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More letters by Paul Feyerabend to Thomas S. Kuhn on *Proto-Structure*

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Abstract

Four letters by Paul Feyerabend to Thomas Kuhn from the 1950s and 60s were found in Kuhn's *Nachlass* at the Institute Archives and Special Collections of MIT. Two of them deal with topics that were at centre stage of the controversy between the two authors that was ignited by Feyerabend's reading of *Proto-Structure*, the draft version of Kuhn's *The structure of scientific revolutions*. These letters are reprinted here. They are probably the continuation of two letters by Paul Feyerabend to Thomas S. Kuhn formerly published in this journal (Hoyningen-Huene, 1995).

Keywords: Paul Feyerabend; Thomas Kuhn; Letters; Proto-Structure

1. Introduction

The word 'more' in the title 'More letters . . .' refers back to two letters by Paul Feyerabend to Thomas S. Kuhn that were found after Feyerabend's death in his apartment in Meilen, Switzerland, and which were published in this journal a little more than ten years ago (Hoyningen-Huene 1995). In those two letters, Feyerabend reacted to a draft of Kuhn's *The structure of scientific revolutions*, a manuscript I will refer to as *Proto-Structure*. Before proceeding to the new letters, I take the opportunity to correct two statements that I made in that earlier paper. First, I contended that 'Feyerabend had obviously forgotten' the two letters and I supported this claim by indirect evidence (ibid., p. 353). This contention is at least misleading, if not false. In his autobiography, Feyerabend writes:

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> my contrariness extended even to ideas that resembled my own. For example, I criticized the manuscript of Kuhn's Structure of Scientific Revolutions, which I read around 1960, in a rather old-fashioned way. (Feyerabend, 1995, p. 141)

These lines were written in the early nineties. So at least Feyerabend was aware of the fact that he had criticized *Proto-Structure*, and he may have remembered that he had done so in the form of several letters to Thomas Kuhn. Second, my guess was that the first letter was written between the fall of 1960 and the fall of 1961, and the second letter between the spring and fall of 1961 (Hoyningen-Huene, 1995, p. 354). However, according to recent archival studies in the Harvard Archives, by 22 April 1961, Kuhn had sent 'a draft of the *Structure* manuscript' to James Conant 'with a letter inviting criticism and making an appeal for Conant's endorsement to a publisher' (Driver-Linn, 2003, p. 272 n. 2). Thus, Conant as well as Feyerabend was among the recipients of *Proto-Structure* (see also Kuhn, 1970, pp. xi–xii). Given the aim of Kuhn's exercise, namely to receive criticism and Conant's endorsement to a publisher, for which a busy Conant (see Hershberg, 1995, Ch. 35) would need time, it is unlikely that Kuhn sent *Proto-Structure* to Conant *later* than to other recipients such as Feyerabend. (In fact, one would guess that Kuhn sent off his manuscript to his first readers very soon after its completion and the manufacture of the mimeographed copies. Thus, contrary to what Kuhn conjectured in a letter of 26 May 1994 to me, *Proto-Structure* was probably not finished in the fall or winter of 1960, but in April 1961.) Thus, Feyerabend probably received *Proto-Structure*, as did Conant, in late April 1961, implying that the likely earliest possible date for the origin of Feyerabend's first letter is May 1961.

At the end of Feyerabend's second letter, he writes: 'I shall perhaps send you another letter' (Hoyningen-Huene, 1995, p. 387). However, there were no additional letters to Kuhn to be found in Feyerabend's apartment. This is not really surprising because the two letters that were indeed found there owe this to Feyerabend's request to Kuhn to return the second letter to him (ibid., p. 372). A more likely place for additional letters would be the Institute Archives and Special Collections of MIT where Kuhn's *Nachlass* is located (Manuscript collection MC 240). And indeed, Box 21, Folder 28 described as 'Feyerabend, Paul, 1977–1988' contains Kuhn's correspondence with Feyerabend, although the time interval mentioned may at first be disappointing. The dated letters in that folder were all written between 1983 and 1993 (the earlier date of 1977 in the folder's description refers to a letter exchange between the Secretary to the President of the ETH Zurich and Kuhn of 1977 *about* Paul Feyerabend).

However, there are an additional four undated letters by Feyerabend. Two of them are handwritten. One refers to Feyerabend's enclosed comments on Hanson's 1958 book on the logic of discovery (Hanson 1958), and to a manuscript of Kuhn's paper 'The essential tension' that was published in 1959 (Kuhn 1959) (the comments on Hanson's book are not in the folder—Feyerabend had asked Kuhn in the letter to return them). We may thus safely assume that this letter was written in 1958 or 1959. The other handwritten letter refers to a manuscript of Kuhn's paper on thought experiments that appeared in print in 1964 (Kuhn 1964). However, Feyerabend refers to an earlier version of that paper because he exclusively discusses its last section in which, according to Feyerabend, Kuhn suggests 'that the procedure adopted by those who discuss thought experiments is similar to the procedure adopted by the Ordinary Language Philosophers' (p. 1 of the letter). Such a section is completely missing from the published version of the paper. Feyerabend urges

Kuhn to publish the manuscript 'for the philosopher's benefit, in either *Philosophical Review*, or *Review of Metaphysics*, or in *Philosophy of Science*' (ibid.) whereas Kuhn does not seem to display that intention: 'However quite apart from this fatal last section I think your paper is a masterpiece and I am again curious about the fact that all you do with it is hide it in your filing cabinet and occasionally give it to your friends' (pp. 4–5). As Kuhn's (printed) paper refers to *Structure* only once and in a very superficial way (p. 262 n. 31 of the reprint), but to other papers from the early sixties, I think it is quite likely that the draft Feyerabend refers to is from 1960 or 1961, but this remains speculation. Feyerabend's letter would then be from the same period. Both letters, denominated 'Letter 3' and 'Letter 4' are reprinted here in the appendix.

The other two undated letters strikingly resemble the previously published letters with respect to their general physical appearance. They are called here 'Letter 1' and 'Letter 2', respectively. Apparently, they are written with the same typewriter as the previous ones, they have the same small margins on all sides, and they are also single-spaced. They both refer to paradigms and both refer to an 'essay' (Letter 1, pp. 4 and 5; Letter 2, pp. 1, 4 and 7). This, of course, suggests *Proto-Structure* or *Structure*, which is strongly supported by the subjects dealt with in both letters. Letter 1 refers once to Kuhn's ideas, 'when published' (p. 1) suggesting that it indeed refers to *Proto-Structure*. This, however, is far from certain, because Letter 1 also refers to some 'reply' by Kuhn, a 'letter' (p. 1). Unfortunately, there is no trace of Kuhn's respective letter so we cannot decide which of Kuhn's unpublished ideas Feyerabend had in mind. Letter 2 refers to two students in Feyerabend's class who handed in papers 'that seem to be inspired by your {Kuhn's} teaching' (p. 1 and more explicitly on pp. 6 and 9). This suggests that at least Letter 2 was written at a time when both Feyerabend and Kuhn taught at Berkeley, that is, before 1964 (when Kuhn left for Princeton). As regards the sequence of the two letters, nothing certain can be determined. Letter 1 firmly announces 'a next letter' at the end (p. 5). However, Letter 2 also indicates a continuation of the exchange at the end (p. 10). In neither letter could I find any reference to the other letter, nor to the letters that dealt with the details of *Proto-Structure* (published in Hoyningen-Huene 1995). However, given that the two letters develop topics that Feyerabend had already treated in the previously published letters, but much more briefly and certainly not in a completely transparent way, it is very likely that the new letters are the continuation of those letters. We may therefore fairly firmly conclude that the letters were written sometime between May 1961 and 1964. With some likelihood, we may narrow this time interval down to the time span between May 1961 and 1962, assuming that the new letters are indeed the continuation of the older letters, and that they were written before *Structure* had appeared.

