Bohr’s Theory of the Atom: Content, Closure and Consistency

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> *For the first time, we have been given a consistent theory to explain the arrangement and motion of the electrons in the outer atom.*
>
> Rutherford, 1923

ABSTRACT

How exactly does the much-discussed inconsistency in Bohr’s theory of the atom manifest itself? A close look at the suggestions made so far in the literature suggests that the theory may not be inconsistent at all. The answer depends on (i) what exactly we take the content of ‘Bohr’s theory’ to be, and (ii) what we take to follow from that content (how we ‘close’ the theory). In lieu of inconsistency, alternative characterisations of the relevant conceptual problems are possible. Looking briefly at the later Bohr theory, I conclude that the theory was only inconsistent after the introduction of the quantum adiabatic principle in 1917.

1. Introduction

Bohr's theory has been controversial from the beginning. At the height of its success in 1914 Einstein is reported as stating, 'The theory of Bohr must then be right.' The same month, and aware of the same successes of the theory, von Laue flatly asserted, 'This is nonsense!'[1] The first goal of this paper is to demonstrate that this disagreement has its counterpart today at the *philosophical* level. Ever since Jammer (1966) and Lakatos's seminal paper of 1970 which describes it as 'a research programme progressing on inconsistent foundations', Bohr's theory has been widely cited as the example *par excellence* of an internally inconsistent theory. But when it comes to identifying the specific scientific content which constitutes the inconsistency important disagreements arise, and this despite the fact that most papers are rather noncommittal regarding details. We can add to this a different type of disagreement: Bartelborth (1989a) and Hendry (1993) claim to demonstrate the *consistency* of the theory, and Hettema (1995) follows suit. In addition at least some of those working at the time thought the theory to be consistent, as the epigraph testifies. It is now thirty years since Feyerabend wrote,

> There hardly ever existed a view so incoherent and at the same time so fertile as this strange and still not too well understood research programme. (1978, p.158)

It would seem that the theory is little better understood in 2008.

Disagreements about consistency follow from miscommunication on two main fronts. On the one hand there is little or no connection between the logical definition of 'inconsistent' and the use made of it to describe Bohr's theory. Instead of providing an 'A&~A' as a theorem (say) an appeal is made to the reader's intuition. On the other hand each author appears to have his or her own take on the content of the theory. In the absence of a consensus on the criteria for theory membership a philosopher is somewhat free to shape

[1] Cited in Pais 1991 (p.154) and Jammer 1966 (p.86).

the content to fit his or her preferred conclusion. That this is crucial to consistency is clear—with one extra assumption, or an assumption stated in a slightly different form, we may well move from a consistent theory to an inconsistent one. Such a small difference in the presentation of the theory means the difference between a theory which couldn't be true in *any* possible world, and a theory which might be true of *this* world.

These will be the main issues as I proceed to provide an answer to the question 'Was Bohr's theory inconsistent?' I begin in §2 by introducing the theory and the consistency debate. I present three different foci of inconsistency which have been proposed, and provide reasons to doubt each claim. In §3 I turn to the 'closure' of the theory, by examining the connection between the logician's conception of inconsistency and its use to describe scientific theories. This leads me to an attempt to prove the consistency of the theory courtesy of Bartelborth (1989), which is then assessed in §4. Here the content of the theory is examined in more detail, and I argue that the doubts expressed in §2 are justified. With the consistency of the early theory thus-argued, §5 introduces various alternative ways in which the relevant conceptual problems of the theory might be characterised. §6 turns briefly to the later Bohr theory, and draws on the tools developed to argue that we do finally find inconsistency here. §7 is the conclusion.

2. Three foci of alleged inconsistency

It should be noted first of all just what a shock it was to find that the classical picture, as developed up to the end of the 19th century, was fundamentally flawed. At this time Lord Kelvin spoke for a large proportion of physicists when he claimed that all that remained for physics was a certain amount of 'mopping up' work, with all the big ideas already in place. Despite the success of his famous constant in 1900, Planck refused for ten years to accept that a fundamental shift in perspective was required. For many Einstein's 'photon' of 1905 was just another development in the *classical* wave versus particle understanding of light. Other developments were more difficult for the classical program, but nothing was to prepare the community for Bohr's famous postulates of 1913. He described the atom as follows:

(P1) Electrons orbit the nucleus analogously to planets orbiting the sun, swapping the gravitational attraction for a Coulomb attraction.

(P2) The only possible orbits are those for which the energy of the electron takes a value $E_n = \frac{hR}{n^2}$, for some integer *n* and with *R* the Rydberg constant (or, equivalently, for which the angular momentum of the electron is an integer multiple of $h/2\pi$).

(P3) Radiation is only emitted when an electron jumps from one orbit to another (a 'quantum transition'). The relation between the energies of the electron orbits and the frequency of the radiation is $\Delta E=h\nu$.

As Kramers et al. wrote in 1923, '[T]he impossibility of retaining [electrodynamics] in its classical form was presented in a much clearer way than ever before.' (1923, p.117).

Putting Bohr's theory in its historical context, then, claims of *external* inconsistency are to be expected. That is, Bohr's theory conflicted with many dearly held and longstanding beliefs, and for many this in itself was a great downfall. This attitude was widespread at the time:

> This is nonsense! Maxwell's equations are valid under all circumstances, an electron in an orbit must radiate. (von Laue 1914, cited in Jammer 1966, p.86)

> [The theory is] in greater or less contradiction with ordinary mechanics and electrodynamics. (Schott 1918, p.243).

But the external inconsistency of Bohr's theory is *not* what this paper is about. The more serious claim, and what allegedly makes Bohr's theory so philosophically important, is that it is *internally* inconsistent or *self*-contradictory.

There are two main features of the theory, expressed in postulates (P2) and (P3), which are repeatedly at the centre of such an inconsistency claim. These are,

(a) The mysterious ‘quantum transitions’.

(b) The non-emittance of radiation from a charged, orbiting particle.

Drawing on these, the claim is made that the theory is incoherent in its own right, regardless of any assumption or hypothesis external to it. Priest’s attitude is typical: ‘Bohr’s theory … included both classical electrodynamic principles and quantum principles that were quite inconsistent with them.’ (2002, p.122f.). This time it is clear that the inconsistency is meant to be internal. But how exactly does it manifest itself?

First I will consider (a), the quantum transitions. Certainly these were the subject of severe criticism at the time. Rutherford wrote to Bohr in 1913, ‘It seems to me that you have to assume that the electron knows beforehand where it is going to stop.’ (Pais 1991, p.153). Later, Einstein was to ask how the light quanta ‘know’ in which direction to emanate (*Ibid*.). But how does an inconsistency, as opposed to mere incompleteness, arise from these difficulties?

Brown (1992) points to the classical nature of the electron trajectories in the stationary states, writing,

> Bohr’s approach provided limited classical descriptions of the stationary states, but no account of transitions between them… This combination of classical and non-classical principles was a logically risky game… The principles are inconsistent with each other. (p.399)

And da Costa and French pick upon this passage, writing,

> Brown emphasises the point that classical mechanics was taken to apply to the dynamics of the electron in its stationary state, while quantum theory was brought into play when the transition between discrete states was

> considered—this discreteness contradicting classical physics, of course. (2003, p.91)

But still we may ask what it is about the juxtaposition of 'transitions' with the behaviour of electrons in orbits which brings about inconsistency. One thought is that the conflict lies with the *continuous* orbits and the *discontinuous* 'jumps'. But even if the jumps truly were thought of as discontinuous,[2] it still isn't obvious that we have inconsistency.

The point here is that the classical principles are confined to the orbital trajectories, and the quantum principles are confined to the transitions. As Bohr said explicitly,

> [T]he dynamical equilibrium of the systems in the stationary states is governed by the ordinary laws of mechanics, while these laws do not hold for the passing of the systems between different states. (1913, p.874)

If such a contextualisation of mutually conflicting principles did not in general prevent inconsistency, many theories we now consider to be consistent would have to be reassessed. For example, mutually inconsistent principles are used in thermodynamics for certain gases above and below a certain 'critical pressure'. There is of course no conflict here because the different principles are confined to their contexts. Whether or not we know the underlying reason for the difference—in this case changes at the molecular level—is irrelevant so far as consistency is concerned. Even before we know about the molecular change, we may simply stipulate that different principles operate above and below some given pressure.

These thoughts are summed up in the first of three general objections to inconsistency claims:

[2] It might well be objected that no break of continuity was intended by the 'jumps'. Cf. Rutherford, above.

Division of contexts objection: A theory with apparently contradictory principles is not inconsistent if it stipulates mutually exclusive contexts of application for those principles.

With Bohr's theory, as the contexts are clearly specified and are mutually exclusive, the division of contexts objection dictates that the theory is not inconsistent in this regard.

The more common focus of an inconsistency claim involves what I called feature (b), the non-emittance of radiation from a charged orbiting particle. Rather than the orbits acting as classical counterparts to quantum transitions, their *non*-classical nature is noted. The orbiting electrons are accelerated, charged particles, and according to Maxwell-Lorentz electrodynamics they are required to emit a constant stream of radiation, as opposed to the intermittent quanta of Bohr's theory. Most seriously there should be no state—the 'ground state'—where the amount of energy emitted is a maximum. On the classical picture the electrons should continue to emit radiation, causing a loss of energy and a consequent suicidal spiral trajectory into the nucleus.

Several authors highlight this aspect of the theory as the focus of an internal inconsistency. From the quotation given above da Costa and French continue,

> However it is not only in the discreteness of the states that we have conflict between quantum and classical physics but also in … the assertion that the ground state was stable, so that an electron in such a state would not radiate energy and spiral into the nucleus as determined by classical physics. *This* is the central inconsistency.

In his 1970 paper Lakatos's discussion, although ambiguous in places, can plausibly be taken as making this same point. This is how Bartelborth (1989, p.221) understands Lakatos, and it is this aspect which he also focuses on.

At this point the importance of the previous discussion distinguishing internal and external inconsistency becomes clear. In a recent paper Bueno notes this same aspect of the theory, but writes, 'Bohr … articulated an inconsistent proposal, *given the accepted theories at the time*.' (2006, p.76, my emphasis). But if Bohr's proposal was *internally* inconsistent, that surely shouldn't depend on the content of *another* theory accepted at the time. What is required for internal inconsistency, recall, is an argument that the relevant classical assumptions were *a part of* Bohr's model. And yet none of the noted authors provide such an argument.[3]

Now it is of course true that classical physics is used to characterise the electron orbits. They are assigned an angular momentum, and taken to follow a smooth, periodic trajectory; they are taken to be charged particles which are held in their orbits by a Coulomb attraction to the nucleus. So if we are going to claim that there is no internal inconsistency we need to be able to logically separate classical electrodynamics (CED) into parts, only some of which feature in the theory.

This is precisely Bartelborth's approach, in what is apparently the only paper which argues for the consistency of the theory.[4] He writes,

> [T]he only necessary theory-element from classical electrodynamics for Bohr's theory is quasi-electrostatics for point particles, because what Bohr really needed from classical electrodynamics was the concept of electric charge and Coulomb's law. (1989a, p.221)

In other words we consider classical physics as coming in independent chunks, such as 'quasi-electrostatics', from which we may pick and choose without contradiction.

That this characterisation is faithful to Bohr's theory is supported by Bohr's original wording. As seen above, he writes, '[T]he *dynamical equilibrium* of the systems in the stationary states is governed by the ordinary laws of mechanics' (my emphasis). In referring here only to the equilibrium a

[3] Shapere for one refers to 'classical electricity' and 'classical mechanics', insofar as Bohr's theory was concerned, as theory-external 'background information' (1977, p.561).
[4] Although Hendry puts forward essentially the same argument, independently of Bartelborth, in chapter 3 of his thesis (1993).

selective use of classical theory is intended. The classical physics necessary to establish the 'dynamical equilibrium' need not include the classical physics which is contradicted concerning the nature of radiation emission.

It remains to justify the division of CED into independent 'theory-elements'. Bartelborth here draws on his meta-theoretical preferences, assuring us that the fact that CED is divisible and has independent parts 'is proven by many structuralist reconstructions of physical theories.' (*Ibid*., p.222). These structuralist reconstructions may be controversial, but this is usually because they are seen to be scientifically artificial, unrepresentative of real science. As a logical possibility the required division of theory is surely unobjectionable.[5]

This discussion can be summed up in a second general objection to inconsistency claims which I now articulate:

Division of theory objection: Bodies of assumptions are not indivisible. A theory may take on board certain assumptions, without thereby committing to other, independent assumptions, whatever the reason for the original grouping.

Of course, just because certain bodies of assumptions *can* be divided does not mean that the community at the time *did* so divide them. This is the real issue, because it is by looking to theories as conceived by the relevant community that we learn more about how science works. I will return to the division of theory objection and questions of theory-identity in §4.

There is one more focus of the early Bohr-theory which has been proposed as the inconsistency. It turns out that the distinction between electron trajectories and transitions is not the only place Brown sees trouble. In his 1990 paper he tells us,

> [T]he radiation emitted by the atom is assumed to be describable in terms of classical electrodynamics (CED), while the emission and absorption

[5] Such a possible division is presumed in Norton (2000) and Hendry (1993).

processes, as well as the behaviour of electrons in stationary states, are accounted for in terms manifestly incompatible with CED. (p.285)

That this is Brown's preferred focus of inconsistency is signalled by the fact that he reiterates *this* aspect (but not the one noted above) in his 2002 paper (p.90). Here the stationary states are seen again as *non*-classical, but this time the classical focus is the emitted radiation. In this way, by giving an example of the application of CED, Brown threatens to dispel the division of theory objection used above by providing some reason to believe that the relevant parts of CED are in fact used in the theory, and thus are a part of the theory.

One possibility here is to use the division of contexts objection once again. That is, it might be claimed that whereas Bartelborth's quasi-electrostatics applies in certain contexts, CED as a whole applies in other contexts (although it would be difficult to delineate mutually exclusive contexts without overlap). Perhaps truer to the history is to take the classical treatment of the emitted radiation merely as a good approximation ('in the limit') to the use of some as yet unknown, superior laws. There is of course no contradiction in taking one law to be fundamental to a phenomenon, and another law contradictory to the first to be a good approximation for that phenomenon. An approximation *to* some part of a theory cannot be said to *be* a part of that theory.[6]

This approach takes a general form:

Reduced commitment objection: If an assumption of a theory is explicitly labelled an approximation or idealisation, then the assumption itself, without the qualification that it is an approximation or idealisation, is not a part of the theory.

[6] I distance myself from approaches such as that taken by Frisch (2005), where what is widely accepted as an idealisation assumption is made a part of the theory on the grounds that the 'idealisation assumption' is (nearly) always used by practicing physicists. See Vickers (2008).

In other words, when approximations and idealisations are involved, instead of taking a scientist to have a weakened state of *belief* in a theory, one takes a scientist to have a *full* belief in a weakened *theory* (during the process of approximation the *theory* is changed, not the character of the belief). As an example, consider the way Bohr came to explain the Pickering spectral lines of ionised helium. When he took into account the finite mass of the nucleus, this wasn't perceived as a change in theory, as it would be if the 'reduced commitment objection' were wrongheaded. The original theory did not state that the mass of the hydrogen nucleus was infinite, but rather that it could be *taken as infinite*, that it was *approximately infinite relative to the mass of the electron*. So when Bohr tackled ionised helium and took into account the finite mass of the nucleus he was then merely adding detail to the theory, rather than changing it. This will be further elucidated in §4.

For now let us take stock. We have three different foci of the 'inconsistency' in the Bohr theory of the atom:

I1 The transitions of electrons between discrete orbits, despite their continuous trajectories *in* orbits;

I2 The fact that in the stationary states (particularly the ground state) some but not all the laws of classical physics are employed;

I3 The fact that the orbits are strictly non-classical, but the radiation interacting with the atom is treated classically.

These three 'inconsistencies' are summed up schematically in the following table, with 'C' standing for 'Classical Theory':

	In orbits	Between orbits	Radiation
I1	C	~C	
I2	C and ~C		
I3	~C		C

The table depicts the way in which **I1**, **I2** and **I3** have all been referred to as conflicts between the classical and the non-classical. However, calling any one of them an 'inconsistency' implies a connection with logic which is nowhere demonstrated.[7] Bridging the gap between science and logic will be the primary concern of the next section.

3. Inconsistency and science

Let us be strict about the use of the word 'inconsistent'. It is of course a term borrowed from the logician, which can take a syntactic or semantic form. A set of sentences Γ is inconsistent iff a contradiction can be deduced or, equivalently (usually), no interpretation can make Γ true. But in both cases inconsistency is defined for a set of *uninterpreted* sentences. Do those who speak of inconsistencies in science identify scientific theories with uninterpreted sentences, then? Surely not, for such an attitude harks back to the syntactic 'received' view of Carnap et al., a view much criticised for entertaining even *partially* uninterpreted sentences. Many viewpoints in vogue today deny any linguistic identification of theories, preferring, perhaps, a deflationary or model-based approach. However, there is a strong tradition of *analysing* and *representing* theories as sets of sentences, albeit *interpreted* sentences, or *propositions*.[8] For example, da Costa and French speak of 'two contradictory propositions within … Bohr's theory of the atom.' (1990, p.186).

How might we define inconsistency for a set of propositions? The thought underlying both the syntactic and semantic definitions of inconsistency is that the set of sentences in question *cannot possibly all be true*. This is part and parcel of the semantic definition, and it follows from the syntactic definition

[7] Cf. Hendry (1993, ch.3): 'There is … no trace of a contemporary "logician's proof" of the inconsistency of Bohr's atomic model.'

[8] See van Fraassen (1991, p.30) on scientific axioms based on 'natural language'. Da Costa and French (2003, ch.2, fn.6) make a similar point.

if we agree that the logical deduction in question is truth-preserving, and that no contradiction can be true. So we might say that a set of *propositions* are inconsistent iff they cannot possibly all be true. One *sufficient* condition for the inconsistency of a set of propositions is then clear: if we can formalise the propositions (in first-order logic, say) and the resultant sentences are inconsistent by the logical definition, then the original propositions are inconsistent. Establishing the 'only if' part of the 'iff' is much more difficult, since there are other ways in which it might not be possible for a set of propositions to be true. What is necessary is to establish all the different types of truth-preserving inference. Once this is done we might say,

> A scientific theory is inconsistent iff a contradiction can be derived by employing any truth-preserving inferences.

Logical and mathematical inferences are almost always clearly truth-preserving, but other types collectively referred to as 'material inferences' make our job very difficult.[9]

Of course, to prove the *inconsistency* of a set of propositions we only need sufficient conditions, so if a contradiction follows from logical or mathematical consequence then we have our inconsistency. The difficulty with the 'only if' part just goes to show that inconsistency is usually easier to prove than consistency. Thus the majority of this paper is dedicated to questioning the kinds of claim made in §2, rather than attempting to prove the theory's consistency directly.

However, there has been one attempt to prove the consistency of Bohr's theory. Bartelborth favours the Suppesian style of theory-formalisation, which has been applied to a number of theories by philosophers in recent years. The idea is to translate the relevant mathematics into the language of set-theory, with the properties and interrelations of the material terms formally expressed. Applying this method to Bohr's theory, Bartelborth (1989b, p.99f.) defines the following material terms: particle (P), mass (m), charge (e), position (s), velocity (v), the dialectric constant (k), the stationary states (L),

[9] See Kapitan (1982) and Read (1994) for recent work here.

Planck's constant (h), orbital radius (r), time interval (T) and force (F), with each allocated some specified set of possible values. Then using only the tools of sets, elements and functions, the formal laws of the theory, implicit in (P1), (P2) and (P3), are defined. Bartelborth thus translates the following:

(i) Newton's 'F=ma'.
(ii) The Coulomb force equation.
(iii) The equation for quantised angular momentum.
(iv) The equation relating energy states of electrons to the frequency of radiation emitted.

With the theory in this form, its consistency can be proven by providing just one set-theoretical structure of the form <P,m,e,s,v,k,L,h,r,T,F> which satisfies the constraints (i)-(iv).[10]

Inevitably when translating a set of propositions into the language of set-theory something is lost in translation. However, it might be claimed that we don't lose what is relevant for consistency. But then this isn't going to be the point of contention here. Rather, I take it that nobody would question the consistency of the set of propositions Bartelborth selects to formalise. What *is* controversial is the claim that these propositions represent the content of Bohr's theory in the first place. As we will see in the next section, this content can be questioned on three different grounds, one for each of the objections defined in §2.

4. Three questions of content

How do we establish the content of a scientific theory? There has been much discussion of 'Bohr's theory of the atom' since its demise in the 1920s, but what is everyone talking *about*? Paradoxically enough, by 'Bohr's theory' we hardly ever mean the theory Bohr himself had, including all his claims about

[10] Cf. Suppes (2003, pp.30-33), and Muller (2007, p.255).

molecules and so on; instead we mean to refer to that part of it which became accepted within the community. And it is only in this latter context that inconsistency is really interesting, because it isn't so surprising that one individual can fail to see or appreciate an inconsistency in their commitments. It is much more interesting, and much more relevant to how science works in general, if we can find an inconsistency in a set of assumptions to which a large scientific community committed. But where should we draw the line between what makes it into the theory and what doesn't? Is Bartelborth justified in selecting such a small subset of Bohr's claims as 'the theory'?

Bartelborth makes his case in terms of empirical success:

> From the thus reconstructed theory it is now also possible to derive the line spectrum of the hydrogen atom, which probably represents the most important foundation of the empirical test of Bohr's theory. (1989b, p.100, my translation)

In addition, one can derive the Pickering lines of ionised helium by doubling the charge on the nucleus. It was upon the prediction of the Pickering lines that Einstein made the remark already stated in §1, 'The theory of Bohr must then be right.' Certainly it was these predictions which drew the attention of the wider scientific community, and encouraged others to work with Bohr's theory. But just because Bartelborth's axioms can reproduce the relevant predictions, should we call them 'the theory'?

The important question to ask is, would these axioms have been seen as representative of the theory *at the time*? Because if not then what Bartelborth is presenting is really a modern reconstruction, a subset of the commitments of the early quantum theorists from which the predictions of interest can be reproduced. The claim would then be that Bohr and others were committed to more than they ought to have been, but we cannot say that the theory *really was* the given subset of commitments.

In an ideal world theorists would never be over-committed, and certainly there is a close link between the success-fuelling constituents of a theory and the commitments of a community. Some of Bohr's claims were adopted by the community because they led to successful predictions and explanations; other claims, for example concerning molecules, were ignored because no such success was forthcoming. But often a community commits to more than is strictly necessary. What is controversial about Bartelborth's focus is that he utilises the concept of electric charge and the Coulomb force equation, but ignores the rest of electrodynamics. Bartelborth shows that the community only *needed* the tools he provides, but weren't they nevertheless *committed* to the rest of CED? Or, to put it another way, is the division of theory objection introduced in §2 a legitimate objection here?

Some evidence was presented in §2, above, that *Bohr* was never committed to more than electrostatics in the atomic domain. What about the rest of the community? A natural divide between theory and non-theory in Bohr's original papers presents itself in the fact that Bohr summarised the key points of his theory in the form of a set of postulates (Bohr 1913, p.874f.). The community following him then usually merely re-stated or paraphrased these postulates, as I have done in §2, above. Therefore, if the postulates themselves suggested a restriction to electrostatics, then that should be taken as the theory. The attitude of the community is well represented by Millikan, in his presentation of Bohr's theory in 1917:

> Bohr's first assumption … when mathematically stated takes the form: $\frac{eE}{a^2} = (2\pi n)^2 ma$, in which e is the charge of the electron, E that of the nucleus, a the radius of the orbit, n the orbital frequency, and m the mass of the electron. This is merely the assumption that the electron rotates in a circular orbit… The radical element in it is that *it permits the negative electron to maintain this orbit* or to persist in this so-called '*stationary state*' without radiating energy even though this appears to conflict with ordinary electromagnetic theory. (Millikan 1917, p.211f., former emphasis added)

The point is clearly made: the theory doesn't include the parts of electrodynamics which give rise to the self-radiation of accelerated charged particles. In fact, the equation given by Millikan is easily expressed using the material of Bartelborth's formalisation, as stated above.

One finds a similar story in any other textbook on Bohr's theory from that time. Electro*dynamics* is explicitly rejected, at least in the atomic domain. As Jeans (1924, p.36) writes, 'The complete system of dynamics, of which it [the quantum theory] is a part, has not yet been found.' With this attitude in place the division of theory objection finds historical justification.[11]

However, even if we agree on the content, precisely how that content is represented may still be a point of contention. In particular, the distinction between the material and the logical may not be clear. The division of contexts objection introduced in §2 stated that if the contexts of application of conflicting principles are made clear and are mutually exclusive, then the theory is not inconsistent. But could we take the attitude that the theory *is* inconsistent in such a case, but that the inconsistency is managed by introducing the contexts via some paraconsistent logic?

Brown (1992) considers the theory to be inconsistent in this way. By use of a non-adjunctive logic he aims to provide,

> [A]n appropriate closure relation on the set of principles accepted by Bohr to replace the trivial closure relation we get from classical logic. (p.405)

Clearly, for Brown, a contradiction can be derived with classical logic from the set of statements constituting the theory. Looking back to §2, and the feature I labelled **I1**, the statements in question might be 'electrons have continuous worldlines' and 'electrons move discontinuously'. If we adopted a non-adjunctive logic, however, from these statements if wouldn't necessarily be the case that we could derive a contradiction, since from 'A' and '~A' we couldn't automatically infer 'A&~A'. In other words, Brown provides a *logical* mechanism which establishes the contexts of application of the

[11] The restriction to electrostatics is also the norm in modern reconstructions. Norton (2000, p.84) writes, 'Bohr retained … the electro*static* model of electron orbits, *so that stationary states are possible*.' (my emphasis).

principles (*in* orbits and *between* orbits), instead of these contexts being a material part of the theory.

The distinction between the material and the logical is an ongoing point of contention, as the discussion in §3 indicates. Brown's suggested distinction is surely a departure from the status quo, but that would be OK so long as there was something to be gained from the change. However, Brown himself continues to refer to 'Bohr's theory' in a more traditional way. For example, in the above quotation Brown claims to be considering the 'set of principles accepted by Bohr'. But we have already seen that in Bohr's postulates the contexts of application are made clear. And Brown sometimes explicitly states that the contexts are included 'in the theory':

> [Old quantum theory] included explicit conditions restricting the application of the conflicting principles. (1992, p.404)

Da Costa and French conclude that in Brown's account the 'contexts' are 'structurally incorporated within Bohr's model.' (2003, p.89). But if a paraconsistent account is necessary then clauses delineating the contexts of application cannot be a part of the theory.

Other philosophers who discuss context-defining clauses also see them as a material part of a theory rather than part of the logic. Priest and Routley (1983) agree with Brown that inconsistent scientific theories should be handled by a paraconsistent logic. But when it comes to contexts of application—what they call 'exceptive clauses' (p.177)—they insist that after their introduction there is no longer an inconsistency (cf. p.187). Smith (1988, p.438f.) provides a more formal approach. If a theory appears to make two assertions which can be formalised as $M(x)$ and $\sim M(x)$, then what might in fact be the case is that there are certain circumstances in which $M(x)$ holds, and others in which $\sim M(x)$ holds. So in fact what we have is $Y(x) \rightarrow M(x)$ and $\sim Y(x) \rightarrow \sim M(x)$, for some uninterpreted 'Y'. Smith actually applies this method to Bohr's theory (p.438, fn.30):

> Bohr was prepared to accept that there were certain microscopic states, so called stationary states… At first, the distinction between 'stationary' and 'non-stationary' states … was purely verbal, tantamount to using 'Y' and 'not-Y'

In other words Bohr did not know what the *basis* for the division of contexts was, but he was able to make a distinction nonetheless. Here '$Y(x) \rightarrow M(x)$' and '$\sim Y(x) \rightarrow \sim M(x)$' stand for, roughly, 'If electrons are in a stationary state then they necessarily have continuous worldlines' and 'if electrons are not in a stationary state then they don't necessarily have continuous worldlines'. And to achieve consistency we don't have to know what grounds this distinction, what it is about a 'stationary state' which does the work, but simply state that it does. (Smith does find the *later* Bohr theory to be inconsistent, which will be considered in §6.)

In Bartelborth's reconstruction of the theory the contexts I have been discussing find expression. They are formalised in his set-theoretical presentation in the fact that the position, s, of an electron can take any real valued number, but the stationary states, L, take natural valued numbers. So in this respect, too, the content of the theory he focuses on finds justification, and we can say that the division of contexts objection is also a warranted objection.

Does it follow, then, that Bartelborth's claims that the theory is consistent go through? Well, if we assume that **I1**, **I2** and **I3** are the only inconsistency claims then yes. However, there is one more way in which we may question the content of Bartelborth's reconstruction. The question which remains to be asked is, what consequences follow for Bartelborth's reconstruction from what I have called the reduced commitment objection?

I want to argue that there is an important respect in which postulate P2, and equally Bartelborth's axiom for the quantisation of angular momentum, is *not* representative of Bohr's theory. Recall that the reduced commitment objection dictates that an assumption stated as an approximation or idealisation is only a part of a theory *as* an approximation or idealisation. In other words we don't have partial belief in the assumption; instead we have *full* belief in the assumption weakened by an appropriate clause (the theory is weakened rather than the belief). This is certainly more representative of real science. P2 just was taken as an approximation by the community at the time; it was not stated absolutely as P1 and P3 were. But this status is often lost in reconstructions such as Bartelborth's.

This characteristic of theories is evident in the present context. Bohr made it clear that the stationary states defined by his postulates were not meant to be the only possible states. He writes,

> There may be many other stationary states corresponding to other ways of forming the system. (1913, p.22)

In particular Bohr was thinking of elliptical orbits here (cf. p.875). And he also notes that he has 'assumed that the velocity of the electrons is small compared with the velocity of light.' The extra details would have to wait until Sommerfeld's efforts in 1916. But it is telling that nobody considered Bohr's theory superseded when Sommerfeld derived a quite different equation for allowed energy states. Rather, Sommerfeld's derivations were considered as *part of* Bohr's theory.

Thus it is my suggestion that Bartelborth's formalisation is a misleading construal of Bohr's theory is this respect. Because Bohr and those following him *intended* P2 as an approximation, then by the reduced commitment objection it (stated absolutely) was never a part of the theory, just as the assumption of the infinite mass of the nucleus was not a part of the theory. Turning to what Bohr and others were *actually* committed to, P2 needs to be watered down to give something like the following:

(P2*) Only certain orbits are possible, and $E_n = \frac{hR}{n^2}$ gives *at least a good approximation* to *at least some* of them.

The famous explanations and predictions can still be recovered using this axiom (although the predictions will have to be qualified as 'at least good approximations'). But now we also have a statement which the community actually signed up to in those early years of the theory. And P2* is also compatible with the developments courtesy of Sommerfeld—these contributions can be understood as *additions*, additional detail, rather than as *changes* to the underlying theory, in line with the use made by scientists and philosophers of science alike of the term 'Bohr's theory'.

Note that exchanging P2 for P2* gives us a weaker theory, since P2* follows from P2, but P2 does not follow from P2*. So if Bartelborth's reconstruction is consistent then so is 'Bohr's theory' as propounded here.

5. Conceptual problems

So far I have noted several ways in which the Bohr theory has been considered inconsistent, and have given several reasons to doubt the claim. But if the theory is *not* inconsistent, then a serious question arises. Just what *was* wrong with it?

Theoretical problems are often split into two categories: empirical problems and conceptual problems. That Bohr's theory of the atom was ridden with the former, especially in its later life, is universally accepted. However, when we are asking questions about the consistency of the theory, we appear to be squarely on the 'conceptual' side of the fence. For one thing we may note that criticisms of the theory were in full swing from the very beginning, when it was making exciting predictions and many of the empirical anomalies were yet to be demonstrated. For another we may note the nature of the criticism—Alfred Landé recalls scientists in 1914 stating, 'If it's not nonsense, at least it doesn't make sense.' Bohr himself is reported as saying that the theory was 'philosophically not right.' And Ehrenfest, in 1913, spoke for many when he remarked, 'If this is the way to reach the goal I must give up doing physics.'[12] Thirdly, as we shall see shortly, consistency and

[12] For these and further quotations see Pais (1991, pp.152-155), Klein (1985, p.278) and Jammer (1966, p.86f.).

related concepts are usually dubbed ‘conceptual’ issues in the philosophy of science literature (e.g. Laudan 1977, p.49). This all suggests that, if I am going to argue for the consistency of Bohr’s theory, I need a different take on the manifest conceptual difficulties. It won’t do to follow Hettema here, who, upon declaring the theory to be *consistent* (following Bartelborth), singles out only the empirical anomalies as the ‘problems’ and ‘shortcomings’ (1995, p.322f.).

In fact, the literature on conceptual problems offers several lines of attack. These come under such headings as:

1. Incompleteness
2. Vagueness
3. Incompatibility with well-grounded background assumptions
4. *Ad hoc*-ness
5. ‘(Un)smoothness’
6. ‘(Un)systematicity’

First of all we might note that Bohr’s theory was radically incomplete, in the sense that, for every question it answered, it immediately gave rise to a host of unanswered questions. However, incompleteness is only a bad thing in certain circumstances, as has been emphasised by Darden (1991, pp.201-202), and Shapere writes, ‘That a theory is incomplete … is no ground for *rejecting* the theory as false.’ (1977, p.560, his emphasis). If an understanding of the ‘degree of seriousness’ of an incompleteness is possible (Darden, p.257) then this remains to be demonstrated.[13]

Moving to ‘2’, we might note that Bohr’s theory was also rather vague, particularly when it came to the stationary states. The latter have been described as ‘uninterpreted’, ‘imperfectly understood’ and ‘not understood at all.’ (Smith 1988; French 2003). However, once again, we may question just how harmful such vague notions are for newly formed theories. Darden,

[13] We might draw here on Laudan’s claim that the age of a conceptual problem is important to its seriousness (1977, p.65f.), but this doesn’t explain the perceived problems with Bohr’s theory in the early years.

drawing on genetic theory, has stated that, 'the criterion of clarity should not be imposed too early in the stages of theory development.' (1991, p.260), and 'New scientific concepts … are often fuzzy in their early stages. Too much rigor in the early stages may not be possible or desirable.' (p.189). In fact he recommends 'beginning with a vague idea and successively refining' as a good methodological strategy (p.256).

'3' comes in various forms, depending on how we read 'background assumptions'. Shapere calls CED and special relativity 'background assumptions', whereas the term is often reserved for metaphysical beliefs (Newton-Smith 1981) or methodological preferences (Laudan 1977). More research is required in this area, but at the least we can say that background assumptions are usually regarded as *external*, and thus are not internal problems for Bohr's theory.

Perhaps the focus should be on the context defining clauses. I have noted that it is not the *existence* of the contexts which is a problem, since one can point to examples from accepted science where mutually inconsistent principles are contextualised. But perhaps there is something unpalatably *ad hoc* about the contextualisation. That is, Bohr could be seen as introducing the contexts without any grounds except to save consistency. However, it isn't clear that this kind of move is always unscientific, and thus something to reject. Leplin (1975) discusses several cases of apparently ad hoc moves including Pauli's postulation of the neutrino 'just to save conservation of energy' (p.338). As Darden notes, 'It may not be easy to find good methods for distinguishing between illegitimate ad hoc additions to [a] theory and good, newly added theoretical components.' (p.264).

This leaves us with 'smoothness' and 'systematicity', which unfortunately haven't enjoyed much attention as yet in the literature. The definitions given in Newton-Smith (1981, p,228) and Darden (1991, p.259) are somewhat preliminary, and there appears to be some crossover. Newton-Smith tells us, 'If a theory is smooth… it means that there is something *systematic* about its failures,' (my emphasis). Perhaps this is touching on the problems with Bohr's theory, as indeed it does seem to need 'a diverse range of different unrelated auxiliary hypotheses to explain the failures.' (*Ibid*.).

However, it ought to be emphasised just how successful Bohr's theory was in its early years. It immediately gave novel predictions which were in agreement with experiment to five significant figures (Pais 1991, p.149), and in addition accounted qualitatively for the size of an atom and various empirical laws such as Whiddington's law and Bragg's law (Hettema 1995, p.312ff.). But it is sometimes forgotten that it continued to produce results as years progressed. In 1916 Epstein (upon explaining the Stark effect) wrote, 'We believe that the reported results prove the correctness of Bohr's atomic model… It seems that the potentialities of quantum theory … are almost miraculous.' (Jammer 1966, p.108).

Given such achievements, it is somewhat perplexing that the theory was so derided. It falls to the philosopher of science to explain this phenomenon. So if the theory is not inconsistent, it is slightly embarrassing that we are still in the dark about the apparent conceptual problems underlying the criticism. However, could it be that we are already aware of all the conceptual problems, and that the sorry state of the theory, with the helping hand of historiography, has in fact been overstated?

The task here is to appraise Bohr's theory as it stood in 1913 and the years immediately following. This is a delicate historical matter, and little or no consideration of the fate of the theory should enter the debate. Consider the following quotations from Darden, 1991:

> In retrospect, we can see that they [the incompletenesses] were not fatal unsolved problems for the theory…, although some critics at the time saw them as such. (p.204)

> [For some] the theory was too narrowly focused and did not seem to provide a basis from which a general theory … could be constructed. (p.208)

Here Darden is in fact discussing *genetic* theory. We can now look back at *this* theory and see that the conceptual problems of the early theory of the gene would dissolve in time. And yet the two quotations discuss conceptual problems which are often cited as downfalls of the early Bohr theory. In fact, a great deal of the philosophical side of Darden's book reads like an analysis of the old quantum theory (OQT), *except* for the fact that the problems dissipate and the theory is not superseded. Thus we see a very different take on many of the 'problems' discussed here to what we find in the Bohr literature. In theory appraisal hindsight can sometimes be less of a benefit, and more of a historical barrier.

To sum up, then, it must be noted that much work, both historical and philosophical, remains to be done if we are to properly appraise Bohr's theory of the atom either in its early or later years. Ought it to have been said with confidence from the outset (as some did) that the theory was no good? Or did it in fact have problems very similar to the early genetic theory, problems which feasibly (OQT) or actually (genetic theory) would dissipate? And if the problems are as serious as most of the literature would have us believe, then how do they manifest themselves? Can we get a better grip on 'smoothness', 'systematicity' and the other nebulous conceptual problems? Here lies an important project.

6. Consistency of the later theory

So far I have focused almost exclusively on the early Bohr theory. This is usually where the attention vis-à-vis inconsistency is placed. But as Bohr's theory developed new theory elements surfaced which the community deemed successful enough to deserve a place within the theory. In this final section I consider briefly this later theory, utilising some of the preceding discussion. I turn first to the correspondence principle, which features in an inconsistency claim courtesy of Smith (1988) and then to the adiabatic principle which led to 'forbidden' predictions in 1926, and where we finally find genuine inconsistency.

The Correspondence Principle

There is good reason to include the correspondence principle in an account of at least the later Bohr theory. Kramers and Holst wrote in 1923,

> The correspondence principle has … given rise to important discoveries and predictions which agree completely with the observations… It has made possible a more consistent presentation of the whole theory, and it bids fair to remain the keystone of its future development. (p.141)

But despite the success, Smith claims that the introduction of the principle to Bohr's theory (in 1918 according to Smith) brought about an inconsistency. He declares,

> The fact that the correspondence principle was an inherent part of Bohr's research programme was undoubtedly the reason that Lakatos referred to it as progressing on inconsistent foundations. (1988, p.441)

The focus is the prediction of the *intensities* of spectral lines, where a classical understanding was invoked despite the explanation of the frequency of emitted radiation in terms of quantum transitions. Now the inconsistency was in fact resolved, Smith suggests (p.443), when Bohr postulated an 'uninterpreted mechanism' in 1924. The pattern is meant to follow that of the stationary states (see §4, above) where a blank placeholder 'Y(x)' stands for 'whatever it is which separates the conflicting principles into separate contexts'. Earlier we saw this used to represent the stationary states, and so dissolve **I1**. Here Smith takes Y(x) to represent the noted 'uninterpreted mechanism'.

I will give just one reason to doubt this logical reconstruction, focusing on the definition of the correspondence principle. It was of course a method by which classical ideas could be 'safely' applied amidst quantum ones. But how do we express its content? To provide details in a rigorous way is notoriously difficult to do, as was noted at the time:

> It is difficult to explain in what it [the correspondence principle] consists, because it cannot be expressed in exact quantitative laws. (Kramers et al. 1923, p.139)

This is confirmed by the fact that authors disagree markedly in dating the introduction of the principle. Heilbron and Kuhn (1969, p.268) say it was already used in a primitive form in 1913. Many have said it arrived properly formed in either 1917 or 1918, but Bohr first called it the 'correspondence principle' in 1920, and Eisberg and Resnick (1985, p.117) tell us that it was 'enunciated by Bohr in 1923'. Crucially for Smith, most authors don't consider OQT to tell us anything about transition probabilities/spectral line intensities. Eisberg and Resnick write, 'The theory … does not tell us how to calculate the intensities of spectral lines.' (1985, p.119) and Shapere is very clear:

> [T]he Bohr theory offered no way to account for the intensities and polarizations of the spectral lines… Use of the correspondence principle as a basis for calculating the polarizations of the lines is not considered here as a 'part of the theory.' The principle was not, in any case, very successful with regard to the intensities. (1977, p.559)

If, as has been suggested, success generation is an important factor for theory membership, Smith has much work to do to convince that the correspondence principle, in a form which includes the calculation of line intensities, is a part of the theory. As it stands Smith's 'inconsistency' appears to be based on a somewhat biased understanding of the content of the correspondence principle.

Is Smith supported by Lakatos, as he claims? In fact Lakatos refers to the correspondence principle as an '*ad hoc* stratagem', which helps to 'conceal the graft [mixing of principles]', and in so doing 'reduce[s] the degree of problematicity of the programme' (1970, p.144). It appears that the theory was *already* inconsistent in Lakatos's eyes, and the introduction of the correspondence principle helped to accommodate the inconsistency somehow. This is, then, something like the opposite of what Smith claims.

The Adiabatic Principle

Despite what has been said there should be no doubt that scientific theories *can* be inconsistent. What is required is for the three objections noted in §2 to fail to apply, and for a contradiction to follow by acceptably truth-preserving inferences, whether logical or material. It is convenient that this can be demonstrated by turning to the later Bohr theory, after the introduction of the quantum adiabatic principle by Paul Ehrenfest. Making use of the adiabatic principle, a contradiction was derived in 1926.

First, a few quotations should suffice to show that the adiabatic principle was a part of the theory. Ehrenfest finally brought it to the attention of the scientific community in a 1917 paper. At first it wasn't clear how it could be used in specific calculations, but this soon changed. As Brown notes, 'With its help, he [Ehrenfest] was able to determine quantization rules for a wide range of systems, given only a rule for one of them.' (1992, p.402). Sommerfeld, at first sceptical, wrote of the predictive power of the principle in 1919 (see Klein 1985, p.291). And its explanatory value at the time is undeniable: Jammer writes, 'The adiabatic principle … revealed the mystery of the quantum conditions.' (1966, p.101). The 'wide and ingenious use Bohr had made of the adiabatic principle' might also be noted (Bergia et al. 2000, p.28). In short we have prediction, explanation, ubiquitous use and widespread endorsement from the physics community. It is surely right to say that, 'from 1917, Ehrenfest's adiabatic principle remained strictly linked with the development of OQT.' (Bergia et al., p.10).

Two systems are adiabatically related if the second can be achieved by taking the first and changing a certain parameter infinitely slowly and smoothly (see Scerri (1993)). For present purposes there is no need to go into the details of the principle; it is enough to note that it was a way to proceed from knowledge of a familiar quantum system to knowledge of a new quantum system. Pauli, in 1926, published a paper applying the principle to the hydrogen atom. It showed that 'allowed' orbits (according to the theory) could be transformed into what he called 'forbidden' orbits. He concluded that,

> An escape from this difficulty can be achieved only by a radical change in the foundation of the theory. (quoted in Mehra et al. 1982, p.509)

The question now is, why are the resultant orbits 'forbidden'? If they are forbidden because in contradiction with another claim of the theory, then we have our inconsistency—two contradictory theorems of the same set of accepted assumptions.

This is exactly what we find. In 1916 Sommerfeld further modified Bohr's quantum condition by introducing a third quantum number, the 'magnetic' quantum number *m*, in order to explain the normal Zeeman effect (see Pais 1991, p.199). In the process he had to make a decision regarding so-called 'pendulum orbits'. These were 'orbits' where, instead of going around the nucleus, an electron would head *straight for* the nucleus, apparently travel *through* it, and then after coming to a halt reverse its steps and repeat the process. Still convinced that the orbits were truly physical Sommerfeld made such 'pendulum orbits' theoretically impossible, by stating within the new quantum condition that the magnetic quantum number couldn't be zero: '$m\neq0$'.[14] But '$m=0$' is precisely what Pauli derived in 1926.

Since I am only interested in the theory insofar as it represents the commitments of a large community, Sommerfeld's particular beliefs are not enough to establish my claim. We must ask whether others were convinced that the orbits were real, and thus convinced that $m\neq0$ was a necessary part of the theory. Simply put, the fact that Sommerfeld's introduction of elliptical and relativistic orbits had been so successful convinced nearly everyone, although there were some notable objectors. Debye (who independently explained the Zeeman effect in the same way as Sommerfeld in 1916) was

[14] As Darrigol puts it, 'Bohr and Sommerfeld excluded the value $m=0$ on the grounds that the corresponding orbit is adiabatically connected to an orbit passing through the nucleus.' (1992, p.188). See also Lindsay (1927, p.413).

one such objector. When Stern and Gerlach tested the explanation of the Zeeman effect in 1921 Debye wrote, ‘But you surely don’t believe that the [spatial] orientation of atoms is something physically real; that is [only] a prescription for the calculation, a timetable for the electrons.’ (quoted in Rigden 2003, p.105). However, the results of the experiment seemed to go against Debye’s scepticism (Ibid., p.106f.), providing further evidence for ‘orbit-realism’. If not the whole community, then at least a large proportion made a serious commitment to Sommerfeld’s $m \neq 0$ during those years.

This particular inconsistency is unaffected by the three objections introduced above. The ‘division of contexts objection’ fails because both Sommerfeld’s quantum condition and the adiabatic principle were explicitly meant to apply to the hydrogen atom, which was the subject of Pauli’s calculation. The conditions which had to be satisfied for the adiabatic principle to be applicable to a system were very carefully specified by the community, as follows:

(i) The introduction of external forces should not alter the degree of periodicity of the system;

(ii) The system should be simple-periodic or multiple-periodic (not aperiodic).

The hydrogen atom satisfies these conditions (as explained by Scerri 1993, p.52). The ‘division of theory objection’ doesn’t get off the ground—the relevant assumptions were explicitly made part of the theory by practicing scientists. And the ‘reduced commitment objection’ also fails—the adiabatic principle certainly wasn’t meant as an approximation, and however Sommerfeld’s quantum condition was meant to be approximate (for example because it ignored electron spin) his stipulation that $m \neq 0$ was absolute. Finally then we can conclude that, from roughly 1917 to 1926, it was possible to derive a contradiction from what counted as ‘Bohr’s theory of the atom’, at least according to a majority of the relevant community.

Of course there was no reason to doctor Bohr’s theory in the face of the inconsistency, since by 1926 Heisenberg’s matrix mechanics and

Schrödinger's wave mechanics were changing the landscape dramatically. But it is interesting to note that, absent these new developments, the consistency of the theory could have been recovered without the 'radical change' that Pauli supposed was necessary. In fact, a suggestion made by R. B. Lindsay in 1927—although not motivated by Pauli's result—would have done the trick. First, $m \neq 0$ should be ejected from the theory, which seems to admit the unpalatable 'pendulum orbits'. Lindsay then provides a way forward:

> [T]he idea of the passage of the electron through the nucleus may be distasteful to some. There is a possible way of avoiding this, namely, by the introduction of a repulsive force (in addition to the inverse square attractive force) operative only in the immediate vicinity of the nucleus. (1927, p.415)

Thus the electron would not travel through or collide with the nucleus, but would bounce back with the newly introduced repulsive force. However, with so many *empirical* problems facing the theory, including the anomalous Zeeman effect and the line spectra of elements heavier then Hydrogen, the recovery of the consistency of the theory would hardly have been much of a comfort.

7. Conclusion

It has been argued that, despite a tradition to the contrary, the early Bohr theory is *not* inconsistent, at least according to what is usually meant by 'Bohr's theory' and 'inconsistent'. Only the later Bohr theory is genuinely inconsistent, but this went unnoticed for roughly nine years, from 1917 to 1926, when Pauli finally derived the contradiction. This version of events obviates the awkward necessity traditionally encountered of explaining why scientists put up with an inconsistent theory for so long. On my analysis scientists never put up with a theory that they knew to be inconsistent. On the contrary, Pauli demanded a 'radical change in the foundation of the theory' when he uncovered a contradiction.

Perhaps the most important general conclusion is that more care should be taken before inconsistency is predicated of a scientific theory. Often this is the easy option, which apparently explains the failure of a theory at a stroke. Unfortunately this can, as in the case of Bohr's theory, obfuscate the more complicated *real* explanation as to why the theory fails. In any case of alleged inconsistency two questions must be asked: (i) 'What exactly should we take to be the content of the theory?', and (ii) 'Does a contradiction follow from that content by acceptably truth-preserving inferences?' A scientific theory is inconsistent if, and only if, the answer to the second question is 'Yes'.

References

Bartelborth, T. (1989a): 'Is Bohr's Model of the Atom Inconsistent?', in P. Weingartner and G. Schurz (*eds*.), *Philosophy of the Natural Sciences, Proceedings of the 13th International Wittgenstein Symposium*, HPT, pp. 220-223.

Bartelborth, T. (1989b): 'Kann es Rational Sein, eine Inkonsistente Theorie zu Akzeptieren?', *Philosophia Naturalis*, **26**, pp. 91-120.

Bergia, S., Navarro, L. (2000): 'On the early history of Einstein's quantization rule of 1917', *Archives Internationales d'Histoire des Sciences* **50**, pp.321-373.

Bohr, N. (1913): 'On the Constitution of Atoms and Molecules', *Philosophical Magazine* (6), **26**, pp. 1-25, 476-502 and 857-875.

Brown, B. (1990): 'How to be Realistic about Inconsistency in Science', *Studies in History and Philosophy of Science*, **21**, pp. 281-294.

Brown, B. (1992): 'Old Quantum Theory: A Paraconsistent Approach', *PSA 1992*, Vol.2, pp. 397-411.

Brown, B. (2002): 'Approximate Truth: A Paraconsistent Account', in J. Meheus (*ed*.), *Inconsistency in Science*, Dordrecht, Kluwer, pp.81-103.

Bueno, O. (2006): ‘Why Inconsistency is not Hell: Making Room for Inconsistency in Science’, in Erik J. Olsson (ed.) *Knowledge and inquiry: essays on the pragmatism of Isaac Levi*, Cambridge, Cambridge University Press, pp.70-86.

Da Costa, N.C.A. and French, S. (1990): ‘Belief, Contradiction and the Logic of Self-Deception’, *American Philosophical Quarterly*, **27**, no.3, pp. 179-197.

Da Costa, N.C.A. and French, S. (2003): *Science and Partial Truth*, Oxford: Oxford University Press.

Darden, L. (1991): *Theory Change in Science: Strategies from Mendelian Genetics*, OUP.

Darrigol, O. (1992): *From c-Numbers to q-Numbers*. Oxford: University of California Press.

Feyerabend, P. (1978): ‘In Defence of Aristotle’, in G. Radnitsky and G. Anderson (eds.), *Progress and Rationality in Science*, Reidel, Dordrecht.

French, S. (2003): ‘A Model-Theoretic Account of Representation’, *Philosophy of Science*, **70**, pp. 1472-1483.

Frisch, M. (2005): *Inconsistency, Asymmetry, and Non-Locality*. Oxford: OUP.

Heilbron, J. and Kuhn, T. (1969): The Genesis of the Bohr Atom. *Hitorical Studies in the Physical Sciences*, **1**, 211-290.

Hendry, R. F. (1993): *Realism, History and the Quantum Theory: Philosophical and Historical Arguments for Realism as a Methodological Thesis*. PhD thesis: LSE.

Hettema, H. (1995): ‘Bohr’s Theory of the Atom 1913-1923: A Case Study in the Progress of Scientific Research Programmes’, *Studies in History and Philosophy of Modern Physics*, **26**, pp. 307-323.

Jammer, M. (1966): *The Conceptual Development of Quantum Mechanics*. McGraw-Hill.

Jeans, J. H. (1924): *Report on Radiation and the Quantum Theory*. London: Fleetway Press.

Kapitan, T. (1982): ‘On the Concept of Material Consequence’, *History and Philosophy of Logic* **3**, pp.193-211.

Klein, M (1985): *Paul Ehrenfest. The Making of a Theoretical Physicist*. North-Holland Physics Publishing.

Kramers, H. and Holst, H. (1923): *The Atom and the Bohr Theory of its Structure*. London: Gyldendal.

Lakatos, I. (1970): 'Falsification and the Methodology of Scientific Research Programs', in I. Lakatos and A. Musgrave (*eds*.), *Criticism and the Growth of Knowledge*, 1970, Cambridge: Cambridge University Press, pp. 91-195.

Laudan, L. (1977): *Progress and its Problems*, University of California Press.

Leplin, J. (1975): 'The Concept of an *Ad Hoc* Hypothesis', *Studies in the History and Philosophy of Science* **5**, no.4, pp.309-345.

Lindsay, R. B. (1927): 'Note on 'Pendulum' Orbits in Atomic Models', *Proceedings of the National Academy of Sciences of the United States of America* **13**, pp.413-419.

Mehra, J. and Rechenberg, H. (1982): *Historical Development of Quantum Mechanics*, Springer.

Muller, F. A. (2007): 'Inconsistency in Classical Electrodynamics?', *Philosophy of Science*, **74**, pp. 253-77.

Newton-Smith, W. H. (1981): *The Rationality of Science*. Boston: Routledge and Kegan Paul.

Norton, J. (2000): 'How We Know About Electrons', in R. Nola and H. Sankey (eds.), *After Popper, Kuhn and Feyerabend*, 2000, Kluwer, pp.67-97.

Pais, A. (1991): *Niels Bohr's Times*, Oxford: Oxford University Press.

Priest, G. (2002): 'Inconsistency and the Empirical Sciences', in J. Meheus (*ed*.), *Inconsistency in Science*, Dordrecht, Kluwer, pp.119-128.

Priest, G. and Routley, R [1983]: *On Paraconsistency*, Research Report 13, Logic Group, Research School of Social Sciences, Australian National University.

Read, S. (1994): 'Formal and Material Consequence', *Journal of Philosophical Logic* **23**, pp.247-265.

Rigden, J. S. (2003): *Hydrogen: The Essential Element*. Cambridge, Massachusetts: Harvard University Press.

Scerri, E. R. (1993): ‘Correspondence and Reduction in Chemistry’, in S. French and H. Kamminga (eds.), *Correspondence, Invariance and Heuristics: Essays in Honour of Heinz Post*, Kluwer.

Schott, G. (1918): ‘On Bohr’s Hypothesis of Stationary States of Motion and the Radiation from an Accelerated Electron’, *Philosophical Magazine* **45**.

Shapere, D. (1977): ‘Scientific Theories and Their Domains’, in F. Suppe (ed.), *The Structure of Scientific Theories*, Illinois, University of Illinois Press, pp.518-565.

Smith, J. (1988): ‘Inconsistency and Scientific Reasoning’, *Studies in History and Philosophy of Science*, **19**, pp. 429-445.

Suppes, P. (2003): *Representation and Invariance of Scientific Structures*, Stanford: CSLI Publications, 2003.

Van Fraassen, B. (1991): *Quantum Mechanics, An Empiricist View*, OUP.

Vickers, P. J. (2008): ‘Frisch, Muller and Belot on an Inconsistency in Classical Electrodynamics’, forthcoming in the *British Journal for the Philosophy of Science*.