Part XII

LOGICAL INCONSISTENCY IN SCIENTIFIC THEORIES

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Old Quantum Theory: A Paraconsistent Approach[1]

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1. Cognitive Commitments in Science

Just what form(s) our cognitive attitudes towards scientific theories take, and what forms they *should* take, is a long-standing puzzle in philosophy of science. Debates continue, between various realist and anti-realist positions, probabilistic models, and others. The nature of cognitive commitment becomes particularly puzzling when scientists' commitments are (at least apparently) inconsistent. Since there are no models of inconsistent sets of sentences, straightforward semantic accounts fail. And syntactic accounts based on classical logic also collapse, since the closure of any inconsistent set under classical logic includes every sentence. Probabilistic models can survive, of course, since the members of a sufficiently large inconsistent set can all have probabilities as high as you like. H. Kyburg has used this fact to argue for a form of inconsistency-tolerance- but his approach assumes a coherent probability assignment on the basis of which we choose what to accept for certain purposes (Kyburg 1983, pp. 232-254). However, it's harder (in purely calculational terms) to ensure that our commitments are probabilistically coherent than to ensure they are consistent. So we must expect that similar problems will arise for probabilistic models of commitment.

Inconsistency, moreover, is not as uncommon as we might wish. From early calculus, naive set theory and naive semantics, to old quantum theory and contemporary tensions between quantum mechanics and general relativity, inconsistencies have often infected our best efforts in science and mathematics. Worse, it's often far from easy to remove them- and until they are removed, classical logic can tell us absolutely nothing about how to go on reasoning. "Allez en avant, et la foi vous viendra." sounds reassuring, and may do quite nicely for the practitioner. But philosophers need a systematic account of *how* it's possible to go on at all.

In this paper I propose a new class of models for cognitive commitment based on a form of *paraconsistent* logic. The term is due to F. Miró Quesada; a logic is paraconsistent if the logical closure of some classically inconsistent sets of sentences is nontrivial. These models are offered as a reasonable account of commitment to inconsistent sets of sentences; I defer until another occasion the development of similar models aimed at coping with incoherent probability assignments. Old quantum theory

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(OQT) will be our test case- these models, I will argue, provide a plausible account of cognitive commitment to OQT. Beyond this, I believe that some features of this sort of commitment, in particular its *context dependence*, are very common in science. These models provide formal tools for representing context-restricted commitments, even when we have not quite worked out the details of just what is restricted to which contexts- tools which may well be useful even when inconsistency is not in the air.

This sort of work inevitably raises a difficult question: Just how should we test and compare different models of cognitive commitment? Our understanding of many phenomena in science, including hypothesis testing, explanation, inference, and theory extension, is affected by the account we give of cognitive commitment. But how actual evidence from scientific practice should guide our choice of an account of cognitive commitment is hard to say. Just to begin with, there is the tension between descriptive and normative roles for philosophical accounts of science: Obviously, the "correct" account of commitment can't be expected to fit all actual scientific practice. On the other hand, extensive failure to fit (in the light of the plausible presumption that much of what goes on in science is sound and sensible) is bad news. But exactly how much fit is enough is a very difficult question. This problem is particularly severe in this case, since the models I am proposing here are aimed at making sense of serious commitments to inconsistent sets of sentences. But many philosophers are inclined to insist that any such commitment is incoherent- thus for some the very fact that these models allow us to make sense of such commitments counts against them, rather than for them.

As a result my goal here must be modest. I can't hope to show that the models I am proposing are the normatively or descriptively right account of cognitive commitment to OQT. What I aim to do is to present some evidence that there really was (on the part of some scientists, at least) a substantial cognitive commitment to OQT, and that some of its characteristics have a simple and straightforward explanation in terms of the model I propose.

2. The Bohr Hydrogen Atom

Bohr's model of the atom applied Planck's' theory of quanta to the task of making better sense of Rutherford's model of the atom. However, Bohr had serious reservations about Planck's theory of black-body radiation:

> In formal respects Planck's theory leaves much to be desired; in certain calculations the ordinary electrodynamics is used, while in others assumptions directly at variance with it are (used)...without any attempt being made to show that it is possible to give a consistent explanation of the procedure used.

Nevertheless, and almost in the same breath, he declared:

> It is...hardly too early to express the opinion that whatever the final explanation will be, the discovery of "energy quanta" must be considered as one of the most important results arrived at in physics, and must be taken into consideration in investigation of the properties of atoms.(Bohr 1922, p. 6)

These later reflections seem to represent an ongoing ambivalence. In a 1913 letter to the editors of the *Philosophical Magazine*, Bohr remarks, regarding the Rutherford model: "Just this necessity, however, of a definite departure from ordinary mechanics seems to offer the possibility of a theory of a formal consistency greater than that possessed by Planck's original theory."(in Rosenfeld and Hoyer 1981, p. 313) Bohr needed Planck's quanta for his atomic theory- at the outset of his essay for needed Planck's quanta for his atomic theory- at the outset of his essay for *Philosophical Magazine* in which his model of the hydrogen atom is first set forth he invokes Planck's theory in the following terms:

> The essential point in Planck's theory of radiation is that the energy radiation from an atomic system does not take place in the continuous way assumed in the ordinary electrodynamics, but that it, on the contrary, takes place in distinctly separated emissions, the amount of energy radiated out from an atomic vibrator of frequency in a single emission being equal to nhv, where n is an entire number, and h is a universal constant.(Bohr 1913, p. 4)

But Bohr also complains that Planck gives no systematic account of how to derive the desired consequences of the inconsistent theory while avoiding absurdities.

