

Consolations for the Specialist¹

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'I have been hanging people for years, but I have never had all this fuss before.' (Remark made by Edward 'Lofty' Milton, Rhodesia's part time executioner on the occasion of demonstrations against the death penalty.) 'He was'—says *Time Magazine* (15 March 1968)—'professionally incapable of understanding the commotion.'

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I INTRODUCTION

In the years 1960 and 1961 when Kuhn was a member of the philosophy department at the University of California in Berkeley I had the good fortune of being able to discuss with him various aspects of science. I have profited enormously from these discussions and I have looked at science in a new way ever since.² Yet while I thought I recognized Kuhn's *problems*; and while I tried to account for certain *aspects* of science to which he had drawn attention (the omnipresence of anomalies is one example); I was quite unable to agree with the *theory of science* which he himself proposed; and I was even less prepared to accept the general *ideology* which I thought formed the background of his thinking. This ideology, so it seemed to me, could only give comfort to the most narrowminded and the most conceited kind of specialism. It would tend to inhibit the advancement of knowledge. And it is bound to increase the anti-humanitarian tendencies

¹ An earlier version of this paper was read in Professor Popper's seminar at the London School of Economics (March 1967). I would like to thank Professor Popper for this opportunity as well as for his own detailed criticism. I am also grateful to Messrs Howson and Worrall for their valuable editorial and stylistic help.

² The criticism of some features of contemporary methodology which appears in my [1969] and [1970] is but one belated after-effect.

which are such a disquieting feature of much of post-Newtonian science.¹ On all these points my discussions with Kuhn remained inconclusive. More than once he interrupted a lengthy sermon of mine, pointing out that I had misunderstood him, or that our views are closer than I had made them appear. Now, looking back at our debates² as well as at the papers which Kuhn has published since his departure from Berkeley, I am not so sure that this was the case. And I am fortified in my behalf by the fact that almost every reader of Kuhn's *Structure of Scientific Revolutions* interprets him as I do, and that certain tendencies in modern sociology and modern psychology are the result of exactly this kind of interpretation. I hope that Kuhn will forgive me when therefore I once more raise the old issues and that he will not take it amiss when in my effort to be brief I do this in a somewhat blunt fashion.

2. AMBIGUITY OF PRESENTATION

Whenever I read Kuhn, I am troubled by the following question: are we here presented with *methodological prescriptions* which tell the scientist how to proceed; or are we given a *description*, void of any evaluative element, of those activities which are generally called 'scientific'? Kuhn's writings, it seems to me, do not lead to a straightforward answer. They are *ambiguous* in the sense that they are compatible with, and lend support to, both interpretations. Now this ambiguity (whose stylistic expression and mental impact has much in common with similar ambiguities in Hegel and in Wittgenstein) is not at all a side issue. It has had quite a definite effect on Kuhn's readers and has made them look at, and deal with their subject in a manner not altogether advantageous. More than one social scientist has pointed out to me that now at last he had learned how to turn his field into a 'science'—by which of course he meant that he had learned how to *improve* it. The recipe, according to these people, is to restrict criticism, to reduce the number of comprehensive theories to one, and to create a normal science that has this one theory as its paradigm.³ Students must be prevented from speculating along different lines and the more restless colleagues must be made to conform and 'to do serious work'. *Is this what Kuhn wants to achieve?*⁴ Is it his intention to provide a historico-

¹ Cf. my [1970].

² Some of which were carried out in the now defunct *Café Old Europe* on Telegraph Avenue and greatly amused the other customers by their friendly vehemence.

³ See, e.g. Reagan [1967] p. 1385: He states: 'We [that is, we social scientists] are in what Kuhn might call a "pre-paradigm" stage of development in which consensus has yet to emerge on basic concepts and theoretical assumptions.'

