Muller's Critique of the Argument for Aim-Oriented Empiricism

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Abstract For over thirty years I have argued that we need to construe science as accepting a metaphysical proposition concerning the comprehensibility of the universe. In a recent paper, Fred Muller criticizes this argument, and its implication that Bas van Fraassen's constructive empiricism is untenable. In the present paper I argue that Muller's criticisms are not valid. The issue is of some importance, for my argument that science accepts a metaphysical proposition is the first step in a broader argument intended to demonstrate that we need to bring about a revolution in science, and ultimately in academic inquiry as a whole so that the basic aim becomes wisdom and not just knowledge.

Keywords Standard Empiricism - Constructive Empiricism - Aim-Oriented Empiricism - Metaphysics - *ad hoc* theories - Scientific Realism - F. A. Muller - B. C. van Fraassen

1 Standard Empiricism and Its Refutation

Some philosophers try to write in as simple and lucid a way as possible. Others do the opposite. I belong to the former category: I work very hard at trying to formulate what I have to say as simply as I can. Fred Muller belongs to the latter category. That, at least, is the impression one gains from a recent paper of his published in this journal: see Muller (2008). The paper criticizes some of my work, and sets out to defend Bas van Fraassen's constructive empiricism against objections I have made to the view. But Muller, in his fierce determination to pin everything down with absolute rigour, has produced a paper of almost unintelligible intricacy. Worse, his account of my views is seriously defective, and his criticisms do not succeed. Muller's striving for logical precision has resulted in a weird splintering of my views: the fragments are there, but as put together by Muller they seriously misrepresent and distort my actual views and arguments.[1]

What is at issue is of some importance because the argument that Muller criticizes is the first step in a much broader argument that I have developed during the past thirty years intended to show that we urgently need to bring about a revolution in science, and in academic inquiry as a whole so that the basic aim becomes wisdom and not just knowledge.[2]

The nub of what is at issue has to do with a view of science that I call *standard empiricism* (SE). In a paper in this journal, I formulated SE as follows:

> In science, ideally, all claims to knowledge are to be assessed impartially with respect to the evidence, the simplicity, unity or explanatory power of theories being taken into account as well, *no thesis about the world being upheld permanently as a part of knowledge independently of evidence, let alone in violation of evidence*. Most, if not all, versions of SE stress that questions of simplicity, unity and explanatory power play a valid, important role in influencing choice of theory in science, in addition to considerations of empirical success … The decisive point

> that all versions of SE agree on is that no substantial thesis about the nature of the universe can be upheld as a part of scientific knowledge independently of empirical considerations, and certainly not in violation of empirical considerations. In so far as theory choice is biased in the direction of simplicity, unity or explanatoriness, this bias must not commit science to making the permanent assumption that nature herself is simple, unified or explainable.[3]

And I went on to point out: "This rather thin thesis is common ground for logical positivism, inductivism, logical empiricism, hypothetico-deductivism, falsificationism, conventionalism, constructive empiricism, pragmatism, realism, induction-to-the-best-explanationism, and the views of Kuhn and Lakatos".

Muller indicates, correctly, that expositions and criticisms of SE are to be found in many places in my writings, and yet Muller falls at this first hurdle. He fails lamentably to reproduce SE as specified above. Right at the outset, Muller declares that I hold that SE asserts "the decision to accept or reject a scientific theory is based *exclusively on the available evidence*"(my italics). Wrong. As I make quite clear (see above), "questions of simplicity, unity and explanatory power play a valid, important role in influencing choice of theory …in addition to considerations of empirical success". Even here, in connection with this thesis that is the kingpin of the whole discussion, Muller seriously misrepresents what I have to say. This initial misrepresentation renders invalid much of Muller's subsequent discussion. Further cases of such gross misrepresentation will emerge as we proceed.[4]

As it happens, towards the end of his paper Muller asserts that I have "lately" reformulated SE to incorporate the idea that simplicity is important in addition to empirical success.[5] But there is nothing recent about this way of formulating SE. It goes back to my very first discussion of SE, published in 1974, where SE was formulated so as to include simplicity.[6]

Muller goes on to expound an argument he calls "Maxwell's Master Argument". The phrase is even in the title of Muller's paper. But as set out by Muller, I don't recognize the argument as something to be found in my writings (although components of the argument are to be found there). Instead, what I would emphasize is the following refutation of SE – only the bare bones of which I reproduce here:[7]

> Given any scientific theory, however well verified empirically, there will always be infinitely many rival theories which fit the available evidence just as well, but which make different predictions, in an arbitrary way, for phenomena not yet observed … [these] infinitely many rivals to accepted physical theories are rejected out of hand, not on empirical grounds, but because they are grotesquely *ad hoc*, grotesquely lacking in simplicity, unity, explanatory power … now comes the decisive point. In persistently rejecting infinitely many such empirically successful but grotesquely *ad hoc* theories, science in effect makes a big permanent assumption about the nature of the universe, to the effect that it is such that no grotesquely *ad hoc* theory is true, however empirically successful it may appear to be for a time.[8]

Muller accepts that this argument is valid – or, at least, he accepts that I have "made it plausible that science permanently *pragmatically accepts* … that the universe is *comprehensible*, i.e. such that it makes every aberrant theory false": Muller (2008, p. 155). This concession is massive. Muller in effect acknowledges that my refutation of SE succeeds. All that Muller can do is snipe at various theses he – mostly incorrectly – attributes to me, associated with the above successful refutation of SE.

Muller has two main criticisms, which I discuss in turn in the next two sections.

2 Muller's First Criticism

Muller develops his first criticism by formulating a series of propositions about aims, methods, expectations, and assumptions about the universe. There are in all eleven of these propositions. Some are propositions I uphold or argue for. Others are propositions that play no part in my refutation of SE, not even when that refutation is spelled out in much more detail. Still others are propositions which seem to me dubious indeed, and are most certainly not propositions which play a role in my refutation of SE. As far as most of these propositions are concerned, Muller makes no attempt to establish that I accept them, by means of quotations or references to my writings. From this mixed bag of eleven propositions, Muller then concocts a quasi-formal argument of some intricacy, which he declares reveals the "logical structure" of my refutation of SE outlined above.[9] This concocted "master" argument is then declared to be invalid: see Muller (2008, p. 143).

This is a trick that anyone can play. Take a simple, clear and valid – if informal – argument. Formulate a considerable number of theses vaguely associated with the subject matter of the argument, and refer to them by various abbreviations. From these elements then concoct a quasi-formal argument, assert that it exhibits the "logical structure" of the original argument, and claim that the concocted argument is invalid.

This is not a rational way to argue. *Of course*, if one is free to reformulate an argument in any way one pleases, one can turn a valid argument into an invalid one. Dressing up the reformulated argument in logical garb does not enhance the rationality, the rigour, of such a way of proceeding. What needs to be criticized is *the original argument*, not something that has been put in its place.

Given the basic irrationality of Muller's procedure, his detailed argument scarcely deserves serious criticism. I am nevertheless obliged to say something about it, if only to substantiate my point that its premises include propositions that play no role whatsoever in my refutation of SE, as outlined above in section I. In what follows, I restrict my attention, quite properly, to the *premises* of Muller's "master" argument. There are six premises and, quite extraordinarily, none figure in *my* argument refuting SE, not even when that argument is spelled out in greater detail than the summary form I give it in section I above. Here, then, are Muller's six premises, and my comments, in turn.

1. **Acc (6)** "If someone follows method M to reach aim A, and expects that following M will help him considerably in reaching aim A, then he accepts the concomitant methodological assumption U[M,A], according to which the universe is such that following method M is of considerable help in reaching A": Muller (2008, p. 137).

Comment: This proposition, crucially, refers to what a person "expects" will "help him" in reaching an aim. But my argument refuting SE does not at any point refer to such subjective or psychological notions. It refers only to what is public and objective. It

is couched exclusively in terms of methodological rules governing acceptance and rejection of theories in the light of evidence, the aim of the methodology of acquiring knowledge of factual truth (insofar as this is achievable), acts of acceptance and rejection of theories, and factual propositions explicitly or implicitly accepted as a result of the acceptance and implementation of the methodology. Nothing like Muller's Acc (6) appears anywhere in my work spelling out my argument (see note 7 for references) and, in appealing to the psychological notion of "expectation" it violates the whole character of the argument.