Bohr's atomic theory improves on Planck by carefully distinguishing the contexts in which the contrary principles he uses are to be applied:

> 1) The dynamical equilibrium of the systems in the stationary states can be discussed by the help of the ordinary mechanics, while the passing of the systems between different stationary states cannot be treated on that basis.
>
> 2) That the latter process is followed by the emission of a *homogeneous* radiation, for which the relation between the frequency and the amount of energy emitted is that given by Planck's theory.(Bohr 1913, p. 7)

When it comes to describing this radiation (including its interaction with our various instruments) classical electrodynamics is the only game in town. But classical electrodynamics is not to be applied to the atom in its stationary states; if it were the result would be an immediate, violently energetic collapse, distinctly at odds with the observed long-term stability of matter. And neither classical mechanics nor classical electrodynamics is to be applied to the transitions between stationary states during which radiation is emitted or absorbed. Ambivalence appears here as well, with Bohr asking McLaren "Do you really think such horrid assumptions as I have used, necessary?"(in Rozental 1967, p. 53) while discussing his surrender of the classical connection between frequency of radiation emitted and some frequency of the electron's motion.

Bohr's approach provided limited classical descriptions of the stationary states, but no account of transitions between them. Energy differences between the stationary states together with the Planck frequency condition were then used to determine the frequency of radiation emitted or absorbed in a transition. This combination of classical and non-classical principles was a logically risky game. Whatever form of commitment to these principles Bohr was proposing, it was not a commitment closed under the classical consequence relation: The principles are inconsistent with each other, so their classical consequences include every sentence in the language. And of course things are equally bad if we try to treat the commitment semantically: There are no models of an inconsistent set of sentences, so we can't regard this commitment as any sort of attitude towards the set of models of CED, classical mechanics, and the quantum principles taken together.

Bohr's final evaluation of OQT continues his earlier ambivalence:

> If this connection (between quantum physics and classical physics) had merely had that asymptotic character which one might expect from the correspondence principle, then we should not have been tempted to apply mechanics as crudely

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chanical considerations that were helpful in building up the analysis of optical phenomena which gradually led to quantum mechanics.(in Rozental 1967, p. 73)

When Bohr's theory was initially introduced, it was greeted as a considerable accomplishment despite the obvious logical difficulties. The fact that it allowed an account of the normal hydrogen spectrum (something it was not in fact designed to do; this application of the theory emerged very late in Bohr's work on the trilogy of papers in which he presented his theory for the first time) convinced many right at the start that Bohr was on to something.

That there might be a serious *context-restricted* form of commitment here is suggested by a number of things Bohr said. For example, in a letter to Oseen, describing a conversation with Debye, Bohr says "In the discussion I tried to say that the necessity of such a (general) principle was perhaps not so evident... that the possibility of a comprehensive picture should perhaps not be sought in the generality of the point of view, but rather in the strictest possible limitation of the applicability of the points of view."(in Rosenfeld and Hoyer 1981, p. 563). And in another letter to Oseen Bohr expressed the hope that his views might be compatible with Maxwell's laws applying fully to light propagating through a vacuum, i.e. that the phenomena which require quantum treatment might be restricted to interactions between light and matter. Here at least Bohr is seeking a division of contexts which would restrict the use of quantum and classical physics, avoiding simultaneous applications of conflicting principles, while allowing the use of each in contexts where it was indispensable. And he seems to be suggesting that such a patchwork "theory" may be the best we can get.

Further, if we look at the confirmation of the theory by various bits of evidence, we find cases in which literal commitment to the theory plays an important role: For example, early work on magnetic moments of atoms and the rotational moment imposed by magnetizing an iron bar was taken to confirm the existence of rotating nonradiating charges by appeal to other CED features of rotating charges (Einstein and de Haas 1915, p. 170). This makes sense only if (for some at least) commitment to some of OQT's oddest features was pretty serious.

3. Extending Bohr's Model

OQT was seriously incomplete. Bohr himself said it gave "no explanation in the ordinary sense" of the atom's emission and absorption of light (in particular, the frequency of the light emitted or absorbed had nothing to do with any frequency of motion of the electron involved). Further, Bohr's model of the hydrogen atom provided no account of selection principles (restricting which state to state transitions could occur), or the polarization of emitted radiation. And the classical account of energy differences between states of a system in terms of the energy required for an adiabatic transition from one to the other had apparently been given up. Somehow classical theory's more wide-reaching explanatory capacities needed to be brought to bear-they offered the only available account of such phenomena. The gradual extension and improvement of OQT involved adding more and more apparatus drawn from classical theory, and integrating that apparatus with the quantum principles, all the while trying to avoid trivialization. This process led to Sommerfeld's relativistic treatment of Zeeman splitting, the gradual extension of the correspondence principle, and to the adiabatic principle first laid out by Paul Ehrenfest, which gave the first systematic insight into quantization rules for a wide range of periodic systems.