⁴ Neurophysiology, physiology, and certain parts of psychology are far ahead of contemporary physics in that they manage to make the discussion of fundamentals an essential part of even the most specific piece of research. Concepts are never completely stabilized

scientific justification for the ever growing need to identify with some group? Does he want every subject to imitate the monolithic character of, say, the quantum theory of 1930? Does he think that a discipline that has been constructed in this manner is in some ways better off? That it will lead to better, to more numerous, to more interesting results? Or is his following among sociologists an unintended side-effect of a work whose sole purpose is to report '*wie es wirklich gewesen*' without implying that the reported features are worthy of imitation? And if this is the sole purpose of the work, then why the constant misunderstanding, and why the ambiguous and occasionally highly moralizing style?

I venture to guess that the ambiguity is *intended* and that Kuhn wants to fully exploit its propagandistic potentialities. He wants on the one side to give solid, objective, historical support to value judgements which he just as many other people seem to regard as arbitrary and subjective. On the other side he wants to leave himself a safe second line of retreat: those who dislike the implied derivation of values from facts can always be told that no such derivation is made and that the presentation is purely descriptive. My first set of questions, therefore, is: why the ambiguity? How is it to be interpreted? What is Kuhn's attitude towards the kind of following I have described? Have they misread him? Or are they legitimate followers of a new vision of science?

3. PUZZLE SOLVING AS A CRITERION OF SCIENCE

Let us now disregard the problem of presentation and let us assume that Kuhn's aim is indeed to give but a *description* of certain influential historical events and institutions.

According to this interpretation it is the existence of a puzzle-solving tradition that *de facto* sets the sciences apart from other activities. It sets them apart in a 'far surer and more direct' way, in a manner that is 'at once . . . less equivocal and . . . more fundamental',¹ than do other and more recondite properties which they may also possess. But if the existence

but are left open and are elucidated now by the one, now by the other theory. There is no indication that progress is hampered by the more 'philosophical' attitude which, according to Kuhn, underlies such a procedure (cf. *this volume*, p. 6). (Thus the lack of clarity about the idea of perception has led to many interesting empirical investigations, some of them yielding quite unexpected and highly important results. Cf. Epstein [1967], especially pp. 6–18.) Quite the contrary, we find a greater awareness of the limits of our knowledge, of its connection with human nature, we find also a greater familiarity with the history of the subject and the ability not only to *record*, but to *actively use* past ideas for the advancement of contemporary problems. Must we not admit that all this contrasts most favourably with the humourless dedication and the constipated style of a 'normal' science?

¹ Cf. *this volume*, p. 7.

of a puzzle-solving tradition is so essential, if it is the occurrence of this property that unifies and characterizes a specific and well recognizable discipline; then I do not see how we shall be able to exclude say, Oxford philosophy, or, to take an even more extreme example, *organized crime* from our considerations.

For organized crime, so it would seem, is certainly puzzle-solving *par excellence*. Every statement which Kuhn makes about normal science remains true when we replace 'normal science' by 'organized crime'; and every statement he has written about the 'individual scientist' applies with equal force to, say, the individual safebreaker.

Organized crime certainly keeps foundational research to a minimum¹ although there are outstanding individuals, such as Dillinger, who introduce new and revolutionary ideas.² Knowing the rough outlines of the phenomena to be expected the professional safebreaker 'largely ceases to be an explorer . . . or at least an explorer of the unknown [after all, he is supposed to know all the existing types of safe]. Instead, he struggles to . . . concretize the known [i.e. to discover the idiosyncracies of the particular safe he is dealing with], designing much special-purpose apparatus and many special-purpose adaptations of theory for that task'.³ According to Kuhn failure of achievement most certainly reflects 'on the competence of the [safebreaker] in the eyes of his professional compeers'⁴ so that 'it is the individual [safebreaker] rather than current theory [of electromagnetism, for example] which is tested'⁵: 'only the practitioner is blamed, not his tools'⁶—and so we can continue step for step, down to the very last item on Kuhn's list. The situation is not improved by pointing to the existence of *revolutions*. First of all, because we are dealing with the thesis that it is normal science which is characterized by the activity of puzzle-solving. And secondly because there is no reason to believe that organized crime will fall behind in the mastery of major difficulties. Besides, if it is the *pressure* derived from the ever increasing number of anomalies that leads, first to a crisis, and then to a revolution, then the greater the pressure, the sooner the crisis must occur. Now the pressure exerted upon the members of a gang and their 'professional compeers' certainly can be expected to exceed the pressures upon a scientist—the latter hardly ever has to deal with the police. Wherever we look—the distinction we want to draw does not exist.

