is only in the long run that they become “ultimately equal”. But do they ever become the same? This is less clear. For the quantities to become equal, they need to be reified and considered not as varying, converging things. They need to be static objects. But static objects do not converge. However one considers the matter, the status of the quantities in question is rather delicate, to say the least.

Later on, aware of the complexities and difficulties involved with the introduction of infinitesimals, Newton indicates the importance of dispensing with such objects:

For demonstrations are shorter by the method of indivisibles; but because the hypothesis of indivisibles seems somewhat harsh, and therefore that method is reckoned less geometrical, I chose rather to reduce the demonstrations of the following propositions to the first and last sums and ratios of nascent and evanescent quantities, that is to the limits of those sums and ratios, and so to premise, as short as I could, the demonstration of those limits (Newton (1687/1729), p. 54, Book I, Section I, Scholium).

Thus, similarly to Leibniz, Newton was also careful to devise a strategy to avoid troublesome infinitesimals.

Among the three cases considered so far, namely, Bohr’s atomic model, Dirac’s delta function, and Leibniz’s and Newton’s early formulation of the calculus, due to the use of infinitesimals, it seems that it is the early calculus that provides the strongest case for the presence of inconsistency in science (or, more precisely, for the presence of inconsistency in a mathematical theory used in science).

## 6. Accommodating Inconsistent Theories

It is controversial that the cases discussed above do involved inconsistencies. Peter Vickers, in particular, has challenged that any of the alleged cases of inconsistency in science are in fact so. On his view:

We have seen that the number of significant inconsistencies which exist in real science is far less than has often been suggested. The main examples of significant inconsistencies in this thesis are the later Bohr theory of the atom, Newtonian cosmology, and (to a lesser extent) the early calculus and Kirchhoff's theory of diffraction. But in each of these cases it is clear that scientists don't avoid the conflict by changing their logic. As the case studies have shown, classical deductive logic is 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 (Vickers (2013), p. 238).

So, even if it turned out that there were cases of inconsistency in the sciences, the strategy to accommodate them, according to Vickers, would not involve changing the logic.

Why would a change of logic be considered a plausible response to the presence of inconsistency in science? The concern, of course, is that if the underlying logic were classical, in the presence of an inconsistency, the result would be triviality: everything would follow. One of the purposes of conducting inquiry is precisely to determine for which items there is good evidence and for which items this is not the case. However, if everything follows from whatever assumptions one may start from (due to an inconsistency), it is not possible to make such a determination. For any given item, one would both have evidence for it and would not have evidence for it. After all, it would follow from the assumptions in question that there is such evidence. It would also follow that there isn't. The result, of course, is triviality.

How can such triviality be resisted? As is well known, in a paraconsistent logic, not everything follows from a contradiction. Thus, classical logic's principle of explosion (according to which contradictions entail everything) does not hold (for details, see da Costa, Krause, and Bueno 2007). In this context, by changing the underlying logic, from classical to a paraconsistent, it is possible to block the triviality that emerges from the presence of inconsistency.

One could then explore the domain of the inconsistent, by reasoning about its various aspects in order to determine which pieces of information should be retained and which pieces eliminated so that consistency can be restored.

But any change of underlying logic from classical to paraconsistent does constitute a move away from scientific practice. To the extent that there is an underlying logic in scientific practice, that logic is typically classical. If the goal is to understand relevant features of that practice, it is unclear how a change in logic would contribute to that task. Such a change ultimately amounts to the production of a different scientific practice, governed by a different logic (again, to the extent that there is an underlying logic in the practice of science to begin with). In this respect, Vickers' complain above regarding the sort of understanding that would emerge from a change of logic does have a point. One needs to find something within scientific practice to properly accommodate the issue.

But if there are inconsistent theories in science, how can they be accommodated? Let me briefly suggest four possible strategies. (a) First, there are *consistency-preserving strategies*: these strategies reject the acceptability of inconsistent scientific theories. Since classical logic is assumed, there cannot be such theories, even in principle, for logical disaster would then ensue.

Two strategies are then implemented in the attempt to secure consistency:

(a<sub>1</sub>) The first is *compartmentalization*: classical logic is maintained, and the domains of inquiry, to which scientific theories that are inconsistent with one another apply, are compartmentalized so that there is no overlap among them. This generates a consistent disunity pluralist view, given that the various domains do not overlap and no attempt is made to unify them. Given the lack of overlap among the different domains, consistency emerges.

(a<sub>2</sub>) The second consistency-preserving strategy is *information restriction*: classical logic is also maintained, and a single domain is kept throughout. In order to eliminate inconsistencies, conflicting pieces of information are (somehow) jettisoned and extracted. This yields a consistent monist view, given that a single domain is posited and consistency is maintained by the elimination of contradictory bits of information.

(b) But, second, there are also *inconsistency-tolerant strategies*. These strategies recognize the possibility of inconsistent scientific theories. Since inconsistencies are tolerated, some paraconsistent logic needs to be invoked.