The extension of OQT with the adiabatic principle and the correspondence principle made OQT a much more complete theory, but only within a restricted range of applications. OQT was never treated as a global theory of physics, but instead as a theory of a limited, though important, range of phenomena. In other areas, full reliance on classical mechanics and electrodynamics remained the only game in town. Finally, it's worth noting that application of OQT to experimental results relied on classical interpretation of the *instruments* involved in making the measurements: spectroscopes, the macroscopic magnetic and electrical fields which produce the Zeeman and Stark effects, and so on are all understood in terms of CED. This was particularly important to Bohr, who emphasized the point in his criticism of Einstein's quantum theory of light (Bohr et al. 1924, p. 157). Bohr later extended this role of classical physics in observation into a fundamental feature of his interpretation of quantum mechanics.

By 1926, OQT was showing its limitations. The He atom and anomalous Zeeman effect constituted serious difficulties as problems that were important, but also extremely recalcitrant for the program. But they did yield partially to analysis in terms of some of the basic principles of the program. The result was perhaps more disappointment at the difficulty of the problems than a sense that the program was in deep trouble. However, Pauli discovered in 1925 that the hydrogen atom in crossed fields admitted a periodic classical model of the stationary states, and that when the adiabatic principle was applied to the model, one could convert allowed states into forbidden ones (Pauli 1926, pp. 163-64). Pauli's result emerged as a natural continuation of the program of OQT, as the principles developed in earlier applications were applied to more and more complicated cases; This time it was clear that the basic principles of the program led to untenable results- even when applied within the contextual restrictions that the program required.

The adiabatic principle and the correspondence principle will be important to our later discussion, so we'll take time now to say a little more about them. The adiabatic principle was applied to give a proper definition of the energy differences between the different stationary states, and to provide a unified account of the quantization rules for systems that could be linked by an adiabatic transformation. The correspondence principle has two components. First, it holds that OQT agrees with classical mechanics in the region of sufficiently large quantum numbers Second, it was later extended to include the claim that coefficients in a classical Fourier series representing the motion of the electron can be used to calculate the intensities and polarization of spectral lines throughout the range of quantum numbers.

4. The Adiabatic Principle

The adiabatic principle made a proper account of the energy differences between different quantum states possible. In classical physics the energy required for an adiabatic change from one state to another determines the difference of energy between the states. But by ruling out the intermediate states which violated his quantum restrictions, Bohr seemed to give up the possibility of characterizing energy differences between stationary states in this way. In response to this problem, Ehrenfest developed the adiabatic principle, which holds that adiabatic transformations of a quantum system preserve the quantum-mechanically allowed states, taking allowed states in the initial system to allowed states in the resulting one. Energy differences between states of a system could then be defined as the energy required to transform a system where the energy difference between the states was arbitrarily close to 0 into the intended system (so long as the transformation in question did not lead through a degenerate state in which the energies of distinct states of the target system were identical). This principle subsequently played an important role in unifying the quantization rules that pick out the allowed quantum states of different systems.

This application of classical thermodynamics was partly motivated by Ehrenfest's recognition of the importance of Wien's law to quantum theory. With its help, he was able to determine quantization rules for a wide range of systems, given only a rule for one of them: Whenever a continuous, slow modification of the state of a system leads to a state of some other system we might want to describe, the stationary states of the resulting system are determined by the results of slow deformation of the original system's stationary states. Thus to obtain the appropriate quantization rule for the system, all that's required is to find the right adiabatic invariants in the transformation from a system with a known quantization rule to the system in question.

The preservation of stationary states through adiabatic transformations greatly expands the range of classical concepts applicable to the description of the stationary states. Though it leaves the largest gap in old quantum theory open, viz. the puzzle of transitions and their associated absorption and emission of radiation, it provides a classical basis for identifying the stationary states, connecting them to conditions for preserving Boltzmann's statistical version of the Second Law of thermodynamics (Ehrenfest 1917, pp. 88-89).

5. The Correspondence Principle

At first the correspondence principle only connected frequencies of light emitted in transitions between states with high quantum numbers to the frequencies in a Fourier-series representing the electron's classical motion in the states. But it was later extended to give a general account of polarization and intensity results for spectral lines resulting from transitions between any pair of quantum states.

On the classical view the coefficients of the terms in the Fourier series representing the electron's motion would give the intensity of the radiation at the given frequencies. Applying this to the stationary states of the quantum theory, Bohr suggests that the coefficients represent probabilities of the transition from one state to another.(Bohr et al., 1924, p. 163) Polarization results when coefficients characterizing the motion in one or two directions equal 0. The classical mechanical description of the electron's motion is the starting point of the inference. Some of the classical electrodynamic consequences of that motion are then deduced for application to the quantum model of the atom. The frequency condition linking the electron's frequencies of motion to the frequencies of the emitted light is left behind, but the information on intensities and polarization is retained. The results agreed with experiment, and the correspondence principles' extension was adopted.