¹ Cf. Kuhn [1961a], p. 357.

² Dillinger considerably advanced the technique of the bank-holdup by staging dress rehearsals in life size models of the target-banks which he built at his farm. He thereby refuted Andrew Carnegie's 'Pioneering don't pay'.

³ Kuhn [1961a], p. 363.

⁴ *This volume*, p. 9; also cf. p. 7 and footnote 1 on p. 5.

⁵ *This volume*, p. 5.

⁶ *This volume*, p. 7; also cf. Kuhn [1962], p. 79.

This of course is no surprise. For Kuhn, as we interpret him now and as he himself very often wants to be interpreted, has failed to do one important thing. He has failed to discuss the *aim* of science. Every crook knows that apart from succeeding at his trade and being popular with his fellow crooks he wants one thing: money. He also knows that his normal criminal activity is going to give him just this. He knows that he will receive the more money and rise the faster on the professional ladder the better he is as a puzzle-solver and the better he fits into the criminal community. Money is his aim. What is the aim of the scientist? And, considering this aim, is normal science going to lead up to it? Or are perhaps scientists (and Oxford philosophers) less rational than crooks in that they 'are doing what they are doing' without regard to an aim?¹ These are the questions which arise if one wants to restrict oneself to the purely descriptive aspect of Kuhn's account.

4. FUNCTION OF NORMAL SCIENCE

In order to answer these questions we must now consider not only the *actual structure* of Kuhnian normal science, but also its *function*. Normal science, he says, is a *necessary presupposition of revolutions*.

According to this part of the argument the pedestrian activity associated with 'mature' science has far reaching effects both upon the *content* of our ideas, and upon their *substantiality*. This activity, this concern with 'tiny puzzles' leads to a close fit between theory and reality, and it also precipitates progress. It does so for various reasons. First of all the accepted paradigm gives the scientist a guide: 'As a glance at any Baconian natural history or a survey of the pre-paradigm development of any science will show, nature is vastly too complex to be explored even approximately at random'.² This point is not new. The attempt to create knowledge needs guidance, it cannot start from nothing. More specifically, it needs a theory, a point of view that allows the researcher to separate the relevant from the irrelevant, and that tells him in what areas research will be most profitable.

To this common idea Kuhn adds a specific twist of his own. He defends not only the *use* of theoretical assumptions, but the *exclusive choice* of one particular set of ideas, the monomaniac concern with only one single point of view. He defends such a procedure first, because it plays a role in actual science as he sees it. This is the description-recommendation ambiguity already dealt with. But he defends it also for a second reason that is somewhat more recondite as the preferences behind it are not made explicit. He defends it because he believes that its adoption will in the end lead to the

¹ 'I am doing what I am doing' was a favourite remark of Austin's.

² Kuhn [1961a], p. 363.

overthrow of the very same paradigm to which the scientists have restricted themselves in the first place. If even the most concerted effort to fit nature into its categories fails; if the very definite expectations created by these categories are disappointed again and again; then we are *forced* to look for something new. And we are forced to do this not just by an abstract discussion of possibilities which does not touch reality, but is rather guided by our own likes and dislikes¹; we are forced to do it by procedures which have established a close contact with nature, and therefore, in the last resort, by nature itself. The debates of *pre-science* with their universal criticism and their uninhibited proliferation of ideas are 'often directed as much to the members of other schools as . . . to nature'.² *Mature science*, especially in the quiet periods immediately before the storm, seems to address nature itself only and may therefore expect a definite *and objective* answer. In order to get such an answer we need more than a collection of facts assembled at random. But we need also more than an everlasting discussion of different ideologies. What is needed is the acceptance of *one* theory and the relentless attempt to fit nature into its pattern. This, I think, is the main reason why the rejection, by a mature science, of the uninhibited battle between alternatives would be defended by Kuhn not only as a *historical fact*, but also as a *reasonable move*. Is this defence acceptable?