The content of the latter two letters is extremely interesting. One finally gets some deeper insights into why Feyerabend so violently opposes essential aspects of Kuhn's enterprise. Just recall Feyerabend's accusations in one of the earlier letters: 'What you are writing is not just history. It is <u>ideology covered up as history</u>.' And: 'It is this bewitching way of representation to which I object most, the fact that your take your readers in rather than trying to persuade them' (ibid., p. 355). In the present letters, it becomes clear that it is not just non-negotiable ideology that is involved, but a fundamental problem of all historiography, namely the selectivity of historical judgment. No historiographical study can cover *all* aspects of its chosen subject but has to make judgments about what to select. Thus, any given historical narrative necessarily rests upon decisions regarding values. 'Historical facts' are thus never value free, but always interwoven with those values that led to their selection as worthy to be narrated. However, even if this is admitted in principle, one might still object that in many cases the necessary value ingredient in historical representations is harmless. What could possibly be wrong with recounting Caesar's violent death in a history of the Roman Empire or, more to the point, about discussing Einstein's role in a history of quantum mechanics? However, Feyerabend contends that the evaluative basis of Kuhn's historiography is by no means harmless and trivial. On the contrary, with respect to that topic he writes:

> that I regard the topic we are discussing as one of the most important topics, not only of philosophy, but quite in general. Perhaps you will not agree, but I judge the importance of a topic from the influence a specific solution of it may have upon the well being of mankind. (Letter 1, p. 1)

So in Feyerabend's view there is a great deal at stake, and this is why he insists upon carrying through with the discussion.

The second very interesting subject in the letters is Feyerabend's sophisticated understanding of the falsificationist position, and why, in his view, Kuhn's criticism of it is ill-founded. According to Feyerabend, it is not the case that Kuhn's view of scientific development contradicts the falsificationist position. On the contrary,

> the description of scientific development which you give is not only not incompatible with the idea of falsifiability, but that such development is even demanded by the idea of falsifiability. (Letter 2, p.10)

This is certainly a sophisticated move on Feyerabend's side. However, as a referee of this paper noted, one may wonder whether there is some tension between this move and Feyerabend's hostility to Kuhn's representation of scientific development in the first letter. When considering this question, one should bear in mind that Feyerabend's preferred mode of criticism is immanent criticism (see, e.g., Hoyningen-Huene, 2000, p. 9). Thus, Feyerabend's rejection of Kuhn's criticism of the falsificationist position does not necessarily imply that Feyerabend endorses this position. In this case, Feyerabend would only point out that Kuhn's understanding of the falsificationist position is deficient, and therefore also his criticism of it.

Obvious misprints in the letters have been corrected. Everything in curly brackets is my addition; especially the indication of page breaks {n|n+1}.

In closing, I would like to point out how irony may turn into reality, even if it takes several decades. At the end of Letter 1 to Kuhn, Feyerabend adds, handwritten: 'P.S. I hear our correspondence will be edited posthumously. So we ought to be careful'. Right you were, Paul!

2. Letter 1

Dear Tom,

If you don't have any time–don't read this letter. I only want to make some comments on your reply. I shall not rest content until either I myself realise that I have been mistaken, or you realise that you have been mistaken or we realise that we agree. This attitude of mine is prompted by the fact that I regard the topic we are discussing as one of the most important topics, not only of philosophy, but quite in general. Perhaps you will not agree, but I judge the importance of a topic from the influence a specific solution of it may have upon the well being of mankind. And by 'well being of mankind' I do not mean physical well being, but the well being which derives, among other thing, from the exercise of one's imagination, from the full development of human faculties, and from spiritual happiness. A topic is important if a certain decision with respect to it, or a certain solution of the problems connected with it implies an increase, or a decrease of that well being. That is I am totally opposed to any attitude which says: "I am out to find the truth, come what may". What truth? And why? would be my question. The world is today full of people who deal with all sorts of problems simply because they are part of a tradition which deals with these problems. But traditions do not develop in a rational manner–this you admit yourself. Therefore the fact that certain problems are part of certain traditions does not impress me at all. For perhaps the whole tradition is superfluous, or dangerous and must be stopped (not by force, oh no, but by the sweet smell of reason). More specifically: why should we discuss complementarity and quantum theory? Why should we be interested in the history of a subject? If this interest does not have any consequences, of the kind indicated with respect to the well being of mankind, it is useless and should perhaps be given up.

Why do I say all this? I say all this in order to indicate why I am interested in the problems we are discussing. I think that these problems are very important because their solution, one way or an other, will have a tremendous influence upon the way we think, the way we act, and thereby upon our well-being. This conviction is the motor which makes me go on with the discussion and which makes me think that the discussion is worth while, however long it may take. I also go on with the discussion because I think that some of your ideas, when published, may have a disadvantageous influence. Now I do not think this is a case of truth being dangerous (as you seem to think; by the way your attitude seems to be: if it is true <u>and</u> dangerous, then I cannot do anything about it; I am a defender of the truth and I shall say the truth, whatever the consequences; this seems to be your attitude; I think that this is a very dangerous attitude; for you are not only a defender of truth; first of all you are a member of humanity and have obligations towards humanity; these obligations come first; these obligations do have the character of <u>certainty</u>; but whatever truths you may unearth – they have the character of <u>hypotheses</u> and therefore ought to be treated with great care when confronted with the certain obligations you possess). Ours is not such a case as I think that you are here not a defender of truth, but are simply mistaken.

Before I am going on let me say the following. You will forgive me, please, when I develop all the reasons for my interest in the topic. I am not doing this in order to preach to you about the obligations of a scientist (or historian), but in order to make clear to you why this topic makes me so exited and why I do not leave you in peace.

Now back to the main topic. Let me tell you only one thing–and having done so I shall at once return to your letter. You say that you are a historian who describes what happens. You will not describe it differently, for {1|2} this would mean being untruthful. Now the important thing with historical descriptions is that they are of a misleading ambiguity and that they very often contain normative and descriptive elements mixed up in one process. This process, this mixture it is which is then put in front of our eyes as a "fact" and we are not aware that in swallowing this alleged fact we at the same time swallow adherence to norms which are all but acceptable. Let me take a very crude example first which is not as unrealistic as it may seem at first sight. Consider a society which has been a society of warriors and on top of the warfaring societies. Assume also that a development sets in in this society to greater refinement, science starts (theoretical science, that is, and not engineering), the arts are developed, all the finer capabilities of man are developed, but at the same time the muscles degenerate and finally the society is overrun by a bunch of toughies. What can a historian do in this case? The obvious thing is to say that the society slowly degenerated, became weak and therefore finally was bound to collaps{e}. This is the way in which, for example, the collapse of the Roman Empire has been described. Prima facie this description is a factual description. For it describes what really happened, the gradual decline of the military strength. But at the same time the following happens: it is a factual description, hence it is a correct description, no other description would be correct–this is assumed. But if it is assumed that no other description would be correct it is also assumed (silently) that military virtues are the only virtues worth mentioning. This apparently factual description therefore contains, wrapped up in it, and hidden from sight, an evaluation: military strength is the only important measure of a society. But I am not prepared to accept this evaluation. On the contrary I am inclined to give a completely different description of what happened. I am inclined to say that what happened was not a decline, but wonderful progress, progress however, which was rudely disturbed by uncultivated intruders. Now note that this description is compatible with the one given first and it is compatible because it is based upon a different evaluation. What I want to derive from this example is the following: many historical description{s} contain a normative component. It is this normative component which allows for different, and yet compatible descriptions to be given of "the same process". Now if this is the case then the historian is obliged, if he wants to be honest, to do one of two thing{s}. One: he has to eliminate the normative component. My hypothesis is that then he will not be able to write history as it is the normative component which guides him in making the selection of material he does make. Two: he uses the normative component. But if he is doing this, then he better use the norms to which he himself is prepared to agree, i.e. the norms which belong to his own value system and the norms, the ethics which he would defend as a responsible citizen. What you do is (seems to me to be) the following: you swallow norms with which, I trust, you would disagree violently when presented with them naked, but you swallow them because they are part of what you regard as something completely factual. So under the cloak of FACT you accept what, I trust, you would dislike when presented with it in its real form, i.e. in the normative form.