Bohr began by identifying an important gap in the existing quantum theory, and then drew on the more complete classical theory for results which might fill the gap without leading to a contradiction. Thus he suggested that the coefficients which determine intensities of radiation in the classical picture be applied to the same purpose in the quantum theory. They had to be transformed, in a sense, in order to do this job: Instead of directly characterizing the amplitude of the electron's vibrations at a given frequency, they instead had to determine probabilities of transitions from one state to another, explaining the relative intensities of various spectral lines in a statistical way. Nevertheless, here we see a consequence of applying classical electrodynamics to the Bohr atom adapted and applied to Bohr's theory, with the upshot that the theory's predictive success is improved.

This is a more or less direct borrowing of intensity relations from CED. The connection of the intensity results to particular spectral lines is suggested by the original correspondence principle, which applied only at high quantum numbers and maintained the classical frequency relation as well as the intensity results. The interpretation of the intensity results in terms of the Bohr model links the coefficients to transitions from the given level, so that the frequencies associated with each coefficient must be the frequencies corresponding to a transition between the initial level and the appropriate lower level. For high quantum numbers these frequencies will be (almost) the same as the classical frequencies of the components of the electron's motion. For lower quantum numbers this must be given up, but the relation between coefficients and intensities can be retained.

Here we see an interesting sort of export and import of principles from and into the isolated contexts that Bohr had invoked to keep inconsistent principles separate. A description that holds of the classically described electron in the stationary states was invoked to provide some account of which transitions do and do not occur, and the polarization of the resulting radiation. Some of the links between the classical motion of the electron and the emitted radiation were thus retained, but not enough to force the classical relation between the frequencies of the electron's motion and the frequency of the emitted or absorbed radiation. The result was a huge increase in the power of the quantum theory, at very small cost. The necessary materials were already there in standard electrodynamics; adding just enough of CED allowed Bohr to understand aspects of one electron spectra that could not be understood within Bohr's original atomic theory.

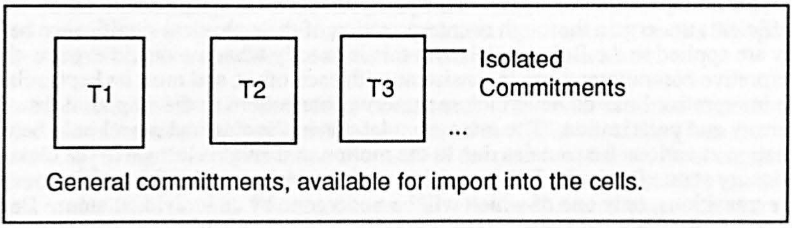
What is imported into OQT is still a pretty low-level consequence of CED. The coefficients undergo a thorough re-interpretation of their physical significance before they are applied to the Bohr model. But this is exactly what we should expect: the interpretive commitments are inconsistent with each other, and must be kept isolated. The interpretive links do nevertheless preserve connections to the empirical data on intensity and polarization. The intensity relations in the classical model hold between radiation at various frequencies due to the motion of a single electron in the classical stationary state. But in the Bohr model they characterize probabilities of various possible transitions, only one of which will be undergone by an individual atom. Despite this re-interpretation, however, Bohr is clearly drawing on CED's answer to these questions in order to obtain a quantum answer. The result is a mysterious link between the classical behavior of a single Rutherford hydrogen atom and the statistical behavior of a large number of quantum atoms. The success of this link in providing an account of the actual data on intensities and polarizations encouraged the hope that a complete theory might ultimately be arrived at by such judicious borrowing from classical physics, coupled with gradual refinement of the quantum principles.

Some might reply that this is simply an analogy between CED and the quantum theory. But even if this particular application is just an analogy, and demonstrates only the methodological or heuristic indispensability of CED to OQT, it remains an important point. The fact that each isolated application of CED results to OQT can be treated merely as a sort of formal analogy between OQT and CED, does not show that the result of doing this in each case is an adequate account of the cognitive commitments of the scientists involved. The point is, why do they develop and extend their commitments in the way they do, if all their commitments are captured by the analogies discovered to date? Some sort of serious commitment to the inconsistent principles seems to be required, if we're to account for the way in which scientists drew on them as they developed and extended OQT.

Notice, for instance, van Vleck's subsequent extension of the correspondence principle to absorption of radiation (Van Vleck 1924, p. 330)- as Van Vleck showed, setting the transition probabilities to fit the emission data automatically ensured that absorption too would fit. Van Vleck also took the fact that Kramer's formula for dispersion met the requirements of correspondence as an important argument for the correctness of the formula. The commitment to the correspondence principle included an ongoing commitment to extend its application to a wide range of data not initially encompassed by it. Each such extension could be treated as a merely formal analogy between quantum theory and classical mechanics *once it was discovered.* But this is not adequate as a characterization of the attitude of the physicists involved: Some at least- Sommerfeld in particular, regarded these successful extensions as confirmation of the general principles guiding their construction, just as if they were dealing with an ordinary, consistent theory.

6. A Schematic Account of OQT in Practice

OQT involved extensive use of classical mechanics and electrodynamics together with quantum restrictions inconsistent with classical physics. It included explicit conditions restricting the application of the conflicting principles. While applications of the theory involved inconsistent principles at various points, the inconsistencies were isolated in separate sub contexts. Within these sub contexts the principles were treated as though true, and consequences deduced. Some of those consequences served in turn as input for calculations carried out in other sub contexts, using other, incompatible principles.



The isolated commitments were kept separate within their cells, but some consequences inferred within those cells were allowed into the store of general commitments. These consequences were then available for use in other cells, or for direct empirical prediction and explanation. So long as the commitment imported into each cell are consistent with the isolated commitments within them, logical catastrophe can be avoided.

The fact that the isolated commitments were applied in separate concrete contexts guides the import and export of general commitments: When a generally acceptable figure for a physical quantity is calculated in one context using some isolated commitments, the figure can then be imported into other isolated contexts for further calculations. For instance, the energy levels for a one-electron atom were calculated using classical mechanics together with the quantum restriction on angular momentum. The resulting energy differences between stationary states determined the frequency of light emitted in a transition from a higher to a lower energy state through Planck's relation, E=hv. Finally, in order to describe the light and explain its interaction with instruments this frequency is read into a classical electrodynamic model of the light. Bohr's approach to the theory involved giving rules for which principles belonged where amongst these isolated commitments, and which quantities are "exportable" into the region of general commitments where they can be imported as needed into the various isolated contexts.

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This schematic view of "how to work with OQT" is helpful, but incomplete. Merely demanding exported results be consistent with whatever appears in the isolated contexts is not enough of a constraint on what can be exported: we cannot model Bohr's commitments as closed under the rule "Any result in any isolated context that is consistent with all other isolated contexts can be exported to whichever other isolated context you like." For example, consider a case in which there are just two isolated contexts, T_1 and T_2, T_1 includes A, and T_2 includes $\neg$A. Using this rule we could then infer $(A \vee B)$ from p in T_1, for an arbitrary B consistent with T_2, export $(A \vee B)$ into the general commitment store and then import it into T_2. We could then conclude by disjunctive syllogism that B. We won't get any contradictions this way, but we will be able to get some thoroughly arbitrary results: If $\neg$B is also consistent with T_2, we could have gotten it instead, in just the same way.

So although this schematic account of how Bohr reasoned with OQT seems to be a reasonable sketch of his commitments and how he applied them, it's not any more than that. We cannot propose this sketch as a model of cognitive commitment to OQT. We do need some sort of consequence relation- without one the idea of commitment is empty. But it must not trivialize, as the classical one does, and it must not allow arbitrary results either, as this sketch does.

The same point can be made within a parallel semantic account of contextual commitment. On this account, we would be committed to models of various components of a quantum system and to selecting the combination of models used to describe a particular quantum system according to the import/export rules- but, again, the import-export rules must be more restricted than they would be if they merely demand the existence of a model of each component be preserved by whatever export/import takes place.

7. Paraconsistent Logic

When we have an inconsistent set of commitments to reason with, classical logic tells us to find another set of commitments- we are (seriously) over-committed, and must eliminate some of what we're committed to before classical reasoning can resume. But it's not easy, in general, to decide what to give up. When giving up any of the inconsistent commitments we hold seems too costly, we have an alternative: We can adopt a paraconsistent logic. Paraconsistent logics provide damage control when we can't see any acceptable way to eliminate inconsistency from our commitments. They offer the hope of modeling a serious form of cognitive commitment that tolerates inconsistency. We can regard scientists as cognitively committed to an inconsistent set of claims, closed under a paraconsistent consequence relation which does not trivialize them.

I want to use paraconsistent logic to characterize commitment to OQT: The job of the logic is to provide a link between explicit commitments and the implicit commitments that go with them by providing an appropriate closure relation on the set of principles accepted by Bohr to replace the trivial closure relation we get from classical logic. This modest proposal regarding closure, allows us to take Bohr's commitment to these inconsistent principles seriously and at face value, without regarding him as committed, implicitly, to anything and everything. The result is an account of commitment that makes it contextual, while also making clear why the sorts of trivialization that threatened our sketch of how OQT worked are not a problem.

Non-adjunctive paraconsistent logics prevent the classical "explosion" of inconsistent premise sets by limiting the degree to which premises may be aggregated in conjunctions- a feature which is at least superficially reminiscent of the contextual restrictions Bohr imposes on the application of his contrary principles. The particular logic I want to apply was developed by P.K. Schotch and R.E. Jennings. Unlike other non-adjunctive logics (Rescher and Brandom, 1980) Schotch and Jennings' logic does allow inconsistent premise sets to have consequences which are not consequences of any individual member of the premise set. Thus Schotch and Jennings do not give up all aggregation of premises- they give up only enough to ensure preservation of a generalized "consistency" property they call the premise set's "degree of incoherence."

8. Forcing

Here is a quick sketch of Schotch and Jennings' consequence relation. To begin, we need the notion of a "degree of incoherence"- a generalization of the classical notion of consistency. CON(G, x) holds for a set of sentences G and a whole number x iff there is a family of sets, $A=\{\varnothing,a_1,...a_i\}$ $i \leq x$, such that $a_1...a_i$ are all classically consistent and for every member of G, γ, there is an $a \in A$ such that $a \vdash \gamma$. i(G), G's "degree of incoherence" is the minimum x such that CON(G,x), if there is such a minimum, and ∞ otherwise. The degree of incoherence of a classically consistent set is either 0 (if the set includes only tautologies) or 1; all and only sets including a contradiction have degree .

Rather than preserve consistency, as the classical consequence relation does, forcing will preserve degrees of incoherence. Thus (among other things) no set not already containing a contradiction will have a contradiction in its closure under forcing. A sentence S is a degree-of-incoherence preserving consequence of a set G such that i(G)=k if and only if every family of sets A, of "width" k, which "covers" G as described above, includes a member that has S as a classical consequence.

Forcing is a proof relation corresponding to this degree-of-incoherence preserving consequence (DPC) relation. It is clearly non-adjunctive: Even if $\{A,B\} \subseteq G$, G may not force $(A \wedge B)$. But forcing still preserves some aggregation of premises- aggregation grows weaker and weaker as a set's degree of incoherence rises, but so long as a set's level is finite it can force sentences that are not consequences of any individual sentence in the set. Given a level of 2, a set closed under forcing will include the disjunction of the pairwise conjunctions of all triples in the set; given a level of 3, the set will include the disjunction of the pairwise conjunctions amongst all quadruples, and so on. These disjunctions of pairwise conjunctions fully capture the aggregation of premises that DPC involves: Closure under the classical consequences of singleton subsets together with the rule 2/n+1, which allows us to infer from any n+1 formulae the disjunction of all their pairwise conjunctions, is consistent and complete with respect to DPC.(Schotch and Jennings 1989, pp. 312-319, Apostoli and Brown, forthcoming)

There is an important methodological objection that paraconsistent logicians must face: Reductio arguments, which show the incompatibility of a sentence S with our other commitments by assuming S and then (drawing on our other commitments) deriving a contradiction, are extremely important methodologically. Any logic which weakens reductio (as a paraconsistent logic must) threatens to deprive us of a very useful and important logical tool. Schotch and Jennings' system, however, provides the basis for a neat and tidy account of reductio arguments, which allows reductios even when our other commitments are already inconsistent. We will say that a sentence S is absurd with respect to a set of commitments G if and only if $i(G \cup \{S\}) > i(G)$.

This has a number of attractive consequences: A contradiction is absurd with respect to every set that does not include a contradiction, and any sentence whose addition to a consistent set gives an inconsistent set is absurd with respect to that consis-

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tent set. So when our starting set is consistent, we have exactly the classical notion of absurdity. But when the starting set is inconsistent (and every sentence is, on the classical account, trivially absurd with respect to that set) this definition continues to make distinctions. Sentences whose addition to G preserves G's degree of incoherence are not absurd with respect to G, while those whose addition to G increases G's degree of incoherence are. This provides some insight into what remains of reductio-style arguments when inconsistency is in the air: When a sentence S is absurd with respect to a set of commitments G, we can add S to our commitments only at the cost of either rejecting some commitments in G or accepting an increase in our commitments' degree of incoherence. Evidently, these costs can be quite high.

9. Other systems of Paraconsistent Logic

There are (depending on how you count them) quite a few alternative systems of paraconsistent logic. However, they all take a very different approach than Schotch and Jennings. Rather than agree with classical logic on the semantics and search for something more to have the logic preserve, they re-write classical semantics to create non-trivial valuations assigning designated values to all the members of some classically inconsistent sets of sentences. Here, for reasons of space, I'll consider only the system of paraconsistent logic developed by Nuel Belnap in "How a Computer Should Think" (1977). Belnap proposes a four-valued system, including the values "told true", "told false", "told both" and "told neither". The logic that results is very close to classical: For example, the computer regards itself as told at least true that $(A \wedge B)$ whenever it is told at least true that A and told at least true that B. But there is a separate clause for "told at least false"- the computer regards itself as told at least false that $(A \wedge B)$ whenever it is told at least false that A or told at least false that B. As a result of the independence of the clauses for "told at least true" and "told at least false", the computer will not "spread" an inconsistency in what it has been told to all sentences in the language.

However, there is something a little odd here. The computer will regard itself as "told both" regarding $(A \wedge \neg A)$ if it has received both A and $\neg A$ as input. Similarly, it will regard itself as "told both" regarding $(A \vee \neg A)$ if it has received both as input. This seems a little bold: After all, if the computer has two sources it seems perfectly possible that, though they disagree on A, they still agree on classical logic. Their disagreement over A is compatible with agreement on tautologies and contradictions. A more conservative computer would regard itself as told (just) false that $(A \wedge \neg A)$, and told (just) true that $(A \vee \neg A)$. Schotch and Jenning's non-adjunctive logic agrees with this intuition concerning inconsistent input: If a set of claims including A, and $\neg A$ is closed under forcing, $(A \vee \neg A)$ is forced but $(A \wedge \neg A)$ is not. A computer using forcing would not regard itself as having been told the conjunction was true (or the disjunction false) in such a case. This seems the right answer for a conservative inference engine to give.

Priest and Sylvan have objected to this, claiming that the computer has indeed been told the conjunction is true- *by implication* . (Priest and Sylvan 1989, pp. 158f) But a satisfactory account of implication is exactly what's at issue here. Moreover, Priest and Sylvan's argument turns on the claim that conjunction just is the connective which gives a truth when the two things it joins are true. Hence when adjunction fails as a rule of inference, this is weighty evidence that the connective it fails for isn't conjunction. But this objection makes an important assumption: That truth-preservation is sufficient for validity. If validity requires more than just truth preservation, the fact that $\{A,B\} \vdash_1 (A \wedge B)$ does not show the logic l has a non-standard truth condition for "$\wedge$". And this is precisely the case for Schotch and Jennings: DPC requires preservation of degree as well as truth. Adjunction *is* truth preserving- it fails to be a rule of their system only because it fails to preserve degree.

10. Forcing and OQT

An interesting pragmatic element in inference emerges for forcing. Some individual conjunction-introductions are guaranteed to preserve a set's degree of incoherence, but allowing conjunction-introduction in general leads to explosion. We must *decide* which conjunctions to adopt (if any). Our reasons for choosing some rather than others will normally derive from our epistemic aims. These usually make some conjunctions indispensable; but they also usually leave us also with a wide field of potentially interesting or desirable conjunctions whose value remains to be determined. On the question of which non-level increasing conjunctions to adopt and which to avoid, the logic is silent. Adding them to our commitments is just like extending any classical theory by adding further sentences consistent with, but not implied by, the theory. Which non-level increasing conjunctions we will accept depends on which are valuable- which seem required for effective application of the theory, which promise to produce interesting predictions without absurdity, and so on.

This feature of forcing suits old quantum theory very nicely. As we've seen, the development of OQT involved a gradual clarification of which classical results could be applied in the quantum domain, and when. This can be read as a sorting out of which classical results could be conjoined with quantum principles to good effect, choosing (degree-of-incoherence preserving) conjunctions from among the many candidates. Thus one prediction the DPC model makes concerning OQT is confirmed by the history of OQT: The addition of conjunctions of classical principles with quantum principles to the theory is regarded as ampliative, requiring independent theoretical and/or empirical justification. Conjunction introduction is not a trivial inference, but a substantial step with both risks and potential rewards. According to the forcing model, applications of old quantum theory (OQT) will only conjoin principles whose conjunction does not increase the theory's degree of incoherence. But, as is common in other areas (Hacking 1983, pp. 72-73) results derived using one subset of the principles are used as input for other, incompatible subsets when this can be done without increasing the theory's degree of incoherence.

Moreover, as I've already pointed out, late in the development of OQT, Wolfgang Pauli showed that an adiabatic transformation of a hydrogen atom in crossed electrical and magnetic fields would lead from allowed states of the system to disallowed states. If we model commitment to OQT in the way I propose, the importance of this result is clear: it is a proof that OQT, as it stood at that time, had a degree of incoherence higher than 2. This is bad for OQT, since the division of contexts which kept contrary claims apart was based on the assumption that the degree of incoherence was only 2. The only way OQT could carry on after Pauli's proof was by re-working the division of contexts to fit degree of incoherence 3 (or whatever higher degree of incoherence the theory might be judged to have) or by giving up one or another of the principles involved in Pauli's demonstration in hopes of reducing OQT's degree of incoherence to 2. Neither of these options was very attractive (especially in view of OQT's other difficulties- for example, its difficulties with the anomalous Zeeman effect and its inability to deal with multi-electron systems). Given our earlier paraconsistent account of reductio, one can treat this as a reductio of the description of the atom as existing in such crossed fields, relative to the commitments of OQT. Since the possibility of placing a hydrogen atom in such fields was not in doubt, the reductio told against the commitments rather than against the description.

My reasons for taking this inconsistent body of theory seriously parallel the reasons van Fraassen offers for taking theories seriously despite his empiricist stance towards them. For van Fraassen, an accepted theory is the means by which we specify the empirical substructures into which the empiricist believes she can fit the observations. While the empiricist doesn't believe in the theory, she needs the theory to say what it is she does believe, especially in new circumstances to which she has not yet applied the theory (van Fraassen 1980, pp. 41-69). Similarly it's this non-adjunctive commitment to apply these inconsistent principles that determines what those who accept OQT in this way believe, not just in familiar and well-studied circumstances (where one might get away with just listing the beliefs), but in new circumstances (where appeal to the principles is indispensable).

The paraconsistent approach I propose allows us to account, in broad, for how new applications of the inconsistent theory were developed: When previously accepted techniques were not sufficient, the theory was extended by drawing on related classical results, care always being taken to avoid bringing in anything that would raise the theory's degree of incoherence. The results of such an extension could always be read, in hindsight, as a mere analogy between the (as yet incomplete) quantum theory and the (superseded) classical theory. But without some sort of commitment to classical physics, the practice of continually drawing on it for such analogies seems odd at best- and it gives short shrift to Bohr's early hopes for a complete patchwork theory, not to mention his later (somewhat mysterious) appeals to classical physics in his account of quantum measurements.

11. Conclusion

This is clearly just the beginning of the story. The history will need to be examined at a more detailed level; and the model applied to it together with much more refined views of confirmation, extension, and falsification. But I think the history at this level confirms the initial impression of Bohr's contextual restrictions on the application of physical principles: This paraconsistent approach to OQT is a promising avenue for further attempts to make sense of this fascinating phase in the development of modern physics.

More broadly, I think this non-adjunctive model of commitment promises to capture some features of our cognitive commitments which are otherwise rather puzzling. Scientists show a combination of confidence and diffidence about their theories which ordinary models of cognitive commitment don't give a very good account of. On one hand, they are extremely confident about well-understood applications of their theories. On the other hand, they are reluctant to commit themselves regarding new applications, or applications outside the range of parameters already explored. These facts have provided comfort to various sorts of anti-realist: If we think of our commitment to a principle as involving a belief in it, or even the assigning to it of some probability, our commitment should not be sensitive to context in the way these commitments are.

But if commitment to a set of claims doesn't necessarily involve commitment to their conjunction, then the fact that we are committed to the principles of a theory, and are willing to conjoin them with the facts in some circumstances in order to reach conclusions about those circumstances does not commit us to conjoin them willy-nilly with the facts in other circumstances and accept the consequences there as well.

In conclusion, a lot of work remains to be done. But I am optimistic- I think the results will bear out the value of this sort of re-thinking of the nature of our various epistemic commitments. From a certain point of view, it may seem that this work only makes things worse: If we have trouble choosing amongst the models of cognitive commitment already available, inventing a new class of models may only make the choice more difficult. But I think there is something to be gained here. This non-adjunctive, context-restricted form of commitment shows that there is a wide range of models of cognitive commitment yet to be explored, which combine substantial formal interest with pragmatic features familiar from scientific practice.

Note

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References

Apostoli, P. and Brown, B., "A Solution to the Completeness Problem for ParaNormal Modal Logic", unpublished ms.

Belnap, (1977), "How a Computer Should Think" in G.Ryle (ed.), *Contemporary Aspects of Philosophy*, Boston: Oriel Press, pp. 30-56.

Bohr, N. (1913), "On the Constitution of Atoms and Molecules", *Philosophical Magazine*, (6) 26: 1-25.

____. (1922), *The Theory of Spectra and Atomic Constitution*. Cambridge: Cambridge University Press.

____, Kramers, H.A., and Slater, J.C. (1924), "The Quantum Theory of Radiation", *Philosophical Magazine*, (6) 47: 785-802. Reprinted in van der Waerden (1967), pp. 159-176.

Einstein, A. and de Haas, W.J. (1915), "Experimenteller Nachweis der Ampèreschen Molekularströme, *Verhandlugen der Deutschen Physikalischen Gesellschaft* (2) 17: 152-70.

Ehrenfest, P. (1917), "Adiabatic Invariants and the Theory of Quanta", *Philosophical Magazine*, 33: 500-513, reprinted in van der Waerden (1968), pp. 79-93.

Hacking, I. (1983), *Representing and Intervening*, Cambridge: Cambridge University Press.

Kronig, R., and Weisskopf, eds. (1964), *Collected Scientific Papers*. New York-London-Sydney: Interscience Publishers, pp. 269-548.

Pauli, W. (1926), "Quantentheorie", in Geiger, H., and Scheel, K., *Handbuch der Physik*, Vol. 23, Part 1, pp. 1-278; reprinted in Kronig, R., and Weisskopf, eds. (1964).

Priest, G. (1988), *Beyond Consistency*. Munchen, Hamden, Wien: Philosophia Verlag.

Priest, G. (1988), *Beyond Consistency.* Munchen, Hamden, Wien: Philosophia Verlag.

Priest, Routley and Norman (eds.) (1989), *Paraconsistent Logic: Essays on the Inconsistent*, Munchen, Hamden, Wien: Philosophia Verlag.

Rescher, N. and Brandom, R. (1980), *The Logic of Inconsistency*, Oxford: Basil Blackwell.

Rosenfeld and Hoyer, eds. (1981), *Niels Bohr: Collected Works 2*. Amsterdam :North Holland Publishing Co.

Rozental, S. (ed.), (1967), *Neils Bohr.* New York, John Wiley and Sons.

Schotch, P.K. and Jennings, R.E. (1980), "Inference and Necessity", *Journal of Philosophical Logic*, 9: 327-340.

Schotch, P.K. and Jennings, R.E. (1989), "On Detonating", in Priest, Routley and Norman (eds.) pp. 306-327.

van der Waerden, B.L. (ed.) (1968), *Sources of Quantum Mechanics*, New York: Dover.

van Fraassen, B.C. (1980), *The Scientific Image*. Oxford: Claredon.

van Vleck, J.H., (1924), "The absorption of radiation by multiply periodic orbits, Part I: Some extensions of the Correspondence Principle" *Physical Review* 24: 330-365, reprinted in van der Waerden (1968), pp. 203-222.