5. THREE DIFFICULTIES OF FUNCTIONAL ARGUMENT

Kuhn's defence is acceptable *provided* revolutions are desirable and provided the particular way in which normal science leads to revolutions is desirable also.

Now I do not see how the desirability of revolutions can be established by Kuhn. Revolutions bring about a *change* of paradigm. But following Kuhn's account of this change, or 'gestalt-switch' as he calls it, it is impossible to say that they have led to something *better*. It is impossible to say this because pre- and post-revolutionary paradigms are frequently incommensurable.³ This I would regard as the first difficulty of the functional argument if used in connection with the remainder of Kuhn's philosophy.

Secondly we have to examine what Lakatos has called the 'fine-structure' of the transition: normal science/revolution. This fine-structure may reveal elements we do not want to condone. Such elements would force us

¹ 'If any one offers conjectures about the truth of things from the mere possibility of hypothesis, then I do not see how any certainty can be determined in any science; for it is always possible to contrive hypotheses, one after another, which are found to lead to new difficulties' (Newton [1672]).

² Kuhn [1962], p. 13.

³ Cf. *below*, section 9.

to consider different ways of bringing about a revolution. Thus it is quite imaginable that scientists abandon a paradigm out of frustration and not because they have arguments against it. (Killing the representatives of the *status quo* would be another way of breaking up a paradigm.¹) How do scientists *actually* proceed? And how would we *want* them to proceed? An examination of these questions leads to a second difficulty for the functional argument.

In order to exhibit this difficulty as clearly as possible let us first consider the following *methodological problems*: Is it possible to give reasons for proceeding as Kuhn says normal science proceeds, that is, for trying to stick to a theory despite the existence of *prima facie* refuting evidence, of logical, and of mathematical counter arguments? And assuming it is possible to give such reasons—is it then possible to abandon the theory without violating them?

In what follows I shall call the advice to select from a number of theories the one that promises to lead to the most fruitful results, and to stick to this one theory even if the actual difficulties it encounters are considerable, the *principle of tenacity*.² The problem then is how this principle can be

¹ This is how *religious doctrines* or *political doctrines* were frequently replaced. The principle remains even today, though murder is no longer the accepted method. The reader should also consider Max Planck's remark that old theories disappear because their defenders die out.

² This formulation of the principle was suggested by an objection which Isaac Levi raised against an earlier version.

The principle of tenacity as formulated in the text should not be confused with Putnam's *rule of tenacity* (Putnam [1963], p. 772). For while Putnam's rule demands that a theory should be retained 'unless it becomes inconsistent with the data' (his italics) tenacity as understood by Kuhn and by myself demands that it should be retained *even if there are data which are inconsistent with it*. This stronger version creates problems which do not appear in Putnam's methodology and which, I suggest, can be solved only if one is prepared to use a multiplicity of mutually inconsistent theories *at any time of the development of our knowledge*. It seems to me that neither Kuhn nor Putnam is prepared to take this step. But while Kuhn sees the need for the use of alternatives (see below) Putnam demands that their number be always reduced either to one or to zero (*ibid.* pp. 770 ff.).

Lakatos differs from the account given in the text above in two respects. He distinguishes between *theories* and *research programmes*. And he applies tenacity to research programmes only.

Now while I admit that the distinction and the use he makes of it may increase clarity, I am still inclined to stick to my own and much more vague term 'theory' (for a partial explanation of this term, cf. my footnote 5 [1965a]) which covers both Lakatos's 'theories' and 'research programmes', to connect it with tenacity, and to *altogether eliminate* the more simple forms of refutation. One reason for this preference is given by Lakatos himself who has shown that even simple refutations involve a plurality of theories (see especially his paper in *this volume*, pp. 121 ff.). Another reason is my belief that progress can be brought about only by the active interaction of different 'theories' which of course assumes that the 'research programme'-component comes forth not only occasionally, but is *present all the time* (cf. also *below*, section 9).

defended, and how we can change our allegiance to paradigms in a manner that is either consistent with it, or perhaps even dictated by it. Remember that we are here dealing with a *methodological* problem and *not* with the question of how science *actually* proceeds. We are dealing with it because we hope that its discussion will sharpen our historical perception and will lead us to interesting historical discoveries.

