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Dear Mario,

Let me first thank you for your comments on Hooker's paper which I passed on to him with the request that he should take them into account.

I should like to offer my apologies for writing with three weeks' delay. The reason is that I received the two referees' reports of your paper on November 20th and since I was rather taken aback by them I thought I would read it through very carefully myself. By an unfortunate coincidence the next day I developed an unpleasant allergy which held up my time schedule for a fortnight, and it is only now that I am back to normal.

I am afraid that there is something in what the two referees say. I find some of your examples and ideas most interesting though it is clear that some of them are already there in your contribution to Mind, Science and History and I find the remark that in your paper it is not clear what is novel and what is not (whether in relation to your own or to others' publications) unfortunately justified; and also I think you will agree that the breakdown of Popper's falsificationism has nothing to do specifically with what happened in physics since 1934.

I am embarrassed that both referees quote my 1968 paper. This paper of course is widely known in England (of course it arrived in the United States only through its expanded version in Criticism and the Growth of Knowledge). However, here at least if you write a paper on Testability 1970 people will find it difficult to understand if you do not refer to my paper in some way. As I mentioned this embarrasses me most about the two reports and I first intended to cut out these remarks, but then I decided I had better send them as they stand. !

I am terribly sorry that my first contact with you since I took over the BJPS has been on something of a sour note, especially as I hope that in spite of this hitch you will contribute more to the Journal than previously.

Please do let me know what you think of the reports.

With best wishes for Christmas and the New Year to both of you,

Yours sincerely,

Imre

Imre Lakatos

"Testability 1970" by Mario Bunge

Given the title of Professor Bunge's paper one would have expected a survey on "Testability 1970", a historical account of the development of the ideas of testability in the last few decades, possibly followed by the orginal contribution which ~~now~~ Professor Bunge now wishes to make to the subject. Unfortunately, the paper does nothing like this. In Bunge's account nothing happened between 1934 and the present paper. In 1934 Popper put forward his Demarcation Criterion which, however, as Bunge now points out, has not withstood 35 years of momentous advance in pure and applied science. But as it's well known to any scholar in the subject, Popper's thought itself underwent two considerable changes from 1934-70, and several sophistications and amendments have been introduced into it by his school, for instance by Agassi, Lakatos, etc. In the light of the present paper one would think that these people and their ideas had never existed.

The paper's references are occasionally quite absurd. For instance, when referring to the methodological principle that "there is nothing wrong with protecting a hypothesis with ad hoc hypotheses as long as the latter are in principle independently testable", he does not refer to Popper (1934) where this is perfectly clearly outlined, but he refers to a book of his of 1967. Also, when he mentions that "no theory faces experience by itself", instead of giving references to Duhem and Popper where these problems are ~~xxlxx~~ classically discussed, he again gives a reference to a paper of his published in 1970.

Thus one does not know what in the paper is expository and what is original contribution.

I do not recommend the paper for publication unless in a radically reduced form where Bunge either gives a clear distinction between published knowledge and his own new contribution. Or he should cut the paper down to what he regards as original contribution and assumes that the reader has reasonable background knowledge in the subject. In the latter case, of course, the title "Testability 1970" must be abandonned.

Bunge then uses the alleged metaphysical character of his "generic framework" type theories to support his view that Popper's demarcation criterion is faulty, or, at least out of date. But since these theories' metaphysical status is doubtful the support it renders to Bunge's theses is negligible.

I would add two criticisms of detail:

(1) Bunge seems to think that the 'weak refutability' or irrefutability of certain 'scientific' statements is purely a matter of logic, whereas, of course, it is often a methodological matter. A formula may be syntactically refutable, but we <u>decide</u> not to let it be refuted, cp. Lakatos [1968]. The failure to grasp this point leads to some confused sections in the paper. A good example is where the author talks about the "law of requisite variety" and quotes, with approval, a very opaque statement from Ashby about this law - (p.10). "Ashby notes rightly that, although this formula does exclude certain events as impossible it has nothing to fear from experience, for it is independent of the properties of matter".

(2) The view is clearly held that Popper's refutability criterion <u>was true</u> 35 years ago but has now <u>become false</u>. A look at the history of science guided by Kuhn or especially by Lakatos would quickly reveal the falsity of this position.

I cannot recommend the paper for publication.

M. Bunge : Testability 1970

The paper suffers from the defect that where it interestingly contradicts Popperian methodology most of its theses and arguments have not only been anticipated but have been anticipated in a more coherent and convincing way.

Bunge argues that Popper's refutability criterion has not "withstood 35 years of momentous advances in pure and applied science". That, in fact, (nowadays) not refutability but "confirmability and compatibility with the bulk of our scientific knowledge are jointly necessary and sufficient for a hypothesis to qualify as a piece of science", cp. Lakatos (1968) and that "there is nothing wrong with protecting a hypothesis with ad hoc hypotheses so long as the latter are in principle independently testable" cp Popper, Lakatos (1968), and further that "every thoroughbred scientific theory contains some hypotheses with a low degree of refutability" cp Agassi (1964), Lakatos (1968).

It is then argued that there are indeed some theories used in science which have zero empirical refutability. This point has been made before, cp. Watkins (1958), Agassi (1964), Lakatos (1968) but the specific theories mentioned do seem to be of a type which have seldom been discussed in this connexion. These theories are "generic frameworks helping one to think of whole classes of entities in a variety of domains". Among the theories here mentioned are game theory, systems theory, automata theory and general field theory.

Although Bunge never makes the point, these theories seem to fall quite naturally into 2 separate categories: (i) theories like general field theory which say something about the world though admittedly in a vague way and which can fail to "cohere" with other clearly scientific theories, e.g. general field theory with quantum electrodynamics. Type (i) theories can be dealt with as "influential metaphysics", Watkins (1958) or as 'hard cores' of research programmes, Lakatos (1968). (ii) theories like game theory, automata theory, which are mathematical in character.

Bunge's arguments that none of these theories is mathematical are terribly loose - "Is it not possible that they be purely mathematical theories? Answer: Not a chance, for they are concerned with concrete entities (?) though rather faceless ones, and they are sometimes used to design concrete systems such as communication nets, computers or even learning systems."

But a Euclidean triangle, say, seems just as "concrete" as an automaton in the sense of automata theory and, of course, arithmetic is used (as well as screws) in the construction of computers. Mathematics can be applied.

Bunge in fact argues that all of these "semi-interpreted schemata" or "generic framework"-type theories are metaphysical, and uses their scientific utility to (rather underhandedly) suggest (in section 6) that other kinds of "scientific metaphysics" like mereology are scientifically valuable. But the theses of section 6 are not supported by any of the arguments about the generic framework theories.