Two strategies are then implemented. (b<sub>1</sub>) The first is *paraconsistent compartmentalization*: there are inconsistent but non-trivial scientific theories,

given that a paraconsistent logic is assumed. Since a plurality of overlapping inconsistent domains is allowed for, this generates inconsistent disunity pluralism.

(b<sub>2</sub>) The second inconsistency-tolerant strategy is *dialetheism*, according to which some contradictions are true (Priest 2006*a* and 2006*b*). A paraconsistent logic is adopted, and all inconsistencies for which there is good evidence are taken to be part of a single scientific domain. This yields inconsistent monism.

As it turns out, however, all of these strategies to accommodate inconsistent theories face difficulties. I will consider them in turn.

(a) As opposed to the consistency-preserving strategies, some of the data from scientific practice seem to support the existence of inconsistent scientific theories. As noted above, infinitesimals do seem to have inconsistent properties. And consistent versions of theories invoking infinitesimals, for instance, in terms of nonstandard analysis (Robinson 1974), do not properly capture the original formulation of the theory (for instance, the simplicity of the original infinitesimals is lost). Moreover, as is well known, quantum mechanics and relativity theory are mutually inconsistent. The fact that two of the major theories of physics are not consistent with one another provides good reason to take seriously inconsistency in the sciences.

(a<sub>1</sub>) *Contra* compartmentalization, crucial information may be lost when the compartmentalization is implemented. Some contexts may require information in overlapping domains. For instance, what happens to quantum particles when they are subjected to huge gravitational fields? Furthermore, compartmentalization can be *ad hoc* if it is devised just to avoid inconsistencies without also providing some independent motivation. Crucial information may be lost in this way.

There is, however, one benefit: compartmentalization may lead to pluralism given that it allows for various domains, and that is a significant feature of contemporary sciences.

(a<sub>2</sub>) As opposed to *information restriction*, once again, crucial information may be lost when some bits of information are extracted to preserve consistency. The extraction can also be *ad hoc*. If it is implemented just to avoid inconsistency and with no independent motivation, its epistemic value is limited in light of the cost of information loss. Moreover, sometimes the only way to obtain new information is to explore theories inconsistent with accepted ones. Consider what Galileo did in light of the accepted Aristotelian theory of his time: he was advancing theories that were in direct conflict with what was accepted at the time

(see Feyerabend, 1993). Thus, strategies that involve information restriction can be highly ineffective. This strategy can also lead to a form of monism, with a single consistent domain, which in fact conflicts with the practice of contemporary sciences.

(b) But there are also significant costs associated with inconsistency-tolerant moves. (b<sub>1</sub>) *Paraconsistent compartmentalization* ultimately generates a form of anachronism. There was no paraconsistent logic available in the 17th century during the development of the calculus. At best, we have a framework to represent an *artificial reconstruction* of the reasoning invoked with the introduction of infinitesimals. The actual reasoning needs to be left behind. Moreover, the underlying logic is hardly ever made explicit in scientific practice, which results in a highly idealized strategy.

There is one benefit, however: this strategy allows for a plurality of overlapping domains, and this pluralism jibes well with contemporary sciences.

(b<sub>2</sub>) *Contra dialetheism*, it is unclear that scientific theories have ever been embraced on the grounds that they yield true contradictions. Nowhere in scientific practice is this strategy in fact implemented. Moreover, this would lead to a monist view, which also does not mesh well with the facts about scientific practice. Furthermore, dialetheism is ultimately not needed. A paraconsistent strategy would allow one to explore inconsistent information that may emerge from scientific theories without triviality, with no need to arbitrarily eliminate some bits of information, and without the commitment to true contradictions. This, in principle, provides a possible middle ground. However, the paraconsistent strategy still faces the difficulties raised above.

Perhaps by taking seriously some of the relevant details of scientific practice an alternative framework to make sense of inconsistent scientific theories could be suggested. The idea is to adopt two complementary strategies, both of which are extensively invoked in scientific practice and illustrated in the case studies above, namely, *dispensability* and *domain restriction*. In light of inconsistencies, one could restrict the relevant domain to avoid inconsistency, but maximizing the amount of consistent information available and ultimately dispensing with the inconsistent objects.

In the case of infinitesimals, as noted above, they are ultimately dispensable (both Leibniz and Newton suggested ways of dispensing with them). In the case of Bohr's atomic model, we found the use of domain restriction to avoid inconsistency. Finally, in the case of the delta function, both domain restriction and dispensability were employed. Perhaps by skillfully combining these moves

a more effective way of dealing with inconsistency in sciences could be implemented?

## 7. Conclusion

Of course, more work is required in the attempt to make sense of those cases in which inconsistencies emerge in the sciences. On the one hand, formal approaches are clear and tractable, but they add an extra layer of structure that is not part of scientific practice. On the other hand, informal approaches, despite being closer to the practice, can be just as confusing and unclear as certain traits of the practice itself. The challenge here is to find the proper balance, and an illuminating account of which features in the practice allow one to deal with inconsistencies in a proper way.

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