Now the solution of the problem is quite straightforward. The principle of tenacity is reasonable because theories are capable of development, because they can be improved, and because they may eventually be able to accommodate the very same difficulties which in their original form they were quite incapable of explaining. Besides, it is not at all prudent to put too much trust in experimental results. Indeed, it would be a complete surprise and even a cause for suspicion, if all the available evidence should turn out to support a single theory, even if this theory should happen to be true. Different experimenters are liable to commit different errors and it usually needs considerable time before all experiments are brought to a common denominator.¹ To these arguments in favour of tenacity Professor Kuhn would add that a theory also provides *criteria* of excellence, of failure, of rationality, and that one must support it as long as possible, in order to keep the discourse rational as long as possible. The most important point is however this: it is hardly ever the case that theories are directly compared with 'the facts', or with 'the evidence'. What counts and what does not count as relevant evidence usually depends on the theory *as well as* on other subjects which may conveniently be called 'auxiliary sciences' ('touchstone theories' is Imre Lakatos's apt expression²). Such auxiliary sciences may function as additional premises in the derivation of testable statements. But they may also infect the observation language itself, providing the very concepts in terms of which experimental results are expressed. Thus a test of the Copernican view involves on the one hand assumptions concerning the terrestrial atmosphere, the effect of motion upon the object moved (dynamics); and on the other it also involves assumptions about the relation between sense experience and 'the world' (theories of cognition, theories of telescopic vision included).

The former assumptions function as premises while the latter determine which impressions are veridical and thus enable us not only to *evaluate*, but even to *constitute* our observations. Now there is no guarantee that a fundamental change in our cosmology, such as a change from a geostatic

¹ It took about twenty-five years before the disturbances of D. C. Miller's repetition of the Michelson-Morley experiment were accounted for in a satisfactory manner. H. A. Lorentz had given up in despair long before that time. ² Cf. his [1968a].

to a heliostatic point of view, will go hand in hand with an improvement of all the relevant auxiliary subjects. Quite the contrary: such a development is extremely unlikely. Who for example would expect the invention of Copernicanism and of the telescope to be at once followed by the appropriate physiological optics? Basic theories and auxiliary subjects are often 'out of phase'. As a result we obtain refuting instances which do not indicate that a new theory is doomed to failure, but only that it does not fit in at present with the rest of science. This being the case scientists must develop methods which permit them to retain their theories in the face of plain and unambiguously refuting facts, even if testable explanations for the clash are not immediately forthcoming. The principle of tenacity (which I call a 'principle' for mnemonic reasons only) is a first step in the construction of such methods.¹

Having adopted tenacity we can no longer use recalcitrant facts for removing a theory, *T*, even if the facts should happen to be as plain and straight-forward as daylight itself. But we can use *other* theories, *T'*, *T''*, *T'''*, etc. which *accentuate* the difficulties of *T* while at the same time promising means for their solution. In this case elimination of *T* is urged by the principle of tenacity itself.² Hence, if change of paradigms is our aim, then we must be prepared to introduce and articulate alternatives to *T* or, as we shall express it (again for mnemonical reasons), we must be prepared to accept a *principle of proliferation*. Proceeding in accordance with such a principle is *one* method of precipitating revolutions. It is a *rational* method. Is it the method which science *actually* uses? Or do scientists stick to their paradigms to the bitter end until disgust, frustration and boredom makes it quite impossible for them to go on? What *does* happen at the end of a normal period? We see that our little methodological fairytale makes us indeed look at history with a sharpened vision.

I am sorry to say that I am quite dissatisfied with what Kuhn has to offer on this point. On the one side he steadfastly emphasizes the dogmatic,³ authoritarian,⁴ and narrowminded⁵ features of normal science, the fact that it leads to a temporary 'closing of the mind',⁶ that the scientist participating in it 'largely ceases to be an explorer . . . or at least an explorer of the unknown. Instead, he struggles to articulate and concretize the

¹ For details concerning the 'phase difference' between theories and the corresponding auxiliary sciences, cf. my [1969]. The idea already occurs in Lakatos's [1963-4]; it is a commonplace for Lenin and Trotsky (cf. my [1969]).

² This is of course not the whole story—but the present sketch suffices entirely for our purpose. Note that Kuhn's argument for tenacity (need for a rational background of argument) is not violated either as the better theory will of course also provide better standards of rationality and excellence. ³ Kuhn [1961a], p. 349.

⁴ *Ibid.* p. 393.

⁵ *Ibid.* p. 350.

⁶ *Ibid.* p. 393.

known . . .¹ so that 'it is [almost always] the individual scientist rather than [the puzzle-solving tradition, or even some particular] current theory which is tested'.² 'Only the practitioner is blamed, not his tools.'³ He realizes of course that a specific science such as physics may contain more than one puzzle-solving tradition, but he emphasizes their 'quasi-independence', asserting that each of them is 'guided by its own paradigms and pursuing its own problems'.⁴ A single tradition therefore will be guided by a single paradigm only. This is one side of the story.

On the other side he points out that puzzle solving is replaced by more 'philosophical' arguments as soon as there exists a choice 'between competing theories'.⁵

Now if normal science is *de facto* as monolithic as Kuhn makes it out to be, then where do the competing theories come from? And if they *do* arise, then why should Kuhn take them seriously and allow them to bring about a change of the argumentative style, from 'scientific' (puzzle solving) to 'philosophical'?⁶ I remember very well how Kuhn criticized Bohm for disturbing the uniformity of the contemporary quantum theory. Bohm's theory is *not* permitted to change the argumentative style. Einstein, whom Kuhn mentions in the above quotation, *is* permitted to do so, perhaps because his theory is now more firmly entrenched than Bohm's. Does this mean that proliferation is permitted as long as the competing alternatives are firmly entrenched? But pre-science which has exactly this feature is regarded as inferior to science. Besides, twentieth-century physics *does* contain a tradition which wants to isolate the general theory of relativity from the rest of physics, and restrict it to the very large. Why has Kuhn not supported *this* tradition which is in line with his view of the 'quasi-independence' of simultaneous paradigms? Conversely, if the existence of competing theories involves a change of argumentative style, must we not then doubt this alleged quasi-independence? I have been unable to find a satisfactory answer to these questions in Kuhn's writings.

Let us pursue the point a little further. Kuhn has not only *admitted* that multiplicity of theories changes the style of argumentation. He has also ascribed a definite *function* to such multiplicity. He has pointed out more than once,⁷ in complete agreement with our brief methodological remarks, that refutations are impossible without the help of alternatives. Moreover,

¹ Kuhn [1961a], p. 363.

² *This volume*, p. 5.

³ *This volume*, p. 7; also cf. Kuhn [1962], p. 79.

⁴ Kuhn [1961a], p. 388.

⁵ *This volume*, p. 7.

⁶ 'Philosophical' in Kuhn's (and Popper's) sense and *not* in the sense of, say, contemporary linguistic philosophy.

⁷ Cf. Kuhn [1961b] and also my acknowledgement in my [1962], p. 32.

he has described in some detail the magnifying effect which alternatives have upon anomalies and has explained how revolutions are brought about by such a magnification.¹ He has therefore said, in effect, that scientists create revolutions in accordance with our little methodological model and *not* by relentlessly pursuing one paradigm and suddenly giving up when the problems get too big.

All this leads now at once to difficulty number three, viz. the suspicion that normal or 'mature' science, as described by Kuhn, *is not even a historical fact*.

6. DOES NORMAL SCIENCE EXIST?

Let us recall what we have so far found to be asserted by Kuhn. First, it is asserted that theories *cannot* be refuted except with the help of alternatives. Secondly, it is asserted that proliferation also plays a *historical role* in the overthrow of paradigms. Paradigms *have been* overthrown because of the way in which alternatives have enlarged existing anomalies. Finally, Kuhn has pointed out that anomalies exist *at any point* of the history of a paradigm.² The idea that theories are blameless for decades and even centuries until a big refutation turns up and knocks them out—this idea, he asserts, is nothing but a myth. Now if this is true, then why should we not start proliferating *at once* and *never* allow a purely normal science to come into existence? And is it too much to be hoped that scientists thought likewise, and that normal periods, if they ever existed, cannot have lasted very long and cannot have extended over large fields either? A brief look at one example, viz. the last century, shows that this seems indeed to be the case.

In the second third of that century there existed at least three different and mutually incompatible paradigms. They were: (1) the *mechanical point of view* which found expression in astronomy, in the kinetic theory, in the various mechanical models for electrodynamics as well as in the biological sciences, especially in medicine (here the influence of Helmholtz was a decisive factor); (2) the point of view connected with the invention of an independent and phenomenological *theory of heat* which finally turned out to be inconsistent with mechanics; (3) the point of view implicit in Faraday's and Maxwell's *electrodynamics* which was developed, and freed from its mechanical concomitants, by Hertz.

¹ A minor disturbance, still accessible to treatment 'can be seen, from another viewpoint, as a counterinstance, and thus as a source of crisis' (Kuhn [1962], p. 79). 'Copernicus' astronomical proposal . . . *created* an increasing crisis for . . . the paradigm from which it had sprung' (*ibid.* p. 74, my italics), 'Paradigms are not corrigible by normal science *at all*' (*ibid.* p. 121, my italics).

² Kuhn [1962], pp. 80 ff. and p. 145.

Now these different paradigms were far from being 'quasi-independent'. Quite the contrary, it was their *active interaction* which brought about the downfall of classical physics. The troubles leading to the special theory of relativity could not have arisen without the tension that existed between Maxwell's theory on the one side and Newton's mechanics on the other (Einstein has described the situation in beautifully simple terms in his autobiography; Weyl has given an equally brief, though more technical account in *Raum, Zeit, Materie*; Poincaré exhibits this tension already in 1899, and then again in 1904, in his St Louis lecture). Nor was it possible to use the phenomenon of Brownian motion for a direct refutation of the second law of the phenomenological theory.¹ The kinetic theory had to be introduced from the very start. Here again Einstein, following Boltzmann, led the way. The investigations leading up to the discovery of the quantum of action, to mention still another example, brought together such different, incompatible, and occasionally even incommensurable disciplines as mechanics (kinetic theory as used in Wien's derivation of his law of radiation), thermodynamics (Boltzmann's principle of the equal distribution of energy over all degrees of freedom) and wave optics and they would have collapsed had the 'quasi-independence' of these subjects been respected by all scientists. Of course not everyone participated in the debate and the great majority may well have continued attending to their 'tiny puzzles'. However if we take seriously what Kuhn himself is teaching then it was not *this* activity that brought about progress, but the activity of the proliferating minority (and of those experimenters who attended to the problems of this minority, and to their strange predictions). And we may ask whether the majority does not continue solving the old puzzles right through the revolutions. But if this is true then Kuhn's account which *temporally separates* periods of proliferation and periods of monism altogether collapses.³

¹ Cf. my discussion in section VI of my [1965b].

² It might be objected that the puzzle-solving activity, though not *sufficient* for bringing about a revolution, is certainly *necessary* as it creates the material which eventually leads to trouble: puzzle solving is responsible for some conditions on which scientific progress depends. This objection is refuted by the Presocratics who progressed (their theories did not just *change*, they were also *improved*) without paying the slightest attention to puzzles. Of course, they did not produce the pattern: normal science—revolution—normal science—revolution, etc., in which professional stupidity is periodically replaced by philosophical outbursts only to return again at a 'higher level'. However there is no doubt that this is an advantage as it permits us to be open-minded all the time and not only in the middle of a catastrophe. Besides—is not 'normal science' full of 'facts' and 'