0039-3681(95)00005-4

Two Letters of Paul Feyerabend to Thomas S. Kuhn on a Draft of *The Structure of Scientific Revolutions*

*Paul Hoyningen-Huene**

Introduction

Paul Feyerabend died on 11 February 1994. When, shortly thereafter, I assisted his widow Grazia Borrini Feyerabend in clearing his apartment in Meilen, Switzerland, we found in a box, underneath a pile of maps, some letters and other documents about which Feyerabend had obviously forgotten: he had never mentioned them either to his wife or to me, nor had he used the material in his autobiography. Usually, he threw away all the letters he received and kept no copies of letters he had written. Among the surviving items were letters by Lakatos and Smart, and the originals of two letters Feyerabend had sent to Thomas Kuhn. That these letters were in Feyerabend's possession is explained by the fact that he had asked Kuhn in the beginning of his second letter to send the letters back to him.

When were the letters written? Since they are undated, one has to use indirect evidence.

1. According to Kuhn's own, admittedly vague recollections (letter of 26 May 1994 to me), he had finished a mimeographed draft of *The Structure of Scientific Revolutions* in the fall or early winter of 1960 when both he and Feyerabend had been at Berkeley. The page references in Feyerabend's letters appear to refer to this mimeographed draft, but Kuhn does not remember and cannot reconstruct when he received Feyerabend's letters. Thus, the earliest possible date of composition of the letters is the fall or early winter of 1960.

2. In his second letter, Feyerabend refers to an article by Kuhn (by page and footnote number) that appeared in *Isis*, vol. 52, pp. 161–193. This implies that the letters (at least the second one) were not written earlier than the spring of 1961.

*Philosophische Fakultät, Universität Konstanz, Fachgruppe Philosophie, Postfach 5560, D–78434 Konstanz, Germany.
Received 20 *August* 1994.

Pergamon

Stud. Hist. Phil. Sci., Vol. 26, No. 3, pp. 353–387, 1995

0039–3681/95 $9.50 + 0.00

3. In the beginning of his second letter, Feyerabend mentions the manuscript of an article he had written whose publication by University of Pittsburgh Press he expected in "January", and whose proofs he expected to arrive in the preceding November. This must be an article of some size and importance since Feyerabend refers to the publication in question as "one of my opera magna". The only major article by Feyerabend published by University of Pittsburgh Press between the late fifties and the mid-sixties is "Problems of Microphysics", in R. G. Colodny, ed., *Frontiers of Science and Philosophy*, pp. 189–283. This volume was published in 1962. Thus, the November mentioned seems to be November 1961, a date which would have had to lie in the near future at the time when the second letter was being composed.

Thus, the most likely date for the composition of the letters can be narrowed down to the time span between the fall of 1960 and the fall of 1961 for the first letter, and to the time span between the spring and the fall of 1961 for the second letter. More precision in the determination of the date does not seem to be necessary.

Why is it worthwhile, from a systematic point of view, to publish these letters, especially since there is a printed reaction by Feyerabend to Kuhn's position in the volume *Criticism and the Growth of Knowledge*, edited by Imre Lakatos and Alan Musgrave and published in 1970? First, the letters appear to be much more direct and lively than Feyerabend's printed commentary, and there is only little overlap between the two. Second, Feyerabend's letters anticipate many of the arguments that were put forward in the public controversy against Kuhn's position, including some of the (numerous) misunderstandings. Third, Feyerabend's assertions and arguments are very characteristic of his position in the early sixties.

Obvious misprints and mistakes in punctuation in the letters have been corrected. After the page references to Kuhn's draft, I have wherever possible inserted, in curly brackets, the respective page numbers of the second edition of *The Structure of Scientific Revolutions*. If the formulation cited in the letter is not identical or not very nearly identical to the respective formulation in *Structure*, I have cited the version of *Structure* for comparison. Everything contained in curly brackets is an addition of mine.

Finally, I would like to thank Grazia Borrini Feyerabend for her kind permission to publish the letters.

Paul Feyerabend's First Letter to Thomas Kuhn

Dear Tom,
I shall start my comments on your essay with some general remarks which will in part repeat what I have told you before. I more than ever think that your essay is quite unique as regards the contribution it makes both to the history and to the philosophy of science. On the other hand my impression of danger, and my misgivings have been

very much increased. What you are writing is not just history. It is ideology covered up as history. Now please, do not misunderstand me. I do not say that we should live without ideology—quite on the contrary, I think that living without an ideology (the correct one, of course) would be both impossible and inadvisable. Nor do I say that history should not be written without a point of view in mind—quite on the contrary, I think that such a history, if it would be possible, would be the most drab and uninteresting affair imaginable. Nor, thirdly, do I pretend that in history a nice distinction can be drawn between what is regarded as a factual report, and what is regarded as interpretation according to some point of view. But points of view can be made explicit, and it is possible to write history in such a manner that the reader is always aware of one's ideology or point of view as well as of the possibility of an alternative interpretation of the historical facts. That is, history can be written in such a manner that what is factual and what is reasonable appear as two clearly distinct affairs. In some cases it is very easy to keep this distinction clear. Nobody will think that the history of crime justifies crime, or shows that crime possesses an inherent 'reason' or an inherent morality of its own. In the case of the sciences or of other disciplines of which we have respect the situation is much more difficult and the distinction cannot be drawn with equal ease. But in these cases it is of paramount importance to make the reader realize that it still exists. You have not done so. Quite on the contrary, you use a kind of double-talk where every assertion may be read in two ways, as the report of a historical fact, and as a methodological rule. You thereby take your readers in. You present your material in such a manner that (at least for the periods following the introduction of paradigms) history seems to satisfy the principle that ALLES WIRKLICHE IST VERNÜNFTIG so that evaluations can then be directly derived from historical study. It is this way of presentation which I find objectionable. I do not object to your belief that once a paradigm has been found a scientist should not waste his time looking for alternatives but try working it out. That is I do object to this belief, and I shall have very soon to say a little more about it. What I do object to most emphatically is the way you present this belief of yours; you present it not as a demand, but as something that is an obvious consequence of historical facts. Or rather, you do not even talk about this belief, you let it as it were emerge from history as if history could tell you anything about the way you should run science (is does not imply ought!). It is this bewitching way of representation to which I object most, the fact that you take your readers in rather than trying to persuade them. This manner of presentation you share with Hegel and with Wittgenstein; and with all those who say, when engaging in a political enterprise, that "history will be our judge". Do you really believe this? (You differ from them only insofar as you refer to the past whereas they refer to the history of the future.) Or have I perhaps completely misunderstood you?

Just a further remark before I turn to the more specific points. You have expressed to me your belief, and you express it again in your essay that it is only by concentrating on a single paradigm, by trying to fit nature into it despite all apparent difficulties, that scientific progress is achieved. You seem to think it psychologically impossible for a scientist and, indeed, for any human being to be able to entertain various alternative hypotheses and to discuss them impassionately. I think you are a little too pessimistic. Faraday did so, if I am correctly informed, and so did the Presocratics, so did Einstein (partly classical, partly non-classical considerations). Also I think I have shown in my own essay that considering a set of mutually inconsistent but factually adequate theories increases the empirical content of any element of the set and this for the simple reason that many tests presuppose the existence of an alternative! (they are crucial tests). If this is the case then we must make the decision: what do we prefer, increased empirical content of the theories we possess, or that unanimity of research and the close fitting produced by it in the periods which you call the normal periods. History cannot help us in this decision. Many scientists seem to prefer the latter alternative—but quite obviously this decision of theirs is not binding to anybody—unless he wants to be a member of one community rather than of another. But advance of knowledge, so I would have thought, has nothing to do with membership in communities (Wittgenstein notwithstanding).

I now turn to the details. page 1, line 4: "Image of science" {SSR, p. 1, top}—this very difficult affair for science has always been at least two things, viz. an intention to find out about the universe, and a practice which is just the result of the attempt to realise the intention. A scientist wants knowledge and he thinks that this is something good and valuable. And if he knows a little, he wants to improve what little he knows, he wants to know more. He wants to conquer fear and superstition. All this is part of the "image of science". But in the attempt to carry out these intentions a practice develops. Institutions are founded with laws of their own. And it may well be that the practice deteriorates and completely gets removed from the original aim, viz. of carrying out the said intentions. The ideal which the scientist has in mind may be something extremely valuable, something which every human being should try to achieve. The practice, on the other side, may be corrupt, degenerate, and not fit to realise the ideal. Therefore we must speak of at least two things when speaking of "the image of science". History can give us only one of them and this is why every historical account of the sciences is bound to be incomplete. "The monograph" you say "is a sketch of the quite different conception of science that can emerge from the historical record of the research activity itself" {similarly in SSR, p. 1, middle: "Its aim is a sketch of the quite different concept of science that ..."}. This conception, this is my main contention, may be completely misleading as it may tell us the results of the degeneration of a valiant effort to achieve something very valuable. Clearly the attempt to know the universe may degenerate gradually and it is quite conceivable that the way in which integrals and other mathematical symbols will be used at some future time will be very similar to the way numbers were used by the number mystics (this is already partly the case). Shall we, then, regard this as the most "modern" "image" of "science"? One could of course make this assertion but it would be very clear that the words would then have lost any meaning. I repeat, therefore, that an

investigation of the whole research activity is not enough and does not tell us all about the sciences. It is much more than what can ever emerge from an investigation of "finished research achievements"—but it is still far from being enough.
page 3, line 7 {SSR, p. 2, bottom}: we must take into account also the methodological difference. That is much of Aristotelianism does not only offer a description of nature which is different of the description accepted today, or in the 17th century. This Aristotelian description is also handled in a completely different manner, according to a different methodology. I am very much against calling a discipline "unscientific" or "mythological" on account of the fact that what it says about nature is different from what we are inclined to say today. But if a discipline is built up in such a manner that any possible occurrence is bound to confirm it, i.e. if it is built up in such a manner that one can (and does) say in advance of the carrying out of tests that it will be, and must be successful, then I perceive a great difference indeed between this discipline and science today. Superstitions are that way. You cannot dispute them away and contrary evidence will be handled in a manner which finally turns it into confirming evidence. It is for this reason (and not because it makes reference to ghosts etc.) that I would like to keep it apart from today's science. Hence your assertion to the effect that "Aristotelian dynamics ... [was] neither less scientific nor more the product of human idiosyncrasy than our own" {similarly in SSR, p. 2, bottom} does not seem to me to be completely correct. Of course, it is always possible to take a theory, however dogmatic, to turn it around and to use it in such a manner that it is capable of refutation by experiment. This is what Galileo did with the Aristotelian theory of motion. But the important thing is that many Aristotelians used this theory in a completely different manner, not as something whose validity should be found out by repeated test, but as a basic theory that could not possibly be false (I always thought that the impetus theory, despite its ingenuity, was nothing but an ad hoc manoeuvre designed to save the basic axiom that any motion needs a mover).
page 4, line 3 of second paragraph: "insufficiency of methodological directives etc." {SSR, p. 3, bottom} This is completely correct. But it should perhaps be pointed out that methodological directives are necessary in order to guarantee that what has been introduced in a very arbitrary manner "compounded of personal and historical accident" (page 5, line 12) {SSR, p. 4, bottom} is not just a fairytale, but a description of properties of nature. The fact that scientific method is not sufficient for arriving at a certain belief, theory, piece of knowledge is reflected by the existence of a variety of schools and the existence of "incommensurable ways of seeing the world and of doing science in it" (page 5) {similarly in SSR, p. 4, middle}. But this variety cannot be unlimited, and the limits are set by the methodological rules which exclude some of the "ways of seeing the world" on account of the fact that they are not about the world at all, but interesting fairy tales (their "logic" is different from the "logic" of scientific theories). Also whom are you attacking on pages 4 and 5 {SSR, pp. 3–4}? Who, apart from some very radical Baconians, Cartesians and Platonists has ever believed that the correct method will lead to the one and only correct theory?

page 6, line 7: for me the passage "so long as those commitments retain an element of the arbitrary" {SSR, p. 5, bottom} is most important. Only on that condition can I swallow the "suppresses fundamental novelties".

page 7 {SSR, p. 6}: I would like to add here that a scientific revolution leads to an increase of empirical content of the theory that is being replaced. The practice of repressing novelties, the attempt to let the theory penetrate every part of nature (and of thinking) usually leads to a situation where people are psychologically unable to imagine alternatives, where procedures such as transcendental deduction will be very successful and where the impression arises that "we are always chasing our own tails in a formal system" (as Eddington has expressed it). This practice therefore finally leads to the establishment of a language which can be used for the description of every and any situation and a situation which does not fit the theory cannot even be imagined. This means, however, that this method of excluding novelties, this attempt to press nature into the boxes of the theory will gradually decrease the empirical content of the theory until it is finally almost zero. A scientific revolution which shows the limitation of the theory and which points out very plainly where it is wrong therefore gives back empirical content to the theory, i.e. makes it again a theory of nature. Conclusion: better live in permanent revolution than in the state of normalcy. Revolution in Permanence should be the battle cry of every empiricist. The 'close fitting' between theory and nature which normal science tries to achieve decreases the empirical content of the theory and thereby also decreases its ontological significance. 'Normal Science' therefore tries to achieve something which is very undesirable and its undesirable aftereffects are eliminated only by a revolution. The closer we try to get to nature (by the method of trying to fit nature into boxes designed by us) the further we get away from it.

page 9, line 4 from bottom: {"If they [distinctions like that of the context of discovery and the context of justifications] are to have more than pure abstraction as their content, that content must be discovered by observing them in application to the data they are meant to elucidate." SSR, p. 9, bottom}—good example of what I called "double talk" in my general introduction. Now you are not content with simply stating the historical fact as to how science actually proceeded—now you add a MUST, as if history could ever force us to repeat what others have done before us.

page 12, line 6{SSR, p. 12, line 7}: perhaps it would be better to leave out Planck who had nothing to do with the photon theory.

page 13, line 10: "Those men were scientists" {SSR, p. 13, top}—I am not quite clear what you want to assert here and on what grounds you want to make this assertion. Is the Timaios scientific? I do not know. But it seems to me that you make "being scientific" a fetish and a fetish, moreover, which you apply rather arbitrarily. I would like to see a little more explanation here. Surely the fact that they talked about "light" does not make them scientific. So what are your grounds for calling them thus? I have a great suspicion that it is in such phrases that you are trying to smuggle in your own ideal of what does and does not constitute a science, i.e. your own methodology and you do this not explicitly, but in an indirect way, by taking your readers in. Also the trouble of these earlier schools does not seem to me to lie in the fact that there were many of them and that people did not concentrate upon the elaboration of a single paradigm. The trouble of these earlier schools seems to me to lie in the fact that their assertions were incapable of test, that crucial experiments could therefore not be staged. Had these earlier theories been developed in greater detail such that crucial experiments were possible, then many of them could have been eliminated by tests (just as the corpuscle theory of light was eliminated by test, and just as the monism of the Ionians was eliminated by test: change does exist—to be replaced by the pluralism of the atomists, of Aristotle, of Anaxagoras etc. etc.). Not the absence of a paradigm makes these earlier researches seem too chaotic, but the absence of clear methods of test and elimination.

page 15, 16 {SSR, pp. 15–16}: electricity, the morass of Baconism: excellent!

page 17 {SSR, p. 17}: this (that is a morass of partly irrelevant facts with a minimum of theoretical coherence) is not the only possible initial stage of science. Another initial stage is just the opposite: a wealth of theories, or points of view (myths) with a minimum of fact connected with it. This second initial stage was the stage of the Ionian science (of the Ionian theory of matter of cosmology, that is). However this second initial stage seems to be much more promising and fruitful than the first. When there is a morass of fact and no theory—what can you discuss? What arguments can you produce? On the other hand when there are many theories, then one can discuss their plausibility, their ability to order facts, their comprehensiveness. Hence a wealth of theories seems to be a much more promising starting point of science (which is, after all, an intellectual enterprise) than a heap of facts.

page 19: a "new, and more rigid definition of the field" {SSR, p. 19}—yes! But remember page 6 where you said that the additional commitments leading to this more rigid definition "retain an element of the arbitrary". The only non-arbitrary elements are the methodological ones, i.e. the stipulations which demand such relatively trivial things as that the theories be testable, that ad hoc hypotheses (i.e. hypotheses for the saving of appearances which do not admit of any independent test) be avoided etc. etc.

page 20, second paragraph, line 2 {SSR, p. 20, top}: the scientist who writes popular stuff finds his reputation impaired: this does not seem to me to be quite correct especially as nowadays there cannot be drawn a sharp line between fundamental inquiry and popularisation: Bohr's articles (for example, Licht und Leben) are certainly popular—and yet they contribute to the understanding of the theories. The same is true of Atomic Theory and the Description of Nature: in these works fundamental inquiry turns out to be so far removed from technicality that it can be understood by the layman. This is a quite curious phenomenon: Bohr's investigations have always advanced the subject; but at the same time they were understandable by people who were not masters of the formalism. But even popularisation in the proper sense—i.e. the attempt to explain what is technical in non-technical terms is not frowned upon today—if it is done properly. What is frowned upon are the Jeans–Eddington excursions into theology and mysticism; but their books are bad popularisation.

page 21, second paragraph: 'history and prehistory' {SSR, p. 21, top}. Here it is very obvious that your 'history' is ideologically infected double talk. What is the criterion according to which you distinguish between 'history' and 'prehistory'? It cannot be succession in time, for a discipline may disintegrate, i.e. become worse, whereas the word 'history' suggests that it has improved since its 'prehistory'. The amount of activity, the amount of publications, the number of scientists in the society cannot be decisive either. For it may well be that science becomes influential, recognised only after it has become degenerate. Hence, when differentiating between the 'history' and the 'prehistory' you must, at least implicitly, refer to some ideal of scientific procedure by which you measure progress (or regress). If I understand you correctly the ideal is 'normal science' or pattern guided science (science guided by a single pattern which everybody accepts with the sole exception of some people you would perhaps be inclined to call cranks). But you never state clearly that this is your ideal. Quite on the contrary—you insinuate that this is what historical research teaches you. Now quite obviously you mean here historical research which concentrates upon the 'history' (as conceived by you) rather than on the 'prehistory' which makes your whole procedure circular and also present the reader with a concealed ideology rather than with clear history. But doing this you do a disservice both to history and to philosophy. You do a disservice to history because you misuse it to wrap up your own predilections in it without saying so. You falsify history just as Hegel falsified it in order to finally arrive at the Prussian state. And you make philosophy irrational by presenting the philosopher not with a doctrine, an ideal concerning knowledge which he would then be able rationally to discuss; you present him rather with a so-called fact (which is not really a fact at all but a misleading report—misleading, I believe, even to you—of your own predilections) which he simply must swallow for facts cannot be different from what they are (your own defence in a discussion with me: this is how things are you said. But the important thing here is that designating a certain period as 'history' means adding value judgement to facts!) As far as I am concerned (following here Popper and Matson) history starts with the Presocratics and this for the reason that critical discussion which we regard as the essence of knowledge, science or whatever you call it, starts with the Presocratics. [In the above paragraph I may have been a little violent—but please take this in the proper spirit. My violence is really the result of an effort to say things as clearly as possible. And also you ought to know that despite all this I find your essay very important, very stimulating].

page 23 {SSR, p. 23}: your definition of 'scientific' is much too wide; as a matter of fact it is so wide that it coincides with 'what can be said in some language'. Any language includes 'implicit laws' and hence what is excluded by your definition is something that is not a language at all. This becomes even clearer by your reference to paradigms. Any language contains implicitly a paradigm (this has been made very clear by Wittgenstein). Now it is my opinion that what adherence to scientific method amounts to is making a distinction within a given language between theories (in a very wide sense) about the world that possess certain likeable qualities and other theories which do not possess such qualities. This is what the empiricists tried to do; they were not content with just having a theory, but they wanted this theory to be related to experience in a manner that allowed for tests. Now I would be the last to argue about the meanings of words (such as, for example, the word 'scientific'). Still it seems to me that what scientists regarded as valuable were testable, and refutable theories and that they were not content with any old scheme that brought order into phenomena. The theory of witchcraft brought order into natural phenomena but in such a manner that no conceivable counter argument existed. All this shows, of course, the importance of methodological arguments in the attempt to delineate the domain of the sciences. To repeat—your explanation is much too wide, as it includes all sorts of dogmas, non empirical statements, ad hoc hypotheses, circular hypotheses and so on.

page 24 {SSR, p. 24}: completely agree with first paragraph. Second paragraph is true as a description. But I feel uneasy about the influence this description may have upon the reader, or the student of physics. Will he not be inclined to feel that he should not upset the applecart and use his strength mainly for mopping-up-operations? Historical presentations have a curious influence. They tell what is the case. But sometimes they make people feel that this is what ought to be done. And they make people feel that way especially when the writer of the history has this belief himself. Do not overlook, by the way, how very well what you call the 'normal science' fits into the pattern of the methodology of falsification. For this activity may just as well be regarded as a continued test procedure and of course the test procedure will be the stronger, the more decisive, the harder one tries to establish a fit between the theory and nature and the narrower the boxes into which one tries to fit nature. From the point of view of the methodology of falsification all this can be justified as it leads to very decisive, and very sharp tests. Exactly the same applies to the fact that initial failure of fitting is not immediately taken as a sign that the theory must be abandoned. This means that also the refuting instances are severely tested, that they are not adopted at once, but again and again investigated and are regarded as decisive failures of the theory only after many attempts to accommodate them have failed. This is a methodological justification of normal science and only after such a justification has been given, only then can we be allowed to be impressed by the phenomenon of normal science, and only then can we defend it. Otherwise our defence would be of the form: do this, because this is what everybody is doing.