This brings me at once to "science". You enter here the stage as a factual historian; as the man who is not interested in evaluations, but who is interested in finding the structur{e} of what is historically given. Like Galileo who looks at his inclined place and who waits what is going to happen you look at the historical material and discover in this material factual relations which you are then going to describe. These relations are there, and if they are undesirable, then we have to accept them as such. Now the important thing is that in describing them you do not bring out their undesirable character. You are the opposite of the man who says of the society in the above example that it degenerated, assuming silently {2|3} that military values are the only values of importance, or perhaps even the only values in existence. You look at history and find certain identities which constitute a tradition. You find relations between thought, instruments of research, publication, terminology etc. etc. and you concentrate on these relations (which include a paradigm). You concentrate on them and in this way discover an identity (of adherence to one and the same paradigm) and this identity it is which you regard as the characteristic of science. So your "factual" description consists in exhibiting this identity. However the important thing is that in this way you overlook that in other respects an identity may not at all exist. If we consider, for example, the way in which the paradigm is treated, we may discover a radical change from a more hypothetical to a more dogmatic attitude (to a certain extent this has already happened with special relativity; to start with it was regarded as a very peculiar, and therefore suspicious hypothesis; now relativistic invariance is a conditio sine qua non of a reasonable theory just as was causal character about a century ago). That is, if one does not put the paradigm into the center of one's considerations, but if one puts from the very beginning the methods for the handling of any possible paradigm used by a community into the center then the history will perhaps be not a history of continuous development, but a history of gradual change from a more hypothetical attitude to a more dogmatic attitude, or the other way round. Now concentrating on the paradigm and investigating its role in the history of a subject you discover an identity which you represent as a factual discovery. But just as in the above artificial example you have combined in your allegedly 'factual' discovery a factual element and a normative element. The normative element consists in the silent assumption that it is the paradigm that is important rather than the way in which it is handled (dogmatically, as the unshakeable basis of truth, or hypothetically, as a promising working hypothesis). Now, of course, you are under the impression that the central role of the paradigm is a factual discovery rather than the result of selection according to interest. But it is not. How do you say you discovered this central role? By looking under which circumstances a research community is established with definite problems as defined in terms of a paradigm. Without knowing it yourself you already start with an evaluation: what is good is research guided by a paradigm. Small wonder that this evaluation leads you to discovering the identities you discover. Look at the Presocratics. There is no paradigm which is retained all the way through from Thales to Democritos and Aristotle (unless we artificially construct one from the hindsight). And there is therefore no unity if by unity we understand reference to one paradigm. But if by unity we understand the way of handling paradigms, any paradigm, then there is a wonderful unity here: it is the unity of a tradition in which ideas are not accepted as infallible truth, but are regarded as fallible, and therefore in need of improvement.

Now considering all this (and, by God, I do hope I have made myself clear) I arrive at the conclusion that your allegedly "factual" discovery is a mixture of normative elements (attention directed towards unity of paradigm, rather than unity of method) and factual elements (how history looks if analysed from the point of view of a person who is interested in paradigms and their influence). The question which is still in need of being answered is why you direct your attention to paradigms rather than to method. Of course, the unity following from a paradigm is more concrete, and therefore may seem to be more "real" than the unity following from adherence to the same method. But is it therefore more valuable? This question still remains unanswered and it must be answered explicitly as you are already answering it, without knowing, implicitly. You may also point out that we are the followers of the paradigm-identity-development and that our science is a science which is the result of a series of developments in which always a paradigm stands in the center. But this is not essential either. For {3|4} as I have by now pointed out again and again alternative descriptions of this development are also possible, for example that considering method. Contemporary science is the result of a development towards dogmatism–and this description is as factually correct as yours, only it is based upon different implicit values and it brings out an aspect that is not brought out by your description, namely that not all may be well in the house of science.

To sum up: you say you describe facts. I say you don't as you introduce evaluations by the back door. And if you do, then you better introduce the values with which you agree rather than those with which you don't agree. And this you can as the alternative description which results from the adoption of a different value system will also be factually adequate. Also in presenting these hidden values and in pointing out that modern science as we know it is based upon a paradigm in the center you invite people to look for a paradigm and to stick to it as they otherwise would not be scientific. You suggest (implicitly in your essay, explicitly in discussion) that we could not even proceed in a different manner. This is only partly correct. Of course, we could not proceed in a different manner and still get the same results in terms of continuity of tradition, i.e. transference of a paradigm. But we would still obtain something – and as a matter of fact such a something did exist (hence a different procedure does lead to results), it was the Ionian cosmology which gave us the atomic theory. What more do you want? Also unity of paradigm existed long before modern science which you want to characterize by this unity: it existed when the Aristotelian philosophy dominated everything. This was unity of paradigm and it was against this dogmatic unity, I think, that Galileo fought and that all those fought who took the side in Democritos, both in physics, and in ethics and who were called 'slaves of matter and of the flesh' by their opponents. Thirdly modern science is possible without unity of paradigm: see the development in the 20th century. See especially all the rival hypotheses which were considered at the time Einstein's theory first came up: Lorentz' first theory, Lorentz' second theory, the theory of Aethermitfuehrung (Planck), Ritz' theory and which were gradually eliminated in a time when no paradigm existed in the domain of motion with respect to coordinate frames.

I repeat: your reference to paradigms as essential for science is not purely factual, it contains a normative element; secondly, paradigms are not essential to what you call science only – Arist{otelian} philosophy and physics; thirdly, what you call science is possible without paradigms. Of course, with respect to the last point you may point out that research concerning relativity did not occur in a vacuum: But neither did the research occur in a vacuum which preceded the Newtonian paradigm.

Now to your letter: Item (1) You say that you are not interested in a prescriptive methodology, but in a "more realistic notion of the practice actually used successfully in physical science". Successfully – according to what standard? Physicists may succeed to well entrench a certain theory such that an alternative account may in the end seem to be completely impossible. This would be success according to some dogmatic standard, but not success according to a standard which puts emphasis upon the fallibility of our knowledge. Conversely the fact that the Ionians did not arrive at a stable theory may induce some other people to say that they did not really succeed – i.e. succeed in establishing a final theory. As a matter of fact all the criticism which has been put forth against the Presocratics proceeds from this assumption: that success must lead to the establishment of the one and true theory. This criticism is the criticism of Plato, of the Baconians, of Descartes. According to the standard of the dogmatists the Presocratics were anything but successful. According to a standard in which progress and change rather than stability are to be valued, the Presocratics are extremely successful in the continuous improvement, elimination etc. of theories {4|5}. Again your "realistic notion of scientific practice" contains value judgements hidden in it; and my contention is that it is better to make these value judgements explicit than presenting them in "factual" covers and thereby putting over values in the masks of facts.

I agree with your point (2) – that from history one should not draw inferences concerning the validity of methodological rules – but, do you agree with yourself on this point (cf. our last discussion). And if you do not, what is your position? All this should be made explicit. Your essay contains so much that is hidden, because implicit. And it may impress for reasons which are different from the reasons for which people think they are impressed.