What my argument does appeal to is a special case of the following proposition: If, given the aim of acquiring knowledge of truth, a methodology demands rejection of all theories that imply a factual proposition F, even when those theories are empirically more successful than accepted theories, then "not F" is implicitly accepted as true. It is interesting that this proposition, which does perhaps deserve discussion, is ignored by Muller, and does not figure anywhere among the premises of his "master" argument.

2. **Exp (9)** "If someone always follows method M, and never goes against M although nothing prevents him form doing so, then he has higher expectation to be successful when following M than when going against M": Muller (2008, p. 138).

Comment: As for Acc (6) above.

3. **Ab (2)** "Science rejects all aberrant theories and accepts only regular theories": Muller (2008, p. 136).[10]

Comment: This premise of Muller's "master" argument is not a premise of my argument. It is a thesis I argue for at some length, by considering the character of the theories physicists do accept, and by considering empirically more successful aberrant theories that are not considered for a moment.[11] And in any case, in the form given it by Muller, it is unacceptably strong. Orthodox quantum theory (OQT) is (or was) an accepted physical theory, and yet – I have argued at length – it is seriously *aberrant*, in that it consists of two mutually incompatible parts, the quantum part (Schrödinger's equation), and some part of classical physics applicable to the measuring instrument.[12] So seriously have I taken this aberrant character of OQT that, over the years, I have even developed a fully micro realistic, fundamentally probabilistic version of quantum theory, free of the aberrance of OQT, which solves the quantum wave/particle problem and is testably distinct from OQT.[13] I should add that *aim-oriented empiricism* – the view of science I defend to replace the untenable SE – implies that aberrance, or disunity, is a matter of degree (see note 16). According to aim-oriented empiricism, physics should accept theories which decrease the overall aberrance or disunity of the totality of accepted fundamental physical theory as much as possible (in addition to satisfying empirical considerations, of course).[14]

4. **SignSubst (13)** "If accepting or rejecting a thesis makes a significant difference in how fast science progresses, then the thesis is substantial": Muller (2008, p. 140).

Comment: This premise of Muller's argument does not figure as a premise in my argument, and nor is it required for the argument.[15]

5. **SignU (14)** "Accepting or rejecting thesis U (3) [whether the universe is comprehensible or not] makes a significant difference in how fast science progresses": Muller (2008, p. 140).

Comment: As for SignSubst (13) above.

6. **Meta (17)**: Muller (2008, p. 141). **Comment**: This concerns metaphysics. It does not figure as a premise in my argument either, and nor is it required for my argument. I discuss it, and what Muller has to say about metaphysics, in a moment.

I conclude from this examination of the six premises of Muller's "master" argument that it really does have nothing to do with any argument I have formulated to refute SE. That Muller's "master" argument is invalid does not in any way whatsoever call into question the validity of my quite different argument.

Can a rational criticism be salvaged from the debris of what may be termed Muller's "irrational logical reconstructivism"? On his way to his "master" argument, his first "logical reconstruction", Muller does put forward one or two criticisms that deserve consideration.

One is that my refutation of SE succeeds in establishing that science accepts, as an item of knowledge, that the universe is such that all grossly *ad hoc* or aberrant theories are false, but fails to establish that this thesis is *metaphysical*. Muller pounces on a definition of "metaphysical" I give in a footnote in my *The Comprehensibility of the Universe* (1998, p. 271), and finds it inadequate. What he ignores is that I give that definition during the course of expounding aim-oriented empiricism (in connection with which it is especially relevant). In the rather different context of refuting SE, and seeking to establish that science makes a permanent assumption about the world *that is metaphysical*, of course I do not use "metaphysical" in some special sense. To do so would be somewhat disreputable; and it would threaten to trivialize the result. As is always clear from the context, I argue that science makes a metaphysical assumption, using "metaphysical" in exactly the standard way it has come to be understood in philosophy of science, after Karl Popper. A proposition is metaphysical if it is empirically unfalsifiable. Muller's criticisms at this point are simply *irrelevant*; they stem from a wilful misinterpretation of what it is I assert and argue for.

A further oddity of Muller's criticism at this point is that he simply *ignores* the argument I have put forward for holding that the proposition in question is metaphysical (in the relevant sense of "metaphysical"). He knows this argument well, because I first formulated it during a lively debate with Muller, and he found no fault with it at the time, as I explained when I published it: see my (2004, pp. 153-4).

Here is the argument. The proposition in question is that the universe is such that no seriously *ad hoc* or aberrant physical theory is true.[16] Muller accepts that there are infinitely many such *ad hoc* theories. We may take the proposition to assert:

(A) Not T_1 and not T_2 and …. not T_∞ , where each T_n is a falsifiable but as yet unfalsified, grossly *ad hoc* physical theory.

Is (A) empirically falsifiable? No, because in order to falsify (A) one would have to *verify* one or other of T_1, T_2, … T_∞ , which cannot be done because physical theories cannot be verified. Is (A) empirically verifiable? No, because in order to verify (A) one would have to falsify all of T_1, T_2, …and T_∞ , and this cannot be done because there are infinitely many theories here to be falsified. Hence, since (A) is neither falsifiable nor verifiable, it is metaphysical. (Popper only requires, of course, that (A) is unfalsifiable.)

This argument, well known to Muller, is ignored in his paper. Instead he argues that we might come to reject (A) because a series of characteristically severely *ad hoc* theories

are successively corroborated empirically, the search for simple, unified, non-*ad hoc* theories meeting with no empirical success whatsoever. Perhaps. But to acknowledge this is not equivalent to acknowledging that (A) is empirically falsifiable. Rejecting (A) because it clashes with a succession of empirically successful theories, T_1, T_2, … T_n (which is what Muller appeals to), is not the same as rejecting (A) because it clashes with an observational or experimental result (which is what is involved when a theory is falsified empirically). Muller's criticism here, as elsewhere, fails.

I might perhaps add that, absolutely fundamental to my work in this field, is the basic point that science *needs* to make explicit metaphysical assumptions implicit in the persistent ignoring of empirically successful, severely *ad hoc* theories, just because these assumptions are profoundly *influential*, and profoundly *problematic*. The assumptions need to be made explicit within science so that they can be critically assessed, so that alternatives can be developed and assessed, in the hope that such assumptions can be *improved*. Aim-oriented empiricism, the conception of science I argue for to replace SE, provides science with a meta-methodology designed to facilitate *improvement* of metaphysical assumptions in the light of their empirical "fruitfulness", and other considerations. Muller, in arguing that (A) might be revised in the light of the empirical success and failure of a succession of theories (i.e. a research programme), is employing an argument I have myself developed during the course of expounding and defending aim-oriented empiricism.[17]

Muller also criticizes me for claiming that without (A) science would not be possible. Eschewing even implicit allegiance to (A), science might persistently accept the most empirically successful theories available even though they are severely *ad hoc*: such a science, Muller argues, is possible, and might even make progress. I agree that it is possible, but it would not make progress at the theoretical level – especially if, as I assumed, it put Popperian methods into practice, which favour theories that are as falsifiable as possible: See Popper (1959). What Muller has in mind – science without (A) – would be a weird parody of science as we know it, and devoid of theoretical discoveries that are comparable to the great unifying, explanatory theories made by physics so far.

Muller criticizes me, also, for accepting Acc(9) – one of the premises in *his* reconstructed argument: see point 1 above. Acc(9) is implausible, Muller says, because it "smacks too much of an Inference-to-the-Best-Explanation (IBE), which is a mode of inference that Van Fraassen is very critical about": Muller (2008, p. 143). Not only am I also just as critical as van Fraassen of IBE; Acc(9) is, once again, as I have explained above, not to be found in my publications, and is not required, explicitly or implicitly, for my argument refuting SE.[18] Once again, Muller criticizes misrepresentations of what I have said, not what I have actually said.

3 Muller's Second Criticism

Muller's second criticism of my refutation of SE follows the same pattern as his first. Instead of criticizing my arguments as I have formulated them, Muller hits upon the irrational "logical reconstructivist" strategy of concocting a quasi-formal argument – Muller's handiwork, not mine – which he then attributes to me. This time Muller declares the argument to be valid but rejects one of his premises, namely:

Neg (30) "Not accepting a proposition implies accepting its negation": Muller (2008, p. 146).

We see once again the absurdity of Muller's methodology. Not for one moment would I accept Neg (30). Nor is it implicitly assumed anywhere in any of my writings. Nor does Muller provide any evidence whatsoever for holding that it is – over and above the grounds that this proposition is needed for *his* quasi-formal argument, an argument not to be found anywhere in my publications. Neg (30) deserves to be rejected, of course, because it leaves no room for suspended judgment. One may not accept a proposition because one holds there are insufficient grounds to accept it; this is not the same as accepting its negation.

I conclude that Muller's criticisms of my refutation of SE are invalid.

4 First Argument Refuting Constructive Empiricism

I turn now to the question of whether Bas van Fraassen's constructive empiricism (CE) is a version of standard empiricism (SE), and thus whether my refutation of SE is also a refutation of CE.

As I see it, the situation is quite clear. My refutation of SE applies straightforwardly to CE. CE is untenable. I have two arguments in support of this contention.

First, a word about what we ought to mean when we say a physical theory is accepted as a part of scientific knowledge. As I have argued elsewhere, to say this is not to say that it is known that the theory is true, or empirically adequate, or is believed to be empirically adequate.[19] Most physical theories, accepted as a part of scientific knowledge (associated with Kepler, Galileo, Newton, Maxwell, Einstein and others) have turned out to be false (empirically inadequate). Indeed, according to aim-oriented empiricism, the view of science I argue for, *all* dynamical physical theories that have a restricted scope (and are not about all phenomena) are false (empirically inadequate).[20] We may interpret "T is accepted as a part of scientific knowledge" in such a way that, so to accept T commits us to the rather modest:

(B) Even though T may be false, it is nevertheless the best available testable account of the phenomena (observable and unobservable) to which it applies; and furthermore, T yields true empirical predictions in standard regions of application, to standard degrees of accuracy, in a way that is sufficiently reliable and trustworthy to form a basis for action.

What emerges from my refutation of SE (summarized in section I above, but spelled out in more detail elsewhere[21]) is that, in order to be accepted as a part of scientific knowledge, in the sense of (B), a physical theory must satisfy two conditions. It must (i) meet with sufficient empirical success, and must (ii) be compatible with the metaphysical (unfalsifiable) thesis that the universe is such that no precise, grossly *ad hoc* physical theory is true.[22] Both conditions are essential, even in standard contexts of practical applications of physical theory. We are, let us suppose, designing and building a bridge, and we employ accepted physical theory, T, predicting such matters as the strength of steel to determine safe dimensions and design of the bridge. Any number of rivals to T can be concocted, *empirically more successful* than T, which make predictions about the bridge (that it will collapse, for example) quite different from those of T. These are

ignored because even though they satisfy (i) better than T, they all clash with (ii). These rivals to T are all grossly *ad hoc*. What this brings out with extreme clarity, then, is that condition (ii) is just as much concerned with the assessment of factual knowledge or truth in science as condition (i) is.

But this reveals at once that van Fraassen's CE is untenable. For condition (ii), vitally concerned with assessment of knowledge and truth, is of course a requirement of non-*ad hocness*, explanatory power or simplicity. And van Fraassen is adamant: simplicity and explanatory power play a role in the selection of theories in science but have no role whatsoever in deciding questions of knowledge or truth. Thus he declares of virtues of theories such as simplicity or explanatory power:

> In so far as they go beyond consistency, empirical adequacy, and empirical strength, they do not concern the relation between the theory and the world, but rather the use and usefulness of the theory; they provide reasons to prefer the theory independently of questions of truth.[23]

Wrong. Condition (ii) is as vital as questions of consistency, empirical adequacy and strength in assessing the relation between the theory and the world; it is vitally concerned with assessment of truth. And condition (ii) has to do with simplicity, with the explanatory character of a theory. If we restricted attention to consistency, empirical adequacy and strength (and ignored simplicity) we would persistently accept as knowledge of factual truth *the wrong kind of theories*, theories, indeed, which would be quite disastrously wrong.

van Fraassen goes on to say:

> To praise a theory for its great explanatory power, is therefore to attribute to it *in part* the merits needed to serve the aim of science. It is not tantamount to attributing to it *special* features which make it more likely to be true, or empirically adequate.[24]

Wrong again. Explanatory power – that is, the capacity to satisfy condition (ii) – plays a vital role in scientific practice in assessing truth, or empirical adequacy.

As if the above is not explicit enough, van Fraassen goes on to say:

> Simplicity is quite an instructive case. It is obviously a criterion in theory choice or at least a term in theory appraisal. For that reason, some writings on the subject of induction suggest that simple theories are more likely to be true. But it is surely absurd to think that the world is more likely to be simple than complicated (unless one has certain metaphysical or theological views not usually accepted as legitimate factors in scientific inference). The point is that the virtue, or patchwork of virtues, indicated by the term is a factor in theory appraisal, but does not indicate *special* features that make a theory more likely to be true (or empirically adequate).[25]

This last quotation is fascinating. It indicates that van Fraassen accepts the central component of my refutation of SE, my argument for aim-oriented empiricism, namely:

(C) Persistent acceptance (in sense (B)) of non-*ad hoc* (simple, explanatory) physical theories, in part because they are non-*ad hoc*, simple or explanatory, would commit one to accepting a *metaphysical* proposition about the world, namely that it is simple or comprehensible.

Just that which Muller devotes so much misplaced energy trying to refute, van Fraassen calmly takes for granted. Where van Fraassen goes wrong is to conclude that simplicity cannot play a valid role in assessing truth in science. In effect, van Fraassen converts my argument into a *reductio ad absurdum*. *If* simplicity played a valid role in assessing truth in science, we would have to see science as accepting the metaphysical proposition that the universe itself is simple. But a metaphysical proposition such as this is not (and perhaps cannot be) a part of scientific knowledge. Hence, simplicity cannot play a valid role in science.

But we need to turn van Fraassen's *reductio ad absurdum* on its head. For I have shown that simplicity (non-*ad hocness* or explanatory power) *does* in scientific practice play a vital role in assessing claims to knowledge of factual truth. This means, as van Fraassen accepts, and as condition (ii) above asserts, that science does accept, as a part of knowledge, that the universe is simple (or comprehensible), even though this is a view "not usually accepted as [a] legitimate factor in scientific inference". Furthermore, and here is the crucial point, precisely because this metaphysical proposition is profoundly influential, profoundly problematic, and "not usually accepted as legitimate" and thus only implicit in scientific practice, it is vital, for scientific rigour and success, that this implicit proposition be made *explicit*, so that it can be critically assessed, so that alternatives can be developed and assessed, in the hope of the currently accepted metaphysical proposition being *improved*. Aim-oriented empiricism, as I have already remarked, emerges as a meta-methodology designed precisely to facilitate this vital theoretical task of improving the implicit metaphysics of physics.

In short, van Fraassen's CE is untenable because it denies that simplicity plays any valid role in science in assessing truth. On the contrary, simplicity plays a vital role in the assessment of truth. Furthermore CE *needs* to be rejected because, if taken seriously, it would undermine scientific rigour. For CE implies that the metaphysical assumption concerning simplicity is not made by science. CE thus obscures the important point that scientific rigour *requires* that this problematic, influential proposition be made explicit so that it can criticized and improved. CE helps undermine scientific rigour – just that which van Fraassen is concerned to defend.

5 Second Argument Refuting Constructive Empiricism

So much for my first argument for the untenability of van Fraassen's CE. I turn now to my second argument.

Consider an accepted physical theory, T, whose basic postulates are about unobservable physical entities. We may take T to be quantum theory plus elements of atomic structure. T is thus, in the first instance, about such unobservable entities as electrons, protons neutrons, nuclei, and photons. Let us assume that T makes a wealth of empirical

predictions about the physical and chemical properties of matter. For scientific realists such as myself, all of T can be interpreted legitimately as being about the physical world, although propositions of T about unobservable physical entities may be judged to be less secure epistemologically than standard empirical predictions of T about empirical phenomena. T is accepted; it thus satisfies conditions (i) and (ii) of section IV.

According to CE, only that part of T that is about *observable phenomena* has factual scientific content: statements of T about unobservable phenomena do not have such content. Granted CE, the theory that has factual scientific content is thus, not T, but T* - where T* is all the consequences of T that are about observable phenomena. Granted CE, then, in considering whether T should be accepted as embodying scientific factual knowledge of truth, what needs to be considered is, not T, but rather T*. But T*, when compared to T, is horribly, horribly *ad hoc*, complex, disunified. Whereas T is about relatively few different sorts of entity – the electron, proton, neutron, photon – T* is about thousands, possibly millions or billions of different entities – different states of matter, different chemical elements, compounds and processes. The underlying unity and simplicity of all the vast diversity of observable phenomena associated with chemical substances and processes can only be captured if one is prepared to construe all this in terms of interactions between unobservable electrons, protons, neutrons and photons.

But T*, because it is horribly *ad hoc*, complex, disunified, cannot remotely satisfy condition (ii) of section IV. T* is exactly the kind of theory that cannot be accepted, despite its immense empirical success, because of its grossly *ad hoc* character. (It is no good arguing that CE can validly consider the simple, unified basic postulates of T rather than T*, since T* can always be derived from these basic postulates. Wrong! Granted CE, T* cannot be derived from the basic postulates of T, since these postulates are about *unobservables*, and thus do not, according to CE, constitute factual scientific statements – whereas T* does. It is not T* that can be derived from the basic postulates of T, according to CE, but merely a formal imitation of T*, without factual content. Scientific realism, it should be noted, encounters no such difficulty since it holds the basic postulates of T, even though about unobservables, are nevertheless, like those of T*, factual statements about the world.[26]) CE, in short, makes it impossible to accept physical theories whose unity can only be captured at the level of *unobservable* physical entities. Or, put another way, there can be no rationale, given CE, for preferring a theory such as T* to endlessly many equally *ad hoc* rivals to T that could be concocted. Once again, CE is untenable.[27]

6 Can Constructive Empiricism be Salvaged?

Muller suggests that CE can be rescued from my refutation by acknowledging that science *does* accept that the universe is such that all grossly *ad hoc* theories are false.[28] I would welcome such a development. It ought to be recognized, though, that this would involve modifying CE very substantially. For it involves acknowledging that simplicity *is* relevant when it comes to assessing claims to truth in science. It involves appreciating that a metaphysical proposition about the nature of the universe is a secure item of scientific knowledge. Despite what Muller says to the contrary, the proposition in question *is* metaphysical, that is, unfalsifiable. It ought to mean, further, that such a version of CE adopts the meta-methodology of aim-oriented empiricism so that the

influential and problematic metaphysical assumptions of physics can be subjected to maximum critical scrutiny, thus promoting scientific progress.

Would such a quasi aim-oriented empiricist version of CE be acceptable? No. It might not fall to my first criticism of CE, spelled out in section 4 above, but it would fall to my second criticism, of section 5. As long as van Fraassen maintains that scientific theories do not embody knowledge – not even conjectural knowledge – about unobservable entities and phenomena, there is no evading the devastating criticism of section 5.

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Notes

[1] Elsewhere Muller has praised my work, for which I am very grateful to him: see Muller (2004).

[2] This broader argument was first spelled out in Maxwell (1976). It was given a much more detailed and authoritative statement in Maxwell (1984), and was further developed in Maxwell (1998, 2001, 2004, 2007a). For lucid outlines of the argument see Maxwell (2000; 2007b; 2008). For a recent critical assessment see McHenry (2009). See also Muller (2004).

[3] Maxwell (2002, p. 382).

[4] There are so many that it would be wearisome to discuss all of them. I shall concentrate only on those that have a bearing on Muller's main arguments.

[5] He refers to my (1998, p. 37).

[6] See my (1974, pp. 125-6), where I make clear that SE acknowledges that simplicity considerations govern choice of theory in science in addition to empirical considerations, and I refer to Mach, Duhem, Kuhn, Goodman, Scheffler and Rudner as having upheld such versions of SE.

[7] For detailed expositions of the argument see Maxwell (1974; 1984, ch. 9; 2004, ch. 1: and especially 1998, ch.2).

[8] Maxwell, (2002, pp. 383-385). This conclusion – that science "makes a big permanent assumption about the nature of the universe" – clashes with the central tenet of SE that "*no thesis about the world* [can be] *upheld permanently as a part of knowledge independently of evidence, let alone in violation of evidence*".

[9] Muller does get the conclusion of his argument right, even if almost everything else about the argument bears no resemblance to anything found in my writings. The conclusion of Muller's "master" argument is "science permanently accepts a substantial, metaphysical thesis about the nature of the universe" which is, near enough, the conclusion of my argument refuting SE, given in summary form in section 1 above. .

[10] "Aberrant" theories are theories that are grossly *ad hoc* or disunified; "regular" is Muller's term for theories that are unified or non-*ad hoc*.

[11] See, for example, my (1998, pp. 46-54 and 123-140).

[12] See Maxwell (1972; 1993; 1998, ch. 7).

[13] See my (1998, ch. 7, and references therein). For the latest exposition of my version of quantum theory see my (2009).

[14] See my (1998, ch. 4; 2004, appendix; 2007a, ch. 14).

[15] SignSubst (13) explicates "important" or "influential", I would have thought, rather than "substantial", which I would have thought refers to the *content* of a proposition.

[16] It may be doubted that this amounts to a definite proposition due to uncertainty as to what "*ad hoc*" or "aberrant" means in this context. One of the great triumphs of the conception of science that I argue for, aim-oriented empiricism, is that it solves the problem of what it means to say of a theory that it is "disunified", "*ad hoc*" or "aberrant": see Maxwell (1998, chs. 3 and 4). For more recent, and simpler expositions see Maxwell (2004, pp. 160-174; 2007a, pp. 373-386). What emerges from this solution is that there are eight different kinds of unity, and these come in degrees, 1, 2, 3,… This means that

the proposition that the universe is such that no *ad hoc* or disunified theory is true is not just *one* proposition, but a whole *range* of propositions. Once my refutation of SE is accepted, the problem becomes to discover how to *choose* the best metaphysical proposition concerning unity from the wide range that are available. Aim-oriented empiricism is a meta-methodology designed specifically to facilitate that choice. It is essentially for this reason that aim-oriented empiricism needs to be accepted, granted that my refutation of SE is decisive

[17] For expositions and defences of aim-oriented empiricism see Maxwell (1974; 1984; 1998; 2004; and especially 2007a, ch. 14)

[18] Acc(9) is interpreted my Muller to take one "from observable behaviour to unobservable mental states": see Muller (2008, p. 143). My refutation of SE, however, as I have already emphasized, does not touch upon unobservable mental states. It is about the explicit, public face of science, its theories, procedures of acceptance and rejection, methods, and their implications.

[19] See, for example, Maxwell (2004), pp. 210-211.

[20] See Maxwell (1998, pp. 211-212). van Fraassen, in linking acceptance to empirical adequacy, makes unrealistic demands that have not been met so far in practice as far as physical theory is concerned.

[21] See works referred to in note 7.

[22] The demand that the *ad hoc* theories in question are *precise* is here essential. Infinitely many *imprecise* grossly *ad hoc* theories are true even if the universe is perfectly physically comprehensible (in a sense of "physically comprehensible" I have explicated elsewhere: see Maxwell, 1998, ch. 5; 2004). I must add that condition (ii) really needs to be put into the context of aim-oriented empiricism, for reasons I have indicated in note 16.

[23] van Fraassen (1980, p. 88).

[24] van Fraassen (1980, p. 89).

[25] van Fraassen (1980, p. 90).

[26] In deriving T* from the basic postulates of T, bridge statements identifying observable with unobservable states of affairs will also be required – of no help in the CE case.

[27] This argument is spelled out in more detail in Maxwell (1993).

[28] Muller (2008, p. 156, point C).