page 25: 'defects which have accompanying virtues' {similarly in SSR, p. 24, middle: "Perhaps these are defects."}—here some explanation is missing: why should detailed investigation of nature be a virtue? Or, to put it in different terms: in what respect is the quantum theory (which is very detailed) a better theory than the theory of relativity (which is not so detailed)? Some people would prefer the quantum theory because it produces more facts. Some people would prefer the theory of relativity because it produces a few very simple principles for the understanding of the whole universe (and not only of some small parts of it). So what is the advantage of being detailed? I know, some people take it for granted that it is better to be detailed than to possess a Grand General Theory—but I just cannot swallow their assertions without hearing some reason for it. I can give a reason: being more detailed means providing stronger tests. And as a theory is the more valuable the more testable it is, detailed research is to be applauded. But this again shows that in order to be able to give reasons for ones predilections one has to refer to methodological considerations. You take much too much for granted!

page 25: "built in mechanism" {SSR, p. 24, bottom}—you describe science as if it were a big organism which falls asleep when it drinks too much alcohol. I.e. you describe the fact that theories are abandoned when things get complicated as a natural phenomenon. You are here even more naïve than Hesiod who was already able to distinguish between laws of nature which one cannot change, and laws of society which one can change. The laws of scientific development are a result of human decisions. Human decisions are being made in accordance with certain ideals. One can persuade people to give up certain ideals and to accept others. Thus the 'built in mechanism' is the result of the fact that people do not want a dogma which they would defend come what may, but a body of knowledge which has a chance of being refuted. However once this has been made clear, in the very same moment we have a possibility to discuss this 'built in mechanism' and perhaps even a possibility to replace it by a different mechanism. You create the impression, by your use of the phrase 'built in mechanism' that there are historical forces at work which, without any help from thinking beings bring about the downfall of a theory once too many counter instances have accumulated. Or that there is a law of human behaviour according to which faithful acceptance of a theory can be shaken by accumulation of counter instances. I do not believe in the existence of any such law. Quite on the contrary I believe that there may exist points of view where accumulation of counter instances is regarded proof to the fallibility of human nature and of its incapability to understand the one, unchangeable and absolutely true theory of which the cases discussed are apparently counter instances.

page 26 {SSR, p. 26, top}: As regards E. O. Lawrence I have heard a rumour to the effect that theoreticians did not believe in the possibility of a machine as Lawrence set out to build. It would be interesting to know whether this rumour is correct. [What follows is excellent and I completely agree.]

page 35: "Deserting the science it defines" {similar in SSR, p. 34, middle: "Work under the paradigm can be conducted in no other way, and to desert the paradigm is to cease practising the science it defines"}—this reminds me of Mach's reply to Planck on the occasion of the latter's having said 'no scientist would do what Mach suggests'. Mach said 'Scientists have now become a church and I do not regard it as an honour to be a member of this or of any church.'

page 41 {SSR, p. 42, bottom}: concerning rules: I think the matter is very simple. Consider the case of chess. No set of rules is here sufficient to define the many special problems which a chess player may encounter, and try to solve, in the course of a game. These special problems arise from the special way in which certain players start out and they are therefore due, to a large degree, to the individual predilections of the participants of the game. However despite this failure of any set of rules to define the 'puzzles' that chessplayers are confronted with there do exist rules which must be followed if one wants to play chess. Only these rules are not detailed enough to explain everything that might arise in the course of the game. They leave considerable leeway. They are mainly negative rules, excluding certain moves, but leaving many other possibilities open.

Now in science, I think, the situation is exactly the same. No number of rules ever 'seems sufficient to define the puzzles that scientists normally undertake, or to restrict scientific attention to their pursuit' (page 41) {similarly in SSR, p. 42, bottom: "those rules may not by themselves specify all that the practice of those specialists has in common"}. Just as in the case of chess it does not follow at all from this that science is not played according to certain rules. Although there is no set of rules capable of explaining every move that is being made, there are rules which definitely forbid that certain moves are made. For example they forbid the use of ad hoc hypotheses, i.e. of hypotheses used for the adaption of a troublesome case to the paradigm which are framed in such a manner that independent tests are not possible (independent, that is, of the fact that they turn the troublesome case into a case that is not so troublesome). They also forbid dogmatic moves, i.e. moves which decrease the testability of a given hypothesis; and so on and so on. In short, the rules according to which the game of science is played are simply the rules of scientific method. To sum up: although there are no rules which explain and define each and every part of the scientific activity it does not follow that science is not played according to rules.

page 44, bottom {SSR, pp. 50–51}: very good (the various traditions which can exist inside the quantum theory).

page 45 {SSR, p. 52, top}: it seems to me to be very important to point out, as you do, that there does exist accumulation, viz. during what you call 'normal science'. It also exists in what you call the prescientific stage, if this prescientific stage is guided by an empiristic methodology.

page 46: "recognition that nature has somehow violated ..." {SSR, p. 52, bottom}—this recognition can take place only if one has first adopted the attitude that paradigms should be testable, and that they are therefore fallible. Churchdogma, for example, will not be treated that way. Any difficulty that arises is in this case regarded as an indication of insufficient understanding rather than of failure of the theory. Even Ptolemaic astronomy could have been saved in the face of any possible difficulty relying, as it did, upon the method of Fourier analysis (which will enable the

astronomer to analyse any motion, even non periodic motions, in terms of motions around a fictitious center with constant angular velocity).
page 49: the reference to the "questionable" account of seeing {SSR, p. 55, middle}. I agree that there is a complete parallel: when seeing some object one may perceive the outlines and not the object—and even the outlines may appear as a rather scattered affair without much coherence. Next step: the outlines are perceived as a whole, but the object is not yet perceived. Next step: the object is perceived, but not as something that is known. Next step: the object is perceived as something known. What seems to be important even here is that the switch from one attitude to the next may be influenced by one's belief that one is dealing, in the world, only with objects of a certain kind, and with no other objects (dogmatism). In this case certain appearances will look as if they were not of real things, but illusions. Hence even the 'perception of anomaly' (page 51, bottom) {SSR, p. 57, bottom} will be method-dependent. [I believe that people in the 15th century saw ghosts with the same intensity as they saw real things—the phenomenal character was the same. This may have changed today. Users of Mescaline report that they are fully aware of the unreal character of what they perceive, not only mentally, but even in the perception itself. I bet that somebody of the 15th century who believed in the devil would have seen the phenomena occurring on the occasion of swallowing mescaline in a very different manner, namely as real occurrences provided he did not know of the effect of drugs.]
page 53 {SSR, p. 59}: I think one must be careful not to interpret any reluctance as indicative of the existence of either a paradigm, or of a part of a paradigm that is being defended. One must take into account that sometimes people are simply unreasonable.
page 58 {SSR, p. 62, middle}: the 'awareness of anomaly' does not seem to me to precede either the special, or the general theory of relativity. The constancy of the velocity of light in all coordinate systems was considered by Einstein when he was 16 and knew neither the investigations of Lorentz, nor the Michelson–Morley experiment. Taking this together with the principle of relativity we obtain at once special relativity and, including the elevator, general relativity (principle of equivalence). Of course, Einstein was aware of the breakdown of the classical point of view in the field of radiation. But what he was looking for was not a new theory to replace the old theory of radiation but a point of view that was general enough to survive the collapse of more specific theories (he refers to thermodynamics in this connection).
page 59: "Without the special apparatus that is constructed only for anticipated functions the results that lead ultimately to novelty could not have occurred" {similarly in SSR, p. 65}—I think this is simply false and it is false for a reason to which you, too, would agree. The reason is that sometimes, and in very interesting cases at that, a test of a theory T can be carried out only with the help of an alternative theory T′ which agrees with T where it has been successful, but is incompatible with it outside this domain. A very good example is provided by the classical thermodynamics. We know now that the second law is not strictly valid; there are

perpetua mobilia of the second kind, and the Brownian particle is one of them. However could this fact have been discovered in a direct way, i.e. by simply sticking to the phenomenological theory and trying to make it fit reality more and more closely? I.e. could anomalies have been brought about in this way? Definitely not. Consider the physical situation: in order to discover in a direct way that the Brownian particle moves by absorbing heat from the surrounding medium one would have to be able to measure the heat disappearing from the medium, the changes of kinetic energy of the particle, the resistance overcome in the fluid (viscosity) and one would have to show that the heat lost by the medium balanced the gain of kinetic energy of the particle. It is quite obvious that the measurements involved in this procedure cannot be carried out as the thermometer would be itself involved in the fluctuations of temperature to be measured. Elaboration of the paradigm of phenomenological thermodynamics therefore will never be able to discover the anomaly presented by the existence of the Brownian particle. But this anomaly was discovered. It was discovered by the elaboration of an alternative account, viz. of the kinetic theory which then produced predictions that could be tested by experiment. Now I think that this is not just an isolated case. I think that very often anomalies can be discovered only in this way; i.e. not by further and further elaborating a given paradigm, but by elaborating an alternative paradigm and producing with its help testable predictions which, if they are confirmed, show that the first paradigm is in trouble. Your insistence upon faithfulness to one and only one paradigm is bound to result in the elimination of otherwise very important tests and it is bound in this way to reduce the empirical content of the paradigm you want to be accepted. It may well be—and Bohm and Vigier are definitely of this opinion—that the situation is the same in the present quantum theory. The 'orthodox' refuse considering alternatives and their argument is that the present point of view has not yet encountered anomalies which would necessitate reconsideration of it in its entirety. Bohm points out that the limitations of the present point of view will become evident only if one has first introduced an alternative and shown that it is preferable. Hence if the absence of limitations is taken as a reason for not considering alternatives, then trouble will never be discovered, simply because it could be discovered only with the help of alternatives. This, then, would make the present quantum theory a wonderful metaphysics.

page 70: "historical setting" of the theory of relativity {SSR, p. 74, bottom}—cf. what I said above in the remark about page 58.

page 72: "Invention of alternatives is just what scientists do not, and probably ought not undertake" {similarly in SSR, p. 76, middle}—ought not? See above. You say 'probably'—what are your reasons? Also the situation is not quite so simple. In some sense Maxwell's electrodynamics is really an alternative to the Newtonian point of view (contact action as opposed to action at a distance). Should it perhaps not have been developed? Should the wave theory of light not have been developed which is also a theory working with contact action. Should thermodynamics not have been developed which is irreversible whereas Newton's theory is reversible? All these theories: the wave theory of light; the electrodynamics of Maxwell; the phenomenological thermodynamics are alternatives to Newton's theory which were developed even before that theory had got into trouble. They were developed by articulation of a very different paradigm (Faraday!) and only after they had been developed, only then did it turn out that they meant trouble for Newton: reversibility objection; recurrence objection; the different transformation properties of Newton's mechanics (Galilei transformation) and of Maxwell's theory (Lorentz transformation). This shows what I have said above: that the shortcomings of a paradigm very often can be demonstrated only by comparing it with other paradigm cases; and that this also takes place in history. The whole development of relativity is the attempt to resolve a clash between two already existing paradigms rather than the attempt to make sense out of the isolated breakdown of one paradigm. This is true both of the special theory (the two paradigms are here Newton's theory and Maxwell's electrodynamics) and of general relativity (the paradigms are here either special relativity and Euclidian geometry [rotating disk]; or special relativity and Newton's theory of gravitation [action at a distance forbidden by special relativity]). From all this it follows only only that alternatives should be considered as they are necessary to accelerate the breakdown of whatever paradigm is in the center of attention; it also follows that they have been considered in the history of thought.