"But then ... I don't know any other source from which one could hope to derive ... conclusions about a "valid" prescriptive method" (your item 3). And you accuse me that I am doing what I forbid you to do, viz. I refer to consequences of social life, democracy etc. in arguments of validity. Now a full argument is of course not so simple. I start from a certain evaluation. For example I regard development of imaginative abilities of human beings, and, more general, development of as many abilities as possible as valuable and anything opposing such development (for example adherence to one system) as less valuable. Hence, such development should be encouraged. It can be encouraged within a democracy, within a hypothetical tradition, hence this tradition is to be encouraged. I therefore derive my evaluation of the hypothetical tradition from other evaluations plus factual knowledge, and not from factual knowledge alone.

Your description in (4) cannot be correct for the reasons indicated above. Galileo dealt with the relations between two objectively given objects – the inclined plane, and the rolling sphere on it. When you assert a certain unity of development you assert this unity with respect to certain situations, for example with respect to a paradigm. But you could have chosen a different point of reference; for example you could have chosen the amount of deity inherent in scientific theories as your criterion and then you would have found that what you call the scientific tradition is characterised by the gradual elimination of divine causation for the purpose of explaining natural events (for example restoration of the loss of momentum in non-elastic collisions). You do not mention the point of reference you choose. You take it for granted that methodological procedure is not the point of reference to be chosen. This is so because you are fascinated by a particular strand of development which stands out for you with your particular evaluations (this is like the duck-rabbit; only in this latter case evaluations do not enter). Looking at history you say – this is the real line of development and report it as if you had made a factual discovery. But if you had chosen, consciously a different point of reference, if, for example, you would have chosen methodological considerations as your point of reference, then you would have to report completely different facts. And a different factual strand again if your point of reference had been the amount of divine meddling allowed. And all these strands would be successful with respect to their peculiar aims. This is also why I cannot dig your item 5. It is not I who wants the facts to be different. It is you who reports mixtures of facts and values as facts and who thereby creates a muddle.

I think this is all I want to write today. I have to go to bed now. My next letter will deal with your remaining items. Be patient with me; that is, be patient with the letters you have and with those you do not yet have.

Paul

{In handwriting} P.S. I hear our correspondence will be edited posthumously. So we ought to be careful.

3. Letter 2

Dear Tom,

My affairs are not yet settled, and will not be settled for another six weeks at least (unless a miracle happens). This means that I am still surrounded by an ocean of noise (since I gave notice the noise seems to have disappeared! I am not quite clear as to what are the causes of this phenomenon.) and not very capable of thinking. However I cannot much longer keep back the comments I want to make on your essay. The longer I delay their deliverance, the more rebellious they become, and the more violent. Also there is a Kuhnian fifth column forming in my class (consisting, fortunately enough, of two members only) and the best thing to stop them is, I think, to stop their source. This is why despite my present predicament I still have decided to invade the lion's den with THREE COMMENTS, two critical, one not so critical.

(1) RELATION BETWEEN METHODOLOGY AND HISTORY. My main assertion is here: reference to history plays no role whatever in the arguments used for establishing and defending (or attacking) a certain set of methodological rules. Or, to express it in different words: history cannot help us a little bit in questions concerning the validity of such methodological rules.

Before going on I should perhaps make clear what I do not deny. I do not deny that history may make us aware of methodological rules of which we have not thought so far. Nor do I deny that history is capable of showing us which people have adopted which rules and who may therefore be regarded as a friend, or as an enemy. However I do deny that history can make a contribution to considerations of validity; that is, I deny that discovery of friends or enemies has got anything to do with the validity of the procedure shared by the friends, or disputed by the enemies. The reason why I deny this is very simple. It is due to my belief that the validity of any rule of procedure does not at all depend on the number of people who act in accordance with it, nor on the extent to which powerful or efficient institutions practice applying these rules (murder does not cease to be murder when practiced by the inquisition and rationalised by saying that souls are being saved that way). Imagine what would happen if the validity of certain demands (such as the validity of ethical rules, of methodological principles, of the rules of logic) were indeed dependent upon their incorporation into practice, or upon their realization in history. Anybody defending a different procedure could then argue for its validity only after he had converted a sufficient number of people, sufficient, that is, for making his school much more powerful and impressive than the school of his opponents. This being the case he could start collecting followers only by deception. But it is reasonable to assume that the Copernicans deceived, and deceived, and deceived, until they were finally strong enough not to need to resort to deception any more? And if this is not a reasonable assumption, does it then not follow that the arguments for, or against the validity of a certain procedure must be completely free from any reference to the extent to which these practices have been, or are being realised by some school, or community or other?

By the way, it is interesting to note that the idea of the methodological relevance of historical studies has a parallel in certain ideas that may be found in Wittgenstein: {one handwritten word inserted, but unreadable} rules, whether methodological or otherwise, are such things as can be obtained by an analysis of existing practices or 'forms of life', as he also expresses himself [by the way –you should really at some time read Wittgenstein in order entirely to eliminate his influence]. What has no foundation in such a practice is a 'castle in the air' that should be abandoned in order not to disturb the firm ground of language, or the practices, upon which it is built. However, if such a 'castle in the air' is built by enough people, then this constitutes sufficient grounds for adopting the rules implicit in it. Again, the transition from a stage where the 'castle' was not adopted by many to its almost universal acceptance cannot be brought about by arguments, it can be brought about only by a kind of deception for which there are found various names {1|2} in Wittgenstein. This point of view of course makes it completely impossible to give a rational account of the development of our knowledge. More especially, it makes it impossible to write a rational history of any subject concerned with the advancement of knowledge [it does, of course, not exclude the possibility of a Hegelian kind of history, where ideas develop miraculously out of other ideas and where the arguments of individuals and thereby the rational side of the subject is more or less irrelevant]. This is the way in which too strong an emphasis on the role of historical research may lead to the deterioration of historical research itself.

Now let me substantiate what I have said in a more concrete manner. In my lectures on the history of epistemology (144) I usually proceed as follows: I start by giving an outline of two traditions which I call the hypothetical tradition and the dogmatic tradition respectively. In the dogmatic tradition a doctrine stands in the center. The aim is to apply this doctrine to all parts of life and to show how well it represents the way things are. The doctrine is never questioned. What is questioned is either man's proper understanding of it, or his capability adequately to apply it to practical matters. Changes of doctrine, if they occur, are due either to change, forgetfulness, misreading of earlier texts; or they are the result of the victory of a tribe subscribing to a different doctrine. A rational, i.e. argued development or improvement of doctrine does not, and cannot exist. In the hypothetical tradition a completely different attitude is adopted with respect to theories. It is realised that theories, being the result of human considerations will be as fallible as the human beings who created them. Hence, the main thing is to find out their limitations, to improve them in cases where they have been found wanting and in this way to come to a better and better understanding of the world. In order to facilitate improvement theories must be formulated in such a manner that inadequacies, should they exist, can be easily recognised, i.e. they should be formulated in a clear language that allows for easy testing of the theory, and so on, and so on. I also point out that the first people to introduce, and to stick to the hypothetical tradition were the Ionian philosophers of nature (in this respect I agree with both Popper's article 'Back to the Presocratics' – Presidential Address of the Arist. Soc., published in the Proceedings 1959; and I also agree with the very similar point of view which Wally Matson has developed in his review of Cornford's Principium Sapientiae and in other papers of his; only, he makes the Ionians inventors of the method connected with cosmology, and not with the method characteristic for all parts of the sciences). That is, these philosophers not only invented cosmological theories, they also invented the method proper for the discussion of such theories, i.e. the method of introducing, and constantly improving (by criticism) hypothetical conjectures. When I say that they introduced this methodology I do not mean to say that they explicitly stated its rules. This was not the case. They introduced this methodology of test and improvement implicitly by creating a tradition, which was very different from the traditions then prevalent, where the doctrine did not form the center of understanding and was supposed to remain untouched, but where it was the starting point of criticism which latter {sic} was regarded as essential. It seems to me that it is the introduction of this method rather than the particular theories used in accordance with this method which must be regarded as the most essential contribution of the Greeks. When we say that we owe our knowledge to the Greeks, we should think of this feature above all. {2|3}

Now having introduced these two traditions, and having discussed early instantiations of either of them, I then trace their development to more recent times. More especially, I point out that science belongs to the hypothetical tradition (specifications will be made later on {handwritten addition:} alas, the letter grew too long, so the specifications could not be made. But I did make them in class.). In this way I try to show that the two traditions with their so very different approach to almost all problems still prevail and that the question as to which tradition we shall support is a very real question. It is also the most fundamental question of all epistemology (philosophy of science, of course, included). It is, to repeat, the question as to which tradition we shall support; or, to formulate it in a way that makes very evident its independence of the existence of either of the two traditions: which methodology shall we adopt, and defend – the methodology of proof in a dogmatic system, or the methodology of the improvement of hypotheses. Still another formulation of the question is this: shall we choose absolute truth and a method leading up to the final establishment of the one, true system; or shall we choose hypothetical truth and a method which enables us to find out the shortcomings of our hypotheses.

Before this question is answered we must become clear about the status of absolute knowledge and of hypothetical knowledge. If we assume, as is done by many philosophers, that absolute truth is there in the world and is adequately represented by absolute knowledge, if this is our assumption, then we shall have to take this into account, and we shall have to admit that absolute knowledge is required in order adequately to describe part of the world. On the basis of the same assumption the hypothetical tradition may be then said to represent only part of what is there; it is incomplete and, if it is supposed to be the only tradition worth considering, prejudiced. Now I argue that certainty, or hypothetical character is a formal characteristic of our theories rather than a material characteristic of a part of the world, a characteristic, moreover, which cannot be defended by reference to any features in the world. Of course, once we have adopted the dogmatic procedure, in the very same moment we shall discover a host of 'absolute truths'. But the important thing is that these 'truths' will be discovered only after one has decided to proceed in the dogmatic manner. Reference to their external existence therefore cannot be regarded as a motive for adopting this procedure in the first place. Or, to express it in a more intuitive manner: certainty, or hypothetical character of a system of knowledge are characteristics of this system that are entirely manmade. This being the case the decision to be made between absolute knowledge and hypothetical knowledge is a decision that cannot at all be supported by reference to characteristics of the world we live in. It is a decision which we have to make prior to any factual discovery and which, if followed through, uniquely determines the kind of factual knowledge (or of knowledge) we are going to obtain. As will turn out a little later this decision influences our attitude to human beings. It is therefore an ethical decision.

To repeat: prior to any factual inquiry a decision has to be made concerning the method to be followed. As this decision makes factual inquiry possible, it cannot be derived from factual arguments. But how shall we now proceed?

The procedure is not too difficult. One step consists in pointing out that absolute knowledge cannot be factually relevant. Other steps consist in pointing out the consequences for man, and for society, of the adoption of the one or the other procedure. It will be pointed out that the absolutist tradition, if it is to be successful, must not allow democratic institutions. It will be pointed out, that the hypothetical tradition increases freedom, power of imagination, and so on. On this basis the decision is made (by myself) in favor of the hypothetical tradition [footnote: all this has far-reaching consequences: for example I shall show in my lectures that having chosen the hypothetical procedure one will have to defend Brecht's discursive and non-aristotelian drama as opposed to the usual dramatic form; hence the decision is not a narrow, and specialised decision; it is bound to affect our whole life, our relation to others, and so on. But this only by the way]. We have therefore decided in favor of the hypothetical procedure. {3|4}

Now please note that historical considerations did not at all enter the considerations leading up to this decision. Of course, I started with the discussion of the two traditions which is a historical matter. But I did so only in order to make it very clear that a very fundamental decision is demanded and not in order already to decide one way or another. The existence of the two traditions presents us with the problem: whom shall we support? The one, the other, both, or neither? And in the discussion of this problem the existence of these traditions, the number of their followers and all these things are entirely irrelevant. Of course, once we have made the decision, in this very same moment we shall look at history, present or past in order to find out who our friends are, and who our enemies are. If there are no friends, then we shall try to persuade people to our point of view and in this way create a tradition just as the Ionians created the hypothetical tradition with its particular ways of argument, with its particular relation between master and pupil (the master encouraged the pupil to criticise him; he does not demand from him absolute following as seemed to be the case in the Aegyptian priesthood). If there are friends, we shall join them. If there are friends who, however, are not quite sure in their ways and who show inclinations to become dogmatic, then we shall criticise them severely. In any case, what happens in history will not at all influence the decision we already made. It will only influence our behavior on the basis of this decision. Hence, historical matters are completely irrelevant in matters concerning the validity of this decision.

This brings me to a very important point in your essay. More than once you emphasize (I do not remember your exact words) that existence of a discipline, such as physics, does not presuppose the existence of a set of rules. More specifically you say that the continuity of the development of physics is guaranteed already by the existence of institutions, of a tradition in the sense of membership in certain organizations, publication in certain journals, and so on, and that it is not based upon the existence of a set of methodological rules to which all participants of the tradition conform. This latter point may also be made by pointing out that continuity of development does not presuppose preservation of any rules throughout the course of the development. Now this point I am ready to concede (although I think there will be difficulties if one tries to formulate it in a more precise manner, but I shall neglect these). That is, I am ready to concede, for example, that the feeling of belonging to a certain tradition may exist in a group of people whose beliefs have nothing whatever to do with the beliefs of those who according to common opinion originated the tradition. Or, to express it in different words, I believe that preservation of doctrine, or of method is not a necessary presupposition for certain people's feeling that they belong to a very powerful tradition. Examples can be immediately given; the one is Christianity (the development from primitive Christianity to, say, the Roman Catholic Church); another example is Marxism; still another example is Greek philosophy (a continuity is perceived from the Ionians through Plato to Aristotle although methodologically the latter have very little in common with the former); also Descartes is by some felt to be the forefather of existentialism; and so on, and so on. But all we can say here is that sometimes people think they have something in common with some of their predecessors when they have nothing at all in common. Of course, the fact that people think they have something in common with some earlier thinkers will be expressed in their actions; they will share certain superficial similarities with these predecessors (such as similarities of terminology, or similarities of institutions) – but even this is not essential as the example of the Roman Catholic Church shows. The only connecting link between the early forms of Christianity and the latter is the fact that both put emphasis on the bible – but they interpret it differently, give it a different place; that is, the difference could not be greater. Now I do not think that it is a sound principle for a historian to make the mistakes of the people he investigates the basis for the selection of his material. That is, he should not make the results of mistaken assumptions of identity and continuity his criterion for identity or continuity. To be more specific: assume that physics starts becoming dogmatic and that it develops some doctrine which by many physicists is held to be fundamental for research and which they are not prepared to give up, come what may. The mechanicism of the 19th century provides an example of what I mean (think of the much quoted passage from the Erhaltung der Kraft). Physicists may still assume, mistakenly, that they belong to the same tradition as belonged Galileo and even Faraday. It will therefore still be possible to expound the new dogmatism in the usual physical journals (as it is possible, for Bohr and others, to {4|5} expound their dogmatism on the pages of Phys. Rev.) Note, that the possibility of doing this and the continuity of practice (of publication, of getting a salary as a physicist rather than as a dogmatic ontologist) following from it is the result of the mistaken belief that what Bohr does (I am now thinking especially of his answer to Einstein in Phys. Rev. Vol. 48) and what Pauli does (some passages in his Handbuch article), and what Heisenberg does (his contribution to Niels Bohr and the Development of Physics) is not very different from what Faraday did, what Einstein did, and what all the earlier physicists did. Now look what happens when a historian enters the scene and proceeds as I think you do proceed. He will notice that Bohr's "rules" are not exactly identical with Einstein's "rules" and that the latter are again very different from those to which Helmholtz and Kelvin adhered. As a matter of fact he will notice that there is a very great difference between any two of these thinkers. He will also notice that both Bohr and Einstein publish in Phys. Rev. (that is, it is not the case that Bohr publishes in Phil. Rev. and Einstein in Phys. Rev. or the other way round) and that both of them occasionally make reference to observational results, and that both of them are paid as physicists and listened to at physical congresses (where philosophers are usually ridiculed). His first discovery (difference of rules) apparently clashes with his second discovery (unity of tradition) unless the assumption is given up that a tradition presupposes the existence of stable rules – and this is how I think you have arrived at your result. But this result is not correct as your second discovery is not correct. What criterion would you apply when establishing unity of tradition? The criterion which you apply is identical with the criterion of the physicists who believe that Bohr and Einstein are doing the same thing and who are mistaken. Now their mistake has very interesting consequences. It has the consequence of creating an outward and apparent continuity (such as usage of instruments, journals used for publication, communities established). This continuity, which is the result of a mistake of judgment on the part of physicists you take as your measure. Small wonder that you arrive at this astounding conclusion that one does not need to follow any rules if one wants to be a physicist. Of course, as a physicist who makes mistakes of judgment one does not need to follow any rules. But assume that the mistakes are corrected; and assume that the tremendous difference is recognised, and emphasised, between Bohr and Einstein (the former belongs to the dogmatic tradition, the latter belongs to the hypothetical tradition). Then, of course, the continuity will not come into existence as people will refuse to do what Bohr is doing. – All this brings me back to my initial point: you want to give history a prominent position in considerations methodological; as a matter of fact you want to found methodology upon considerations historical (history, you say, is the material for methodology). History, for you, comes first. But the result of this attitude is now that you are not any longer able to give a reasonable account of historical developments. Instead of analysing the reasons for continuity you take continuity, which, as I have tried to show, is the historical result of an error of judgment in those creating the continuity, as an unanalysable thing and you are therefore prevented from giving a rational account of the history of the sciences, i.e. an account which includes all motives, mistakes included. That is, your over-evaluation of history leads to something like historical irrationalism, where things develop, where this development is the measure of truth and unity, but where we cannot understand the reasons which brought about the development. This is just as in Hegel: everything is history, and history is a mess.

Assume, however, that methodology is given prevalence. That is that we emphasise different possibilities of procedure and the importance of the difference between these possibilities. We shall then be able to put a well defined yardstick to history. We shall be able to see how certain ideals are given {5|6} up {"up" is inserted, the word is not readable} partly because of opportunism, partly because of sloppiness and we shall also be able to judge the mistakes of those who assert that there is an unbroken line from Galileo to Bohr. We shall be able to discover when theology is preached in the trappings of physics and we shall also be able to raise a warning voice when the first signs of such deviations become apparent. We shall not any more be impressed by existing continuity and choose contemporary science as an example just because it has somehow developed out of the ideas of Galileo (or of More?). We shall be able to criticise, and thereby to advance the cause of science because our yardstick is not part of the very indefinite mass of historical facts. What this difference amounts to, i.e. the difference between you and myself is of course the difference between immanent, and therefore relative values on the one side; and transcendent, and therefore absolute values on the other. I myself prefer the absolute point of view not only because of its absolutism, but because of the clarity it leads to in history, science, and everywhere else. I do hope I have made myself clear on this point. And if I have – do you agree?

(2) FALSIFIABILITY. It is on this point that I got reactions from my students. As you may know I have abandoned questionnaire-midterms. This is too uninteresting a kind of examination. For the important thing is not to be able to give answers to questions; the important thing is to be able to select problems. Hence I ask my students to write about a topic of their own choice which to them seems to be important and somehow related to the topic of the course. Two papers which I got seem to be inspired by your teaching and they made it very clear to me what the issue really is (by the way, the papers were quite good, though not very critical). I shall split discussion of this issue into three parts viz (a) relation between method and actual practice; (b) proper interpretation of what actually happens in the sciences; (c) possibility of doing science on the basis of the methodology of falsifiability.

Regarding the first point I have already made clear my position: if it should indeed happen that scientists do not take falsifications seriously and are content with ad hoc hypotheses, if this should indeed be the case, then I would say "the worse for the scientists". Such procedure would be an indication for me that scientists have become dogmatic and this would finish them for me. (I am not interested in dogmatic systems, even if such systems are taught in physics departments, and even if they get the Nobel-prize which would then correspond to the donation of a bishoprice, or some other remunerative thing in the Roman Catholicism of the Middle Ages). By the way, it should be clear what I understand by an ad hoc hypothesis. Copernicus' assumption to the effect that the fixed stars are very far away is not an ad hoc hypothesis as it admits of independent test. An ad hoc hypothesis concerning the theory T is a hypothesis whose only criterion of truth is that it would be incorrect if the hypothesis were not true. Such hypotheses are characteristic of dogmatic systems. They abound in primitive myth where the myth is indeed the measure of truth. They are not frequently found in the sciences. A good instance is the later form of Lorentz' theory which includes not only the Lorentz-contraction, but also time dilatation and all the other effects to be expected if an aether exists. In this later form Lorentz' theory is indeed nothing but a hypothesis which preserves the aether assumption without adding new tests to the theory. Hypotheses of this kind must be excluded (I think that the impetus theory is of a similar kind). But, as I repeat, they are very rare within the sciences. But if they should occur my attitude would be as indicated: too bad for the sciences. And I would start becoming one of the most violent critics of scientific procedure thus degenerated. And I think that you, too, would join me in my criticism. For the cases which you do discuss are not at all of the kind envisaged here. However they cannot be regarded as an objection against the methodology of falsifiability either. As a matter of fact they are perfectly compatible with this methodology. This brings me to the second one of the three points separated above. {6|7}

In your essay you point out more than once that falsification is too simple and naive an account of what happens in the sciences. I do not remember your exact words, but this, at least, is their spirit: when we investigate what happens to theories we do not find that they are first introduced, and then given up immediately after the first contrary experimental result. They are retained a considerable time, people try to adapt the experimental results to the theory, or they simply wait, hoping that at some future time a satisfactory way will be found of incorporating the resisting experimental results. Nor is the theory given up if it is found to be in real trouble, unless a new theory is available which can take over. This being the case, you infer, the idea of falsifiability gives much too superficial and naive an account of actual scientific practice. For this idea, so you seem to assume (and it will turn out that it is this assumption, which is erroneous, which leads you to criticising the theory of falsification) demands that a theory be given up immediately after the first falsifying instance is found. Not only is this not the case, you say, but it is also desirable, that it should notbe the case (item c above): for if we were indeed to proceed in such a manner that we give up a theory as soon as a difficult case arises, if we were indeed to abandon a theory as soon as the first falsifying instance turns up, then theories would come and go, they would never be worked out in detail, they would never obtain that imposing shape we know from classical mechanics, from classical electrodynamics, nor would they be ever developed in such detail as to give us a universal picture of the world. The idea of falsifiability, you conclude, is therefore not only naive as regards the actual procedure of science; adoption of it would also be detrimental to the state of the sciences; and it would be detrimental to the state of science as falsifying instances always turn up almost immediately and as no theory could therefore live very long and be developed in sufficient detail. This, so it seems to me, is your position. My reply is that this argument of yours is due to too naive an interpretation of Popper's methodology. Hesitation to give up a conceptual scheme, retainment of it even after decisive falsifications have occurred and all the other instances you refer to are perfectly compatible with the ideal of falsification and perhaps even demanded by it. The only thing that is not compatible with the theory is conscious use of ad hoc hypotheses in the sense explained above and refusal to worry in the face of admittedly refuting instances. It is true that these demands are very modest, and almost trivial and at some time in our discussion you have said to me that you find them trivial. However it must not be overlooked that despite their triviality these demands are consciously violated by all members of the dogmatic tradition where the theory is the only measure of truth and where contradicting statements, even if they should happen to be empirical, are excluded just because they contradict the adopted theory. But let me now analyse a little more in detail the historical cases which you have adduced with the purpose of showing the limitations of the idea of falsifiability and let me also discuss some of the points which arose in our oral arguments. To repeat, my contention is that all your findings are perfectly compatible with the idea of falsifiability and that even the demand not immediately to give up a theory may be made compatible with it.

I shall proceed in small steps in order to make things as clear as possible. Let me therefore start with the most extreme example. Let me suppose that a theory T is found to be in trouble and that it is admitted by everybody that it has been refuted by the facts. This was the situation in classical mechanics before Bohr had started his investigations. You assert that even in this case T will be retained unless a new theory has been found which takes its place. My reply is (α) that it will be retained even after such a new theory has been found; and (β) that such a procedure need not be incompatible with the idea of falsifiability. Concerning (α) I need {7|8} only point out that after all we still use the classical mechanics for the calculation of the behavior of the upper planets. But (β) such use is perfectly compatible with falsifiability. For this idea does not deny that a false theory may be a good instrument of prediction, and it therefore does not prohibit its continued use as such. It only asserts that a refuted theory must be regarded as false. What it forbids therefore is drawing realistic consequences from a theory that admittedly has been falsified. As soon as a theory has been falsified we must stop making it the basis of our interpretation of the world. On this point we must completely suspend judgment until we are able to understand what are the reasons for the failure of the theory. The theory is therefore finished in one sense: it is finished as a picture of the world. Which, as I said above, does not preclude its use as a predictive instrument or as a heuristic guide in the search for a new theory.

However the cases which you discuss are not cases where a theory has been admittedly refuted. They are cases where certain difficulties exist but where it is not yet admitted that these difficulties amount to the refutation of the theory. Such a situation is not excluded by the idea of falsifiability. Quite on the contrary: the idea of falsifiability demands that criticism be applied not only to the theories we possess, but also to the falsifying instances which we believe we possess. And a prolonged hesitation with respect to the latter simply means that we are not yet convinced that their falsifying character has been definitely established. Such prolonged hesitation and the attempts to make the instances compatible with the theory, all this amounts to vigorously testing the apparently falsifying instances themselves. If all such attempts misfire, then the falsifying instances have been confirmed to a very high degree and therefore very decisively argue against the theory. This being the case hesitation, and the attempt to show that what prima facie seems to run against the theory as a matter of fact is perfectly compatible with it {and} must be regarded as completely in accordance with the idea of falsifiability. For this idea says that a theory should be given up (that is, that the realistic interpretation of a theory should be given up) as soon as a falsifying instance has been discovered. It does not say that the theory should be given up as soon as the first inconsistency with facts arises. Quite on the contrary, it demands that a falsifying instance should not be trusted more than a theory to be tested, unless it has been subjected to at least as vigorous tests as the theory itself. In other words, it demands that it should not be accepted too lightly. It should not be accepted before it has been tested in all possible ways. And testing a falsifying instance in all possible ways is just the same as trying out all possibilities of making it compatible with the proposed theory. This is why the attempt to try out a theory as much as possible is compatible with the idea of falsifiability. You seem to assume that accepting this idea means giving up a theory on the slightest pretext. But doing this would mean accepting some other lower-level hypothesis on the slightest pretext, viz. the refuting instance. And this we shall not do once we have decided vigorously to test all elements of our knowledge. Also Popper's theory of falsifiability makes not special assumptions concerning the way in which falsifying instances are produced. We both agree that sometimes a falsifying instance can be produced only by confronting the theory under investigation with an alternative theory; and as a matter of fact I (and perhaps also you) would go even further and say this is what always happens (although the alternative theory very often is not explicitly formulated, but is part of our 'background knowledge'). The general theory of falsification has nothing to do with this specific ways {sic} of producing falsifying instances. It makes this trivial, but still important statement that once a falsifying instance has been produced the theory falsified will have to go. Taking all this into consideration I cannot {8|9} agree with you when you say that adopting the theory of falsification would mean giving up theories on the slightest pretext, that theories are not given up on the slightest pretext, and that therefore falsification is too narrow on epistemology. Such a criticism applies to any theory of basic statements: if we assume that there exist absolutely certain statements of observation, and that the theory is about them, then 'a disagreement with facts' indeed would be the end of the theory. But if we drop the idea that statements of observation can be established with certainty, if we admit that any scientific statement is but a hypothesis, in the very same moment we may try to retain the theory in the attempt to test the test statement itself. One thing, of course, must not happen: it must not happen that we take the truth of the theory as a criterion (and perhaps the only criterion) of the truth of the test statement. This would mean accepting dogmatism.

A final remark about what one of your students wrote in his paper. It is essential for Popper's theory that we must not attempt to show that a theory is correct. What we must do is rather to test it; and as every test is an attempt at falsification we may therefore also say that what a scientist tries is to overthrow the theories he possesses. To this your student objected when a man who deals with a theory and wants to use it for giving an account of what happens in the universe must first of all believe in it. He emphasises that it is psychologically impossible for a person to be concerned about a theory and at the same time to try to overthrow it and that for this reason Popper's methodology cannot work. I also got the same argument in class with reference to a passage in your 'Copernican Revolution' where you say (I quote) that "a scientist must believe his system before he will trust it as a guide to fruitful investigations of the unknown". Now this is of course very correct, and almost trivial. A scientist will consider a theory only if he is interested in it (by the way – he does not at all need to believe it, he may try out a theory he does not believe in in order to check his non-belief and discover to his surprise that it works). But at the same time he knows that his interest, and his belief is not sufficient to guarantee truth. He must therefore test the theory and he will test it exactly because he believes in it and because he is interested in it [we may perhaps test the virtue of a woman we want to marry; but we shall not test the virtue of a woman in whom we are not interested; in the latter case her faithfulness is of no interest to us; as a matter of fact we could not care less]. Now what does it mean to test the theory? A test is an experiment one of whose outcomes would refute the theory. Hence a test is an attempt to overthrow the theory. This attempt is of course undertaken with the belief, and the hope that the theory will come out alright. But a possible overthrow it is nevertheless. For if it would not be a possible overthrow there would have been no need to carry out the experiment. We could have foreseen its result in advance and we could have outlined in advance the structure of the world as seen by the theory. However apart from being possible refutations the tests, if successful, i.e. if confirming the theory, have also another function. They are successful explorations of the world according to a definite map, i.e. according to the theory. They therefore shape a certain picture of the world; this picture will be more definite and clear the more decisive the test and the more confidence we may possess in the correctness of its outcome. I.e. this picture will be the more definite, the more critical we are with respect to the outcome of the test itself. This is how belief in a theory leads to attempts to overthrow it and how attempts to overthrow a theory lead to the construction of a picture of the world which becomes more and more definite until finally we as it were see the world from the point of view of the theory in a very similar manner as Kant thought we always see the world from the point of view of the Newtonian framework. {9|10}

I do not know whether I have made my points clear. If I have succeeded in doing so you should have realised that the description of scientific development which you give is not only not incompatible with the idea of falsifiability, but that such development is even demanded by the idea of falsifiability.

There are various other things I would have like{d} to say, but I leave all this for later. I shall send this letter by a student. At the present moment I am confined home with a swollen foot (infection), an affair which may easily last another three days. I am curious as to your reactions.

Be well!

Paul

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I wish to thank Dr Eric Oberheim for improvement of my English.

Appendix 1. Letter 3

Dear Tom,

Enclosed my comments on Hanson's "Logic of Discovery". The comments are only partly about the relevant passages in this book – in the main they are about an article he published in the Journal for Philosophy + about a very similar paper he read in Chicago. Please return my comments as soon as you have read them – they are the only copy I possess. Could I please get a copy of the item you refer to in footnote 3 of your paper on the "Essential Tension"?

As regards this latter paper, I have one remark and one question.

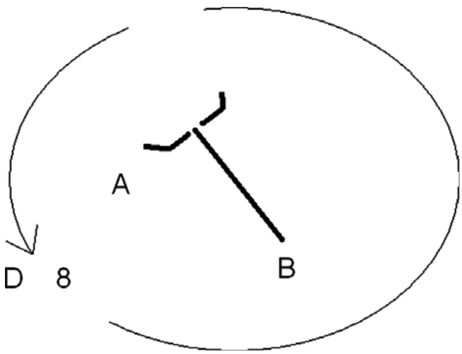
My remark: On page 6 you deplore the absence of collections of readings in the natural sciences. This does not seem to me to be entirely correct. Collections which come to my mind are (a) Beyer's collection of papers on Nuclear Physics (English, Italian, French, German); (b) Sommerfield's collection of papers on Relativity (Lorentz-Einstein-Minkowski); (c) Wax's collection of papers on Stochastic processes; (d) Schwinger's collection of papers on quantum Electrodynamics, with its very interesting and highly critical introduction. Again, when asserting the "convergent" character of the textbooks you have not mentioned that some {1|2} Encyclopedias, such as the famous "Blue" and "Green" Handbuch der Physik, as well as the Encyclopaedie der mathematischen Wissenschaften (in which the Ehrenfest's article on statistical mechanics originally appeared) are written in a sometimes very different spirit. The same applies to the review-article in Reviews of Modern Physics where all difficulties are explained. My thesis of your paper, if I understand it correctly, is this: revolutionary advances in the Greek Style do not occur until some kind of convergence has been established. I wonder whether it is not the case that here cause and effect have been interchanged. Could it not be the case that the first time a universal and testable (not metaphysical) theory is put forth which is detailed enough to account with precision for a lot of things, that in the very same moment many reasonable people will become interested, will try to work out consequences, formally improve the theory, to test it and that thereby a kind of convergence is established which of course will in its turn soon lead to the discovery of weaknesses of the theory and thereby to further revolutions. If that is correct then the main emphasis should not be upon convergence, but {2|3} upon theories that are likely to bring about convergence, i.e. upon theories which are universal, testable, detailed and therefore, because promising to many, liable to create convergence, i.e. concentrated effort which of course will then lead quickly to new discoveries. Have I made myself clear? And am I right?

Kind regards
Paul

Appendix 2. Letter 4

Dear Tom,

I have read your paper on thought experiments but I shall not yet return it as I want to reread it most carefully. Let me only make some very general remarks. I found the paper extremely illuminating and I think you should publish it, for the philosopher's benefit, in either Philosophical Review, or Review of Metaphysics, or in Philosophy of Science. However my elation left me completely when I turned to the last chapter where you seem to suggest that the procedure adopted by those who discuss thought experiments is similar to the procedure adopted by the Ordinary Language Philosophers. For I believe that the attitude is completely different! Let me start with a secondary remark, secondary, that is, to the adopted procedure. Hardly anybody within the Ordinary Language Circle is acquainted with the work that has been done with respect to the "puzzles" he is dealing with. I got my shock of my life to hear Austin seriously discuss the following "puzzle": a dog D circles around a cow AB (A: the head of the cow).

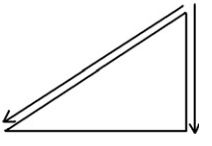


The cow, afraid of letting the dog get out of sight, turns round with him: does the dog, or does he not run "around" the cow? This was not meant to be an example of a thought experiment {1|2} carried out two thousand years ago, but an example of a "puzzle" to which a solution has not yet been found. Has this man, who is supposed to be one of the greatest philosophers of our time (how on earth could it happen that a charlatan became so famous?) never heard of the relativity of motion? Also, in his "Dilemmas" Ryle discusses at length Zeno's paradoxes with not the slightest reference to modern ways of resolving them (quite possible that he would not be able to understand any of these solutions!). I think that it is not too far from the truth if we say that as regards knowledge of suggested solutions, knowledge of the history of a problem, knowledge of the particular circumstances under which it arose, it is not too far from the truth to say that with respect to all these things the Ordinary Language Philosophers are a bunch of Ignoramuses (again the problem arises why they could become so famous). However this remark is only secondary. The most important difference between the OLPs and those scientists who frame consideration of thought experiments proceeded to a recasting of our ways of thinking about nature is the following. The scientist is aware that our ordinary means of expression may be faulty and may need revision. Once this need of revision {2|3} is recognized, and presented, most clearly, in an imaginary experiment, he will resolutely start out to tear down large pieces of common idiom and to replace it by what at first sight will seem a fairly artificial idiom, but what as a matter of fact will be much more adequate. For him a thought experiment is a starting point of the revision of language. Most OLPs I have read, or met, proceed in a completely different manner, and this by the way is also Wittgenstein's manner. They will consider paradoxes; quite correct. But first of all their paradoxes very rarely can be regarded as Centers of decisive innovations. And secondly the purpose is not revision of language, but realization that, properly understood, the language one is talking can already take care of the situation. Take Wittgenstein. What the thinker ought to be done ? (within what he calls "philosophy") is to "assemble reminders for a particular purpose" (Investigations, 127). What is the purpose? The purpose is "complete clarity" (133). However how is this clarity to be achieved? Not {3|4} by retiring and completing the system of rules for the use of our words (133). As a matter of fact "Philosophy may in no way interfere with the actual usage of language ... <u>it leaves everything as it is</u>" (124). But this means that the OLPs remove the paradoxes exhibited in their thought experiments in a way completely different from the way adopted by the scientists. Their intention is to show that without any change our language is already capable of accounting of the case. Without further argument (for my letter is already getting too long) I must say that this is done always either by a kind of swindle, or by the suggestion of most primitive and unilluminating theories. There is therefore not the slightest justification to consider the discussion of paradoxes by OLP as a parallel to the discussion of thought experiment by scientists. Standards, procedures, and the cases discussed are completely different in both cases.

Forgive, please, my strong words, but the fact that uninformed cranks are taken seriously has always aroused, first my curiosity, then my anger, then again my curiosity. However quite apart {4|5} from this fatal last section I think your paper {is} a masterpiece and I am again curious about the fact that all you do with it is hide it in you{r} filing cabinet and occasionally give it to your friends.

Paul

P.S. As a matter of fact, your paper could be a most important contribution to exhibiting the complete barrenness of OLP. This could be done by comparing discussions of the example of the inclined plane



with a similar discussion of a concrete "thought experiment" by an OLPer. Such comparison would, I think, most convincingly show the tremendous advantage of the "Galilean method" of revision as opposed to the other method of talking around and around the problem until one has lost track of it (one has "dissolved" it).

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