This result is, of course, very decisive for the evaluation of the last chapter. What is the result: alternatives are both used, and needed; and they are needed as it is only with their help that it is possible to find anomalies in whatever theory is being held at a special moment.

page 73: "what scientists never do" {SSR, p. 77, top}—of course, they continue playing around with the theory they have. This is quite correct. For without this theory they would have nothing to do. However one thing must be changed and this quite irrespective of whether the scientists as a matter of fact carry out, or do not carry out the change: the theory cannot any more be regarded as a description of reality. Or at least it must now be regarded as questionable whether it is a correct representation of reality. It can now only be used as an instrument for prediction—but this is quite alright, for even false theories may be good instruments of prediction in certain domains.

However as I have pointed out above the situation is not always as bad as that. Very often the downfall of a theory is brought about by comparing it and its results with an alternative and if the comparison is against the theory then the alternative can take over at once.

Now as regards the "methodological stereotype of falsification" {SSR, p. 77, middle} I have to say two things. First, that history is irrelevant to methodology. And secondly, that the history of the sciences supports, rather that is opposed to, that "stereotype".

History is an irrelevant judge: How do we arrive at the "stereotype"? We arrive at it in somewhat the following manner: we compare testable theories with non-testable theories (dogmas) i.e. with theories which from their very nature can never get into trouble when being compared with experience. This comparison is, of course, motivated by the existence of dogmas and by the existence of theories that are capable of empirical test. However what is important is that the result of the comparison and the arguments leading up to that result have nothing to do with history. What are the arguments? The arguments are very simple, almost trivial. We want our theories to be about the world, we want them to have factual relevance. A dogmatic theory does not have factual relevance. Hence we cannot use non-falsifiable theories. The thing is, of course, not quite as simple as that (which is shown by the discussion of the synthetic apriori). However you will understand what I mean—as a matter of fact I remember that you were prepared, at some time, to accept the argument in the simple form in which I have presented it above. Now look at the argument. Do historical considerations occur in it? Not at all. And would it be relevant to object to the result by pointing out that science does contain dogmatic elements? It would not. Quite on the contrary—the brief argument given above would now give us grounds for criticising whatever dogmatic elements exist in the sciences. And the criticism would simply consist in the remark that a theory is the less about the world, the smaller its degree of testability. On the other hand the demand for factual reference leads at once to the demand to use theories that possess a high degree of testability and this quite irrespective of what theories are actually being used. Note that the very same demand implies that alternatives should be considered. The reason which I have outlined above is that the consideration of alternatives provides additional tests and thereby increases the factual relevance and the factual content of the theory. I regard this as a very important result. I can give reasons why alternatives should be considered. You apparently cannot give reasons for the opposite position held by yourself. For you either simply report what has happened, or, when you turn to methodological considerations, you use quite frequently words such as "probably", "in all likelihood" which show, at least to me, that you really do not know what to say. But the situation is even worse. Your hidden predilection for monism (for one paradigm) leads you to a false report of historical events. You regard as one paradigm (classical physics, for example) which is in fact a bundle of alternatives (contact action: Maxwell vs action at a distance: Newton; reversibility: Newton vs irreversibility: Clausius; Galilei transformation: Newton vs Lorentz invariance: Maxwell). Which only confirms what I have said on the first page, viz. that you do not write history plain and simple, but that you present an ideology, and a very questionable monolithic ideology at that, in the covers of history. In this respect you are really very similar to those who point to history in order to justify their crimes. You are a mystic, an irrationalist. And by this I mean that you not only hold certain beliefs (conservative character of normal science) but that you are not prepared to let these beliefs speak for themselves; you rather present them in a manner which suggests that they are facts and thereby force people to swallow them without criticising them. What are you afraid of? Are you afraid that people will oppose at once when your beliefs are presented to them in their proper form, viz. as demands as to how science ought to be run? When discussing such demands you are very careful and give the appearance of a critical person ('probably' etc. etc.). But this is just a trick (of which you yourself may not be aware). For you present the very same demands a little later as facts ('what scientists never do' etc. etc.) and then with the assurance of the historian who knows. Again, it is this kind of double-talk to which I object most. You really are like a witch doctor. That is, I do not object to your findings, but to the manner in which you represent them—as if they were (α) undisputable [which they are not—see above about the many alternatives contained in the classical point of view] and (β) unescapable; neither of which is the case.

However history is not only irrelevant, you have also interpreted your findings in an incorrect manner when suggesting that they refute the "stereotype of falsification". What does the stereotype say? It does not say that a theory that has been refuted cannot be used as an instrument of prediction. Quite on the contrary, the stereotype admits that false theories may be very valuable instruments for prediction. It only insists that it be realised that they are false, i.e. that they are not more than just an instrument. Nor does the stereotype assert that any theory that is in trouble must be regarded as refuted. Quite on the contrary it demands that the falsifying instance (which is described by a lower-level hypothesis) be treated just as critically as is the theory. I.e. it demands that the possibility of a fault of experimentation or calculation be not dismissed too readily. After all, the stereotype applies the demand for falsifiability and thereby the demand for the application of critical tests to all statements alike, and not only to theories. Only when it is agreed that the counterinstances cannot be dealt with in any other way, only when it becomes clear that we have found a genuine counterinstance, only when the statement expressing the counterinstance has been tested to such a degree that it may be regarded as a highly confirmed hypothesis, only then falsification sets in and this means that the theory must now be regarded as false and that it must no longer be used as a means of representing the world. It must no longer be interpreted realistically. Which, as I said above, does not exclude its use in the building of instruments, for prediction etc. etc. To sum up: the fact that theories are not given up the moment some difficulty arises does not at all show that scientific practice does not conform to the stereotype of falsification. Quite on the contrary, immediate abandonment of a theory as soon as the first difficulty is perceived would mean that an uncritical attitude is adopted with respect to the test statement itself which a falsificationist would never allow. I conclude, then, that your statement to the effect that "no process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature" {SSR, p. 77, middle} is both false (second point above), and irrelevant (first point above). You will really have to change your attitude on this or else I am looking forward to writing a blistering review.

This immediately applies to your page 74 {SSR, p. 78, top} where you say that as long as a new paradigm is not available scientists will try to patch up the old paradigm by ad hoc procedures etc. etc. If that were really the case science would

be in a bad spot. If that were really the case you should at once abandon science or try to change it rather than make propaganda for it. After all, you would not really like to accept something like witchcraft (although sometimes I am not quite sure about you in this respect; yes sometimes I am afraid that in 1500 you would have been one of the foremost defenders, on historical grounds, of Sprenger and Co. You are too much impressed by the existence of powerful tradition. Where there is such a tradition you at once suspect there is reason too and you object to anybody who tries to start completely afresh). But what difference does it make that the modern witchcraft is being called 'science'? After all, a name does not make the difference. Nor does the existence of institutions, of journals, of instruments, of technicalities. What distinguishes reason from folly is the kind of procedure adopted: are difficulties taken seriously? Is it admitted that the theory may, after all, be false? Or are they only taken as an incentive to invent ingenious ad hoc hypotheses? Hence, if your description were really correct, if it were really correct that scientists devise ad hoc hypothesis as long as a successful, or a better alternative is not available [a presumption which, as I have tried to show above is hardly ever fulfilled—alternatives do always exist and are considered!], if that were really the case, then to hell with science. But I do not believe that your historical description is correct. What is an ad hoc hypothesis? It is a hypothesis whose only confirming instances is what otherwise would have to be regarded as a failure of the theory. Such hypotheses are never framed by scientists. Consider Copernicus's argument, as against those who attacked his theory on account of the absence of a parallax: the stars are too far away for a parallax to be noticeable. If this hypothesis were ad hoc in the true sense then the absence of the parallax would have to be taken as the only measure for the distance of the stars. But in cases such as the one mentioned one immediately tries to find independent confirmation, independent, that is, of the occasion for which the hypothesis has been invented.
page 75 {SSR, p. 79, top}: I quite agree: there is never anything like research without a paradigm. This is the reason why one should always cultivate alternatives in order to be able both to drop a falsified theory, and to continue realistic research into the properties of the universe.
page 76: "either no scientific theory ever confronts a counterinstance, or all such theories confront counterinstances at all time" {SSR, p. 80, top}—this is patently false. What do you mean by a counterinstance? A difficulty? An inconsistency between a theory and an experimental statement? In this case the second part of your statement would be correct: there exist always experimental statements which seem to be inconsistent with some theory. However such a situation is just the starting point. For we want to know who fares worse in this inconsistency, the theory, or the experimental result. And we also want to know whether the appearance of inconsistency will remain on closer examination. This means, of course, that we must test the apparent counterinstances. Now if by a counterinstance we mean a falsifying fact whose accurateness has been definitely established by many careful tests, then it may well be that at a given time no counterinstance in this sense exists for a given

theory although this does not exclude that a counterinstance in this strict sense may be discovered tomorrow. Now it seems to me sensible to mean by a counterinstance not just anything that seems to run counter the theory (just as Berkeley thought that the fact that plants grow upwards refutes Newton's theory of gravitation) but a low level hypothesis that has been carefully tested and highly confirmed. But if we mean this by counterinstance, then your surprising dictum is quite obviously false.
page 77: "It is a poor carpenter who blames his tools" {SSR, p. 80, middle}—sounds great, especially in view of the slightly sentimental and disciplinary note it carries. But I would like to add that it is even a poorer carpenter who works with crooked nails and blames himself for not achieving a good job. And it is also a poor carpenter who uses only one type of nails pointing out (wrongly) that this is what every carpenter does and adding that one cannot do a good job if one shifts back and forth from one type of nail to a different type of nail. "Failure to achieve the solution discredits only the scientist, and not the theory" {SSR, p. 80, middle}—who says so? You? the community of scientists? Assume the latter does. You refer to those who tried to change Newton's law of gravitation and you point out that their efforts were completely neglected. And quite unjustly so we must say today. For Newton's law needs revision (general relativity). Now non Euclidian geometries could well have been invented much earlier. As a matter of fact they were (Saccheri) only people did not realise it—they regarded the implausible and counterintuitive character of the theorems they had derived from their non Euclidian assumptions as a refutation of non Euclidian geometries [Euclides ab omni naevo vindicatus]. A nominalist would not have drawn such a conclusion. A nominalist could have invented non Euclidian geometries, and perhaps even applied them to problems of physics. This would have led to a better theory than was Newton's theory. So the people who tried to change Newton's law were on the right track—a change was necessary—only they had the wrong ideas about what kind of change should be brought about. You seem to take a completely different point of view. You seem to say that what I say is completely unrealistic. Now this I readily admit: the real, the actual development went along different lines. Newton's theory was only abandoned after a lot of trouble had arrived [by the way your insistence that the trouble must be big enough seems to be circular; there is always great trouble and maybe that in the beginning there was greater trouble than there was at any time later on; the case of the moon equation could have been hardly worse; by "great trouble" you seem to mean "trouble that leads to the search for a new paradigm"—now this is purely psychological; people may look for a new paradigm on the slightest pretext, and they may on some other occasion stick to a theory much longer than would seem to be reasonable; this all depends upon the climate of the time, upon the character of those carrying the main burden of the research; there is no fixable amount of trouble after which a theory should be abandoned and before which it should not be abandoned. The fact is that it very often is abandoned when some trouble arises and only in this sense the trouble can be called big enough; recall the story of the lethal illness: it is an illness which is grave enough

to end in death]. But the truth is that it should, and could have been abandoned much earlier. The same is true of Aristarch's theory versus Ptolemy's. It is a historical accident rather than a fact giving us insight into the nature of the scientific enterprise that it was the geocentric theory that was elaborated in sufficient detail to become of practical value and that it was this theory which therefore gained great popularity. At some place you seem to insinuate (I have forgotten the passage) that this was a much more natural way to start. That is you not only say that this is how things went, but that it was better that they went that way. I do not see any reason in this. Had the heliocentric theory been elaborated in Greece we would perhaps be farther now than we actually are. It was not. Why? Because of Aristotle's triumph over Democritos. And why did Aristotle triumph? Well, this is again one of the historical accidents. Perhaps he triumphed because Plato succeeded in making the atomists look mean fellows who strived after lust instead of the higher things. In sum: it seems to me that you do not tell history as what it is: a series of accidents combined with struggle for power etc. etc. You perceive some inherent reason in it. But in order to perceive inherent reason in a series of accidents one must distort reason itself.
And this, I am afraid, is what you are doing.
page 78 {SSR, p. 82, top}: character of the anomaly that evokes the crisis. This is what I have just been talking about. How does a crisis come about? Of course there must be some reason to worry. But do not forget that a crisis very much depends on the character of those participating in it. If they have great influence, and if their voice is heard, and if they are conservative, then the crisis may be postponed simply on their authority. Assume Einstein had not invented the photon hypothesis. Planck was conservative. Nobody could have really said what would have happened. There have been attempts of something like a photon hypothesis before. Not much changed between 1900 and 1905—only Einstein was persuasive enough. Now if a conservative authority dies, then the crisis may come in full blast—simply because some personal influence, some very irrational thing, does not any longer exist. It is not simply a matter of the kind of anomaly arising. Some people may make a mountain out of a molehill and they may succeed in persuading their contemporaries that there is indeed a mountain. Hence, all you can say is that at some point people feel that something must be done. But this feeling of theirs can never be justified by reference to an objective crisis-situation alone (degree of misfit between theory and facts; number of counter instances). Conversely you can not lay down a rule to the effect: do not consider abandoning a theory when the counter evidence is slight—and try to justify this rule by reference to history. For what happens in history is that people sometimes exaggerate and create a crisis that way, especially when they are influential. Your attitude that "there is probably (again your probably) no fully general answer to the question as to what makes an anomaly seem worth concerted scrutiny" {similarly in SSR, p. 82, middle} is quite correct, but you do not seem to see the reason why it is correct: it is correct because people turn their attention to an anomaly sometimes not because of the anomaly, but because of somebody else who says that the anomaly

is worth considering, that it is decisive. Hence there is no inherent property of an anomaly which makes it create a crisis. But you make it look as if there were. You make it look as if the great men who consider the anomaly have first studied it and now have reason to consider it decisive.

I think I shall now finish*. I am looking forward to your reaction.

Paul Feyerabend's Second Letter to Thomas Kuhn

Dear Tom,

eccola la lettera promessa—I am also enclosing one of my opera magna for your scrutiny and I would like you to return it to me together with the letter, as it is one of two copies which I have left and this, you will admit, is very little. The thing is going to be published by the University of Pittsburgh Press in January {of 1962, see the introduction to the first letter}. If you should discover some scandalous inaccuracy I shall still have an opportunity to correct it (I am expecting the proofs to arrive in November, or some such time).

Returning to your own opus I would first like to repeat, briefly, two points which to me seem to be important and which you conceded: First, that a revolution is brought about not only by the failure to fit nature into the categories of one paradigm, it is brought about also by the further development of the paradigm at hand which development may gather a momentum of its own and turn into an entirely different, and incompatible paradigm. There is not only revolution from 'below' (i.e. from confrontation with 'nature') but also from 'above' (i.e. by confrontation with ideas which have developed out of the original paradigm). By the way, it still seems very paradoxical to me that you allow for such deviations, i.e. that you allow for deviations which are brought about unintentionally (deviations, that is, from the original paradigm) whereas you frown upon the explicit development of alternatives. What is your reason for this position of yours (and a reason you must give!) i.e. that alternatives to the paradigm which are unintentional side effects of an attempt to develop the paradigm are to be welcomed whereas alternatives which are the result of an explicit effort to look for something different are not so good. The end effect will be exactly the same: there will be a new paradigm. However in the one case this new paradigm is the result of a mistake, as it were, whereas in the other case it is the result of conscious and rational planning. You prefer it to be the result of a mistake whereas it seems to me that of course the rational procedure is to be preferred by far—unless you provide some very weighty arguments in your favour. I am waiting for these arguments, also for my benefit. As an aside I may tell you that I believe that here again your historicism has got the better of you. You believe in blind historical development. Consciously inventing a new theory means disturbing this develop-

*tired

ment, which is something very deep, by bringing in something very superficial, viz. human reasoning (so fragile when compared with the forces of history!) Anyway, please answer me this question.

The second repetition is this: three (and not only two) things are needed in order that a certain gestalt switch be brought about: (a) nature (b) a paradigm (c) a certain attitude with respect to paradigms in general or what I call a methodological attitude (dogmatism: the paradigm cannot possibly go; if trouble arises then this shows that mankind is rotten and perhaps needs some help from the inquisition; non dogmatic: a paradigm is something which, if need arises, very pressing need, has to be replaced by a different paradigm and can be replaced as it is not an absolute truth). This is important. For you suggest that there is only one procedure which will be adopted when the paradigm comes into trouble: to look for a new paradigm; and that this is the procedure which will be adopted by all those who are interested in the paradigm. Not so. Not so. An alternative would be to admit the sinfulness of human nature and to attempt to purge it from sin. Another alternative (which is accepted by some primitive tribes, for example the Zande) would be to point out that this was obviously a year where demons were very active and made understanding impossible. etc. etc. Only if it has first been admitted, either explicitly, or implicitly, that a paradigm is something that may be in need of replacement and not something that lasts forever, only then will the reaction which you describe be the normal one. This is how historical development is guided by methodological beliefs and attitudes.

My third repetition (which is very brief, and which I did not foresee when I started the letter) is a reference to the problem of tautological degeneration of your thesis that "great trouble leads to a search for a new paradigm". From one point of view the problem of the moon equation in Newton's time was very troublesome. However it did not lead to the search for a new paradigm. Will you then call trouble "great" only if it leads to search for a new paradigm? But then your thesis that "a new paradigm is looked for only if great trouble has arisen" is a tautology.

Now for the details of the second part of your essay.

[1] Remember my reservations concerning your comparing political revolutions with scientific revolutions. The most fundamental revolution, to me, in the domain of knowledge, would be the transition from a stage of dogmatism to a stage where replacement of any paradigm is possible (although, as you have pointed out, one will be very careful not to give up the old paradigm too quickly and too lightheartedly). Now it seems to me that political revolutions are more closely related to this fundamental revolution than to changes of paradigms about nature. That is they are more closely related to attitudes concerning any possible paradigm (methodological attitudes, as I would call them) than to the change of attitude with respect to one particular paradigm. This is just a guess on my part, and it may be a mistake.

By the way, it is quite interesting to note what application of your attitude means in the field of politics. In the sciences you advocate considerations of alternatives only if the existing paradigm is in real trouble (again, you must explain what real trouble is. For the moon equation was real trouble—what difficulty could be greater that a factor of two! And yet you say that physicists were right not to give up Newton's theory, or at least, not to look for something better implying, of course, that this trouble was not real enough. Is this not wisdom from hindsight? And how can such wisdom from hindsight guide the scientist who does not yet possess the hindsight because he has not yet made up his mind as to how to proceed? That is what use can your historical evaluation be to him if this historical evaluation, based upon historical fact, can exist only after he has made up his own mind?) But back to politics: in the domain of politics your rule not to consider alternatives unless the existent paradigm has got into trouble would mean that nobody should consider alternatives to a tyranny until it has become very obvious that it cannot work; this means revisions, or democratic ideas should be considered only after quite a lot of people have been killed and only after it has become apparent that the tyrant cannot keep his power a moment longer. Only then the democrats are allowed to come forth and to suggest different means of ruling the people. That is, they are supposed to be cowards, they are not supposed by conscious propagation of their ideas to contribute to the downfall of tyranny and the philosophy connected with it (like racialism, or the idea that there are people chosen to rule by destiny, and others who are chosen to obey); they are supposed to wait until others have made life difficult for the tyrant, or until economic circumstances have weakened the rule of the tyrant etc. etc. and only then may they come forth and preach the creed of democracy. Now let us apply this immediately to the domain of the sciences. You see: preaching the creed of democracy may just be one of the things which lead into difficulties for the tyrant. And suggesting an alternative paradigm may just be one of the things which leads into difficulties for the existing paradigm (for it may turn out to be much more attractive, much more simple, and much better able to cope with problems than the existing paradigm). You propagate ceaseless efforts to fit nature into categories of one paradigm because it is in this way the crises will be brought about. You therefore implicitly assume that everything that leads to a fullfledged crisis (and not only to a temporary inconvenience) is to be welcomed. Now if that is so then consideration of paradigms which are alternatives to the paradigm in existence is to be welcomed too, for it too may lead to crises (this you have admitted yourself, however you have admitted it only in connection with paradigms which are the unintended results of an attempt to further elaborate the existing paradigm. The above argument is quite general and I do not see why it should not be made general in this way: after all the fact that Maxwell used all the little wheels is a historical accident. Electrodynamics might well have been invented on the basis of a completely different metaphysics, on the basis of a metaphysics of 'waving' spirits such as was the metaphysics of Henry More. I conclude, then, that if you welcome acceleration of the development towards crises you must also welcome consideration of alternative paradigms which, as you admit yourself, may lead to crises. Only, you admit this only for paradigms which have been invented in a very particular manner, i.e. on the basis of a mistake, as it were: they were intended to be further applications of the existing paradigm, and they turned out to be rivals of that paradigm. I do not see any reason for that restriction and I would be very glad if you could give me one, a single good reason. If you don't I take it you admit that consideration of alternatives is not the anathema you want it to be but, on your own premises, one of the means to make the crises (which you want) occur sooner, or perhaps to lead to a stage which is a crisis in permanence.

[2] page 90 {SSR, pp. 93–94}: when the country is divided into different camps then political recourse fails only if being divided into camps is something quite out of the ordinary. If it is not, as is the case in a democracy (many-party-systems) then there exist political institutions which have been designed with the express purpose of dealing with such a situation of polarization. One may even say that the battlecry of a democracy is 'polarization in permanence'.

Now if it is correct, on the other hand, that alternative paradigms are needed in order to accelerate the development that in the end leads to scientific revolutions, then there must exist scientific institutions which are capable of dealing with polarizations that may arise in the scientific community and which prevent these polarizations to become "incompatible modes of community life" {SSR, p. 94, top}. A theory of such institutions is as yet missing. All existing philosophies of science (yours included!) are monistic in that they deal with what happens when one paradigm resigns supreme.* They are in that sense also non democratic. (This even applies to Popper!) And that your point of view is not democratic, and possesses an element of dogmatism can be seen most clearly from the fact that failure of a paradigm, as you describe it (a whole world breaks down; the activity of the whole group undergoes a most acute crisis etc. etc.) is seen as a dogmatist would see it: when his absolute truth breaks down, this means, to him, the end of his world. Only for a dogmatist can breakdown of a paradigm have such tremendous consequences, as you say breakdown of a paradigm has. For only he is not accustomed to the view that anything he believes in may at some time fail and that he never possesses the absolute truth. Now it is very well possible that scientists working in a certain tradition do tend to become dogmatists. This may be seen, very clearly, from the fact, that they sometimes use methods such as transcendental deduction etc. for defending their point of view. But this is not necessary—and it is not even true of all scientists. Alternatives are considered, and should be considered. (By the way, another difficulty of Newton's mechanics, besides the empirical one and those created by the existence of alternatives such as Maxwell's electrodynamics and the phenomenological thermodynamics is the lack of conceptual clarity of, say, the notion of an inertial system. This leads to publications such as Lange's, Neumann's (the older Neumann who was no 'von') and Hertz's as well as to Boltzmann's defence of the traditional point of view. This was dissolution from within comparable to the dissolution of the older set theory (Cantor) through the discovery of the paradoxes). We have, therefore, at least three clearly

*You only say that if there are more paradigms, then there will be a mess.

distinguishable causes for crisis concerning a given paradigm: (1) failure to fit nature into its categories, which I call, positivistically, 'revolution from below'; (2) inconsistency with successful alternatives that have been developed, either with the intention of elaborating the main paradigm considered at a given time (this is the case you admit), or on the basis of a completely different metaphysics that has been sleeping for some time (this is the case you want to exclude—with no arguments to support you, not even history)—this I shall call 'revolution from above' (as it involves ideas which, positivistically, may be placed 'above' nature); (3) internal unclarities such as are discussed by Berkeley (with respect to Newton's idea of absolute space and with respect to his calculus of fluxions), Mach, Lange, Neumann (all these about the role of inertial systems and the method to ascertain one; also the real meaning of the law of inertia was involved here), Hertz (eliminating forces), Boltzmann (trying to represent the "orthodox" point of view, as he calls it himself, as clearly as possible in order to show, thereby, that it is not in difficulty as Hertz has assumed), Voss (who in his Encyclopaedia article reports about all these difficulties without being able to resolve them), Ostwald (who thinks than the difficulties are due to the metaphysical character of mechanics)—and which may lead to a 'revolution from within' or a 'crisis from within'. It seems to me that every crisis contains all these three elements: ill fitting with nature, inconsistency with successful alternatives, and internal inconsistency or unclarity, though in different degrees. The purest case of a revolution from above is perhaps special relativity; the purest case of a revolution from within is the development of modern set theory; the purest case of a revolution from below—well I am completely unable to think of such a case which in your presentation seems to play such a great, and almost the only role.

[3] p. 91: "pure logic" {similarly in SSR, p. 94, but without "pure"}—I have explained to you my misgivings about that phrase. To me your intention is beyond doubt. But some readers may be misguided into thinking that you attack a theory much more primitive and ridiculous than the theory you intend to attack. Other places where reference to logic occurs in that misleading manner are: pp. 92,

[4] p. 91: "techniques of persuasive argumentation" {SSR, p. 94}—it is here that reference to method comes in.

[5] p. 93: "cumulation of unanticipated novelties proves to be an almost non existent exception to the rule of scientific development" {very similarly in SSR, p. 96}—excellent! Popper says, in his lectures, that discovery is always intimately connected with refutation or, as you would say, with the changes of paradigm brought about by a crisis.

By the way, development by cumulation—(and without much revolution)—is this not what happens today in many parts of psychology and sociology where we have papers with plenty of data and with no apparent purpose. One is here almost reminded of the character of some of the early publications of the Royal Society where anything is taken up, checked empirically, whether it is not interesting, or whether it is not.

On p. 94 you are exactly Popper when you say "Unanticipated novelty, the new discovery, can emerge only to the extent that his anticipations about nature and his instruments prove wrong" {SSR, p. 96, bottom}. This I do not say in order to make priority claims, but in order to show to you who your friends are (or, to be more precise, who the friends are of your better self).

This applies to the whole of page 94 {SSR, pp. 96–97}—and it is for exactly these reasons that alternative paradigms should be considered all the time, and not only when there is a mess. Their consideration will accelerate the appearance of the mess, but also of its resolution. If you start looking for alternatives only after a mess has arisen, then it will most likely take you a long time to discover them and thereby to establish a new order. If you have them present all the time and use them as instruments for the downfall of one of the paradigms used, then they may be used at once for bringing about order. The more I think and the more I write, the more reasons I find why scientists should consider alternatives at any time, and not only in a crisis. I have even found now a reason why it seems to you to be otherwise. Let me explain it: you say that historical research has taught you two things: (1) that normal science is science which is carried through according to a paradigm, and to a single paradigm and which refuses, and to your mind correctly so, to consider alternatives; and (2) crises arise when this paradigm is found to be in trouble when confronted with nature and not only at one place, but at many places. It is then that alternatives are considered, and legitimately considered. This is your original thesis, the thesis of your paper. Now in the meantime you have admitted that crises may be created, and have been created, not only by absence of fit between nature and the paradigm, but by the existence of alternatives, which are very successful, and which are yet inconsistent with the main paradigm (if it can still be called the main paradigm). Alternatives are therefore desirable because they lead to crisis (you do say that crises are desirable as they lead to interesting discoveries; hence anything that leads to them is desirable). But what happens now to "normal science". Normal science (if that period exists at all and if the truth is not rather "crisis in permanence") is a period where nobody uses this particular means of precipitating crises and the reason for this may be lack of ideas, or the fact that the initial success of the main paradigm has dazzled people to such an extent that they cannot think of anything else. The latter thing, however, is far from desirable and so it may well be that the absence, from "normal science", of alternatives, is far from desirable, too. Hence, "normal science" is far from desirable, too. You say, it has inbuilt guarantees to the effect that it will be upset sooner or later. Let me first point out that one of them is the non-dogmatic attitude of the community of scientists, i.e. an attitude which is prepared to change the paradigm if very good reasons arise. This guarantee and the concentration upon the elaboration of puzzles is bound to lead to crises. I completely agree (although I do not like the historicistic flavor of the whole affair). But why should we not add another guarantee ("doppelt hält besser" as the Germans say—doubly bound is more secure) viz. alternatives which will work in the same direction, namely in the direction of crisis and therefore new discoveries? "Normal Science" as you describe it is therefore an activity where only one of the crisis-creating means is being used and this for reasons which are psychologically understandable, but not otherwise impressive: people are dazzled by the initial success of the main paradigm; they say they have no time etc. etc. It is as if a person whose only aim is to have as many children as possible sleeps with one woman only because this is what everybody does the reason being that nobody is bold enough to approach more than one woman in five years.

[6] p. 96: "It is hard to see how new theories could arise without ... destructive changes in beliefs of nature." {SSR, p. 98} Read in one way this is a tautology: ideas don't change unless they change. But with you one has to read the quoted statement as follows: "It is hard to see how new theories could arise without a previous crisis due to ill fitting between one paradigm and nature." The answer to this is, as you yourself have admitted, that they do arise without such a prelude (thermodynamics etc. etc.). See also the above.

[7] p. 96 again: concerning an interpretation "closely associated with logical positivism" {similarly in SSR, p. 98, but with 'early' inserted: "early logical positivism"} which "would restrict the range and meaning of an accepted theory so that is could not possibly conflict with any later theory ..." {SSR, p. 98}. Let me comment, at this point, that the intention of the Copenhagen people was to construct exactly such a theory and that they think they have succeeded in constructing it. One element of the Copenhagen thought may be represented as follows: classical physics ran into trouble because it was not purely factual but went beyond the factual evidence that was available at any time of its existence. Or, to express it in terms which are used by some members of the Copenhagen school: because it contained metaphysical elements. It was the aim of the quantum theory (again only for some members of the Copenhagen school) to remedy this shortcoming of the classical physics and thereby to achieve, at one stroke, two different things: first, to obtain a theory that was correct from an empirical point of view; secondly, to obtain a theory that did not contain any metaphysical elements and all of whose basic postulates were a mere expression of fact, of empirical fact. According to Bohr the remedy lies in restricting the application of the classical concepts. The classical concepts, Bohr says, are abstractions. That means that they are partly related to experience (they are after all abstracted from experience), and go partly beyond it. It is the part that goes beyond experience that has caused the trouble. If this part is removed we shall obtain a theory which is wholly empirical and wholly correct. Now the question arises: which part of the classical concepts is the part that goes beyond experience? According to Bohr the answer is to be found by an analysis of the experimental fact of duality. Duality forces us to restrict the use of the classical concepts (the wave concepts, the particle concepts) and it also tells us to what extent their use must be restricted: it must be restricted exactly to the extent that is given by the uncertainty relationship. Classical physics was bad, incorrect, and it was incorrect because it went beyond experience. Duality teaches us how far it went beyond experience and it thereby teaches us what parts of the classical theory must be cut out, as it were, in order to obtain a theory that is

correct and wholly empirical. The trouble of the classical paradigm therefore does not show that it must be replaced by a different paradigm; it only shows that this paradigm was too pretentious and that its pretense must be curtailed by cutting out what is not justified by experience. The quantum theory is therefore not a new paradigm; it is rather what remains of the classical paradigm when its metaphysical pretensions have been eliminated. To express it even more boldly: the quantum theory presents us with the empirical content of the classical physics, rather than with an entirely new paradigm. This, I think, is the basic idea of Copenhagen. Any theory that is obtained by treating some classical body of theory in this manner is called by Bohr a "rational generalization" of the corresponding classical theory. Now what does this theory of theories say about the progress of science? It says the following: many new discoveries will be made. These discoveries will lead to new laws. But all these laws will be compatible with the uncertainty relationships and it cannot be otherwise as these relations are repetition of fact. The basic assumptions of the present theory therefore "cannot possibly conflict with any later theory" as you express it—and of this the Copenhagen people are quite certain as they have constructed the quantum theory and the uncertainties in such a manner that they simply repeat facts without making idealizations. The quantum theory is not a paradigm in your sense. It is what remains of a former paradigm (the classical theories) when this has been freed from anything that goes beyond experience. All this must be known if one wants to understand the Copenhagen philosophy. They have not simply added another theory to the theories of the past which at some future time may be replaced by again another theory. They have taken what they think to be the quintessence of all past theories, viz. classical physics and reconstructed it in such a manner that it fits the facts closely and can therefore not any more run into any crisis. From now on we have entered a new age of scientific activity. There will be no more revolutions, there will only be accumulation. This, as you will also see from my enclosed paper {very probably "Problems of Microphysics", see the beginning of this letter}, is the Copenhagen point of view. And this makes it understandable why they react so violently against Bohm. For them Bohm wants to do nothing less than turning back history. He wants to make undone the tremendous progress towards unmetaphysical theories made in Copenhagen and again invent theories that go beyond the facts thereby reverting to a history that will again be full of crises, revolutions etc. In this connection I beg you to read (and then to return) the enclosed review, by Rosenfeld, of Bohm's book (it was published 1958 in the Manchester Guardian[1]; I have written a very impolite

[1] {The attached review reads as follows, with underlinings added probably by Feyerabend}:**CAUSE IN PHYSICS** By L. Rosenfeld

Causality and Chance in Modern Physics. By D. Bohm. Foreword by Louis de Broglie. Routledge and Kegan Paul. Pp. xi. 170. 21s.

This is the most paradoxical book I have seen for many years. Its author is not only a distinguished physicist but a thinker with progressive views on most things; yet he is blind to one of the greatest of the advances in rational thinking which the development of physics has initiated. He purports to discuss fundamental aspects of modern science but, scorning the scientific method established since Newton's time, he revives the metaphysical attitude of system-makers like Descartes, whose self-assurance he wonderfully counterreview. Only be aware what you will face in Copenhagen and how careful you will have to be. You will face a metaphysics. And metaphysicians usually are very dogmatic; but they are even more dogmatic when they believe their metaphysics to be truly factual). You will now also see that your "today this remains a minority view" (of line 7 from bottom of page 96 of your essay {SSR, p. 98 bottom}) is even more true. Not only are the philosophers reluctant to accept such a view; but the majority of physicists agrees that this view correctly describes the relation between the theories of the past, that this property of past scientific development was due to the partly metaphysical character of the theories employed; that this shortcoming has in the meantime been remedied by Bohr; and that the future of science is therefore bound to be very different from its past and is to be all "normal science", i.e. totally accumulative. You see that the situation is now even more difficult: the Copenhagen people admit what the positivists do not admit, viz. the past science has been as you have described it. They therefore seem to be on your side. But they then point out that this was due to metaphysics, and that this won't happen any more in the future as now we have finally arrived at truly empirical theories.

[7] p. 97 {SSR, p. 99}: excellent! You could not have said it better.

[8] page 98, last paragraph {SSR, p. 100, middle paragraph}: I completely agree. I have to add two things. First, the absurd view derived by you from the point of view you criticise viz. that if it were correct all a theory would say would be a repetition

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[1]*continued*

imitates. He holds out the expectation that his ideas will eventually elucidate the strange properties of matter in its states of highest energy, but there is not a word in the book about the possible relation of these properties to the themes treated at great length in it. The elementary style of exposition could easily mislead the unwary layman into the belief that he is here presented with a straightforward account of the problem stated in the title; but what the specialist sees in these dreary pages is a picture distorted beyond recognition by misinterpretation and misrepresentation.

The author's original endeavour was to vindicate the traditional conception of deterministic causality in atomic physics. Having failed in this, he now tries to argue that one ought to consider more general types of causality, partaking of both determinism and chance: the element of chance appearing at any "level" of description would have to be underpinned by the determinism of some hidden processes going on at a "deeper" level. This novel conception, however, is not "synthetic", as he thinks, but just confused; at any rate, it does not correspond to the actual situation with which we are confronted in atomic physics. In fact, the infinite sequence of deeper and deeper levels of reality which our author imagines, unsupported as it is by any concrete evidence, appears to serve no other purpose than to provide him with a perpetual line of escape from embarrassing difficulties.

But why, the reader may well ask, all this fuss about determinism? The true character of the causal relations of atomic physics has been fully elucidated by Bohr and Heisenberg thirty years ago, and all that we have learned since about atoms and nuclei and other fundamental agencies of Nature has only strengthened the conclusions then reached and extended their scope. It turns out that the laws governing the behaviour of atomic systems are "statistical", in the same sense as those governing the behaviour of human communities: physicists thrive on chance just as insurance people do. This situation may look strange at first sight, and indeed its explanation would exceed the bounds of a review; but there is nothing "irrational" about it. If it still disturbs some physicists, it is because, in contradiction with the exigencies of sound scientific method, they give to some metaphysical prejudice, like determinism, greater weight than to the immediate lessons of experience. That such irrational dogmatists should hurl the very accusation of irrationality and dogmatism at the defenders of the common-sense, uncommitted attitude of the true scientist is the crowning paradox which gives a touch of comedy to a controversy so distressingly pointless and untimely.

of past experiment, this view has been held both by philosophers and physicists. Schrödinger, of all people, defended it in a long discussion we had in Alpbach. Secondly, however, I don't understand how you can say what you say here and at the same time assert, in your paper on measurement in ISIS {vol. 52 (1961), reprinted in The Essential Tension, pp. 178–224} (page 166, footnote 9) that "to the extent that a scientific theory must be accompanied by a statement of the evidence for it ... the full theory ... must be analytically true".

pages 99, 100, 101 again excellent {SSR, pp. 101–103}.

[9] page 101, beginning of last paragraph {SSR, p. 103, beginning of second paragraph}: you have not shown that the differences are necessary!

[10] page 107 {SSR, p. 108}: the fluctuations of the community of scientists described by you on this page, taken together with Planck's dictum (and the consequences it implies!) shows very clearly, at least to me, that history is the history of human idiosyncrasies, of folly, and therefore without reason. Anybody who wants to derive reason from history is therefore bound to cheat at one place or another!

[11] page 108, bottom {probably refers to SSR, p. 109: "Therefore, when paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions."}: but one thing must remain: the belief that there is no absolute paradigm. If this belief is given up as well, then there may still be historical continuity in the sense that there will be the same institutions, the same journals. But science as a rational undertaking won't exist any longer (and this even if people continue to call the new dogmatism 'science').

[12] page 109: "they will inevitably talk through each other" {SSR, p. 109, bottom}. This is not true! If Bohm were to produce a prediction which is forbidden by the current theory, if the experiment were carried out and decided in Bohm's favor, then a discussion would ensure which would be to the point and where people would not talk through each other. An important historical example is Eddington's expedition in order to decide between Einstein and Newton.

paradigms "necessarily" self-justifying? (3rd line from bottom) {not in SSR}

[13] page 110, line 5 from bottom: almost always irreversible {SSR, p. 111}? This does not seem to be correct.

page 111 "always slightly at cross-purposes" {SSR, p. 112}—this seems to be a little exaggerated. bottom: things "are again seen right side up" {similarly in SSR, p. 112: "are again seen as they had been"}—this is not factually correct. Things are not seen right side up but the situation is rather like the situation of a person who has learned reading a paper that is upside down. He does not see now the paper right side up, but rather he sees it still upside down without this disturbing him in his attempt to read the print.

[14] page 114, middle "that light was a self-consistent entity" {SSR, p. 114, bottom}—this is not quite what people think in Copenhagen. There is no such thing as "light", there are, however, phenomena such as interference, Doppler effect, photoelectric effect which in the classical theory were explained, all of them, by

reference to a single entity. Now this can't be done. Nor can we introduce a new entity (remember that the quantum theory is not a new paradigm, but it is classical physics, disinfected from metaphysics, and therefore also disinfected from the idea of real entities which are independent of observation. This having been done we cannot any longer speak of light being an entity, except in a picturesque manner, but we can only speak of different experimental results, and we can use the formalism of the quantum theory in order to predict what happens under these different circumstances).

[15] pp. 115 ff {SSR, pp. 115 ff.}: quite excellent. Here, at last, is empirical evidence, over and above vague talk such as propounded by Hanson, that we see different things when we see things differently.

[16] p. 118, last line of second paragraph {SSR, p. 118, last line of second paragraph}. In discussion you told me that in order to see something, or to experience a change of vision there must be a nature, a paradigm, and then you admitted, some general 'methodological' attitude concerning any paradigm. Now what you call 'the world' or 'a world' here is nature as seen through the spectacles of a paradigm, and 'different worlds' are simply different spectacles; (i.e. what Kant would have called 'nature'). This still presupposes the existence of one nature that is unchanged, although the paradigms are changed. I do not see how this nature can be eliminated by some "principle of economy". This is an almost positivistic argument. It also makes nonsense of the attempts of the psychology of perception to explain how one and the same world can be mirrored in a different manner in people who have accepted a different paradigm.

[17] page 121 {SSR, p. 121, middle}: you talk about the traditional epistemological paradigm, about its past successes, about the fact that it was developed at the time of Newton, about the need for its revision. All this I do not understand. What are you referring to? Are you referring to the theory of sense data? Then your history is incorrect: this theory was discussed in antiquity (see its explicit discussion in THEAITETOS), it was defended by Antisthenes, known to Aristotle etc. etc. "Its exploitation" you say "has been fruitful of a fundamental understanding that perhaps could not have been achieved in another way" {SSR, p. 121}. What is the evidence for this? What were the successes? What did it explain? It seems to me that here you simply want to please the philosophers by saying that in some sense they were right without knowing yourself the sense in which they were right. I think you are not confident of your judgement here and you should either omit the matter altogether, or deal with it in a much more explicit manner. Even more so as you seem to identify the theory of sense data with a rarely accepted particular version of it, i.e. the version that sense data are pictures on the retina. What I would like you to retain, however, is your excellent argument against the assumptions that a sense data language is the neutral starting point (rather than the already infected point of arrival) for a scientific theory. This is a very good argument that should remain. Another thing that puzzles me is your diffident tolerance towards the sense data theory and your strenuous attempt not to hurt the sense dataists too much ["None of these remarks is intended

to indicate" you write, on page 122 "that scientists do not characteristically interpret observations and data" {SSR, p. 122}—this sound to me like "do not worry, babes, I am not out to kill you, I might even become your friend" etc. etc.]
[18] page 123: "Paradigms are not corrigible by normal science at all" {SSR, p. 122}—this is a tautology if I ever heard one. But the last lines of this page are quite excellent. By the way: look for all places when you speak of 'retinal impressions' and eliminate this expression for the reasons stated above.
[19] page 127: "In the absence of a developed alternative" {SSR, p. 126}. May I modestly remind you of my "Explanation, Reduction, and Empiricism"?
"it no longer functions effectively" {SSR, p. 126}—did it ever function? And if so, when? You can tell me in a minute which problems were solved at what time by Newton's theory. Can you tell me the same thing about what you call the "epistemological paradigm that has dominated philosophy for three [twenty would be better] centuries" {similarly in SSR, p. 126: "epistemological viewpoint that has most often guided Western philosophy for the three centuries"}? In the last paragraph you say that a sense datum language will perhaps some day be devised {SSR, p. 126, bottom}. And this after you have shown that this could not be done! This whole sense datum paragraph is very unsatisfactory. It seems to me that you are not quite sure of yourself, at least much less sure than you are when you are talking about science, and that your prose is very often dictated by the wish to be polite to the philosophers, and perhaps to get their sympathy, and their interest by not being too rough to them. Carnap and Goodman? (p. 128). Ridiculous! Very far? Carnap did not even arrive at tables let alone at atoms. And he explicitly abandoned his "Aufbau" ideas in 1935 (Testability and Meaning). And the main objection against all these attempts, however far they might get in constructing physical objects is that their elements of construction, i.e. the elements of a sense data language, have not yet been shown to exist in a neutral manner. For all these reasons I emphatically agree with you when you say "that scientists are right in principle as well as in practice when they treat oxygen and penduli (and perhaps also atoms and electrons) as the fundamental ingredients of their immediate experience" (p. 128f) {SSR, pp. 127–128}. By the way, a theory which would allow for such procedure has been sketched, in 1932, by Carnap himself and I have discussed it under the title of "the pragmatic theory of experience" in my explanation essay. Now from this there is still a long step to the point of view which you at some time seem to want to adopt (p. 118 {SSR, p. 118}, and my item [16] above) viz. that in addition to these immediate experiences no further 'nature' should be assumed to exist. You want to identify the 'worlds' of the scientist with 'nature', or omit the latter altogether which would mean that you identify experiences with 'nature' (experiences of the more complicated kind you are defending such as atom-experiences, penduli-experiences etc. etc.) or omit the idea of nature altogether (of a nature that is independent of experiences) which would mean that you fall back upon the 'traditional paradigm of epistemology' the only difference being that your experiential language is more complicated than theirs. I must repeat

that this chapter on epistemology makes a somewhat confused impression upon me. page 130: again the 'retinal imprints'. The same on page 131. {SSR, pp. 128, 129} page 131 bottom {SSR, p. 130, middle}: very important (same operations, different aspects) the same applies, of course, also to words: same words obtain a very different meaning.

[20] page 112: your reference to Hanson {SSR, p. 113}. Your defence, in conversation, of this reference, seems to me to amount to saying that you prefer being member of a community to searching after the truth. True, Hanson, in some vague sense, defends the same point of view which you also defend. But first of all he has added nothing at all to Wittgenstein whom he imitates (all you need is to read, in this connexion, and really should read, are Wittgenstein's investigations on seeing in the second part of the INVESTIGATIONS), and not at all well. Of course, Hanson is closer to you than, say, to Ayer, and his book is widely read. But as regards the former argument I must retort that in some sense he is very far away indeed from you and this is care in presentation of point of view. And as regards the second argument I really cannot accept it: when many people read Hanson, the worse for them; no obligation for you can be derived from the follies of others. Worse still, by referring to the book you as it were make legitimate their follies, which is a very bad thing indeed. Do not misunderstand me: I love Hanson, and this is the reason why my review was so tame (I should have referred to the incredible mess he makes of the purely formal parts of quantum theory in the footnotes). You really seem to practice what you preach: belonging to a community is the highest value for you as well as your criterion for the selection of quotations. You quote Hanson because he supports the epistemological paradigm you are prepared to defend (though neither you nor he has succeeded in formulating clearly what is involved in this paradigm) and you do not at all care about his sloppiness, inaccuracies on physical, philosophical (and perhaps even historical) points. You by your quotation recommend Hansons to people despite the fact that they will acquire sham knowledge etc. etc. (misleading popularization)—but this does not matter for you as soon as, by hook or by crook, they are made to join the very varied community of those who believe in a new paradigm. Using a political example this seems to me like putting the party above the morals. If somebody is an influential party member, he is to be welcomed and to be drowned in honors, even if it is well known that all his influence derives from his contacts in the underworld and that his money has been obtained by holding up banks. Tom, you are immoral, as immoral as a Hegelian who identifies the main forces of history with what is good. I take a completely different course, also in my classes. People ask me to talk about Wittgenstein, because he is an influential contemporary philosopher, and they also ask me to talk about Ayer, etc. etc. I refuse to do this. And if they ask "shall we read the PHILOSOPHICAL INVESTIGATIONS?" I reply "If you want to waste your time, yes". In reading Hanson, however, I cannot see the slightest merit. Wittgenstein, at least, is an original source for some craze, and so there may be a justification, for historical reasons, to study him. But for reading Hanson there is not

the slightest justification. By the way it may interest you that Stanley seems to be on your side in the question of quoting Hansons. This became clear to me when we discussed Wagner. "Wagner is an influence" he said, "therefore we must study him in order to overcome him". All this is historicism: Wagner is a historical influence, and will act, unless one counteracts. Has nobody ever hit upon the simple idea of simply forgetting about Wagner? This, surely is less time consuming, and just as effective. Why first teach something, and then spend all one's energy in order to refute it? Simply don't mention it.

[21] page 138 "must be slightly at cross purposes" {similarly in SSR, p. 148: "are always at least slightly at cross-purposes"}—this may happen as regards this or that problem, but not as regards every problem. For if the two theories were at cross purposes as regards every problem there could not be a "conflict" (your page 94 {SSR, p. 96, middle}) between the two paradigms. However as soon as there is a conflict, in the very same moment there exists also common ground and the possibility to decide, to the satisfaction of both parties, which paradigm has to go. It seems to me that you exaggerate, and generalise a situation that may occasionally occur (that people talk through each other) in such a manner that you cannot any longer say what you want to say, namely, that they hold conflicting points of view.

Let me mention in this connexion that the assertion, made by many ordinary language philosophers (including your clever friend Stanley in his attack upon poor Pole) that certain philosophical theories do not conflict with the ordinary point of view and are therefore nothing but misleading reformulations of that point of view, let me say that this assertion is incorrect for exactly the same reasons. Take the assertion which is sometimes made by philosophers (and which may also be subscribed by a Popperian) that we never can fully establish any statement. Now the ordinary language philosophers (OLP for short—I like this abbreviation, it sounds like GULP) will at once point out that the word 'fully establish' is used here in a very unusual sense; so, he will say, when the ordinary man says, having seen and touched a table, that he has 'fully established' the presence of a table in front of him, the attacked philosopher will say that this has not been fully established etc. etc. And as both use the words 'fully established' in different senses, they are not in conflict when the one says 'fully established' and the other 'not fully established'. This is quite correct—in this single instance. Still there is a world of difference between the two as regards questions of test. The ordinary man is satisfied with the testimony of his eyes and he therefore stops after a finite amount of steps. The attacked philosopher is not so satisfied and he will regard any stopping as only preliminary and due to lack of time. He will not ascribe any logical preference to the process of looking, assuming, for example, that it is very convincing evidence that settles the matter. For him there is nothing that settles the matter except lack of time. And this point of view is very different from the corresponding point of view of the everydayman (or from the point of view which people such as Cavell ascribe to somebody they call the everydayman and who does not exist).

[22] page 140 good reasons why textbooks "should be systematically misleading" {SSR, p. 137}—this sounds like there being good reasons for stealing. Also I hope I have shown above that alternatives to any present paradigm should be considered, such consideration leading to the more speedy arrival of crises. Hence it is imperative that textbooks be not written in the manner indicated. I therefore cannot accept the "as pedagogy this technique ... is unexceptionable" of p. 144 either {SSR, p. 140}. You are too good to too many people at once. This can only lead to anarchy.

[23] Your chapter XI does not at all take into consideration the creation of crises 'from above' and from 'within' (cf. my item [2] on page 4 of my letter). Adding this will, I think, lead to a radically different summing up! For example you will not be able to say that testing occurs only "after the sense of crisis has evoked an alternative candidate for paradigm" (page 150, line 8 from below) {SSR, p. 145, top} as you have admitted that a crisis may be brought about by the existence of an alternative paradigm from whence it follows that alternatives will not be invented only after the arrival of a crisis.

[24] page 151: "probabilistic verification theories demand the comparison of all theories that could be possibly constructed for the explanation of a given body of sensory experience" Not so! Carnap's theory, for example, does not need reference to all alternatives (Reichenbach's does), nor does it refer to sensory experience as evidential basis: it refers to experimental results. {The respective sentence is changed in SSR, p. 145 to: "One probabilistic theory asks that we compare the given scientific theory with all others that might be imagined to fit the same collection of observed data"}.

[25] page 153: "to the historian, at least etc. etc." {SSR, p. 147} But the historian can only show that people are sloppy, for example, that they do not all obey the 10 commandments. He cannot show, that the 10 commandments are wrong. Same about scientific method.

page 154: end of first paragraph: resolved by proof {SSR, p. 148}—this suggests that you attack the very nonsensical theory that logic alone (see my above reference to other occurrences of this expression).

[26] page 155: "The men who scoffed at Einstein etc." {similarly in SSR, p. 149}—you say that their space could not be curved {"their" not in SSR} and you thereby seem to indicate that they had some reason to smile when Einstein said that space was curved. You ought also mention, if you give the defenders of this argument such an advantage (you seem here to lean towards Heisenberg's idea of a 'closed theory' and also a little towards Hanson), that space in their sense does not exist, or at least that it does not exist, if Einstein's theory is correct and this quite irrespective of whether the latter theory is now expressed by saying that 'space' is curved, or in a different, and to the people you mention, less offensive manner. Same about 'earth'. ARGUMENTS FROM MEANING NEVER LEAD ANYWHERE, AT THE VERY MOST, IF YOU GRANT THEM, YOU HAVE TO ADD THAT THE THING WITH THIS PARTICULAR MEANING DOES NOT EXIST.

page 156 {SSR, p. 150}: again the different worlds with ideas which are at cross purpose: how can such different worlds conflict? And if they conflict, on the other hand, is it then not possible to force the transition from one paradigm to the next?

Let me repeat this argument. You say (1) that succeeding paradigms will conflict; (2) that people holding them will talk through each other; and that (3) they live therefore in two different worlds such that the transition from the one to the other (4) cannot be forced, but is something like a conversion. Now (2), (3), and (4) seem to represent too radical a view of the matter (crucial experiments do exist), and moreover they are inconsistent with (1): if paradigms do conflict then there must be at least two statements, the one from the first paradigm, the other from the second, which are inconsistent, and therefore do not "talk through each other". Hence, if the holder of the first paradigm can, by experiment, show that his statement represents the facts, then the second chap must give up and this without any conversion being involved. For as both share a statement we must also assume that they share a set of ideas concerning the circumstances under which this statement is established by experiment (otherwise they would not share the statement, but at most a formulation of a statement, or a sentence!)
line 5 from bottom of page 157 is therefore much too general {probably SSR, p. 151: "The transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced"}. Conversion may be necessary, it is not necessary in the case of conflicting paradigms. That is, it is never necessary (for you assert that all succeeding paradigms conflict—by the way, you assert this without giving reasons for it!)

I think I stop here. I shall perhaps send you another letter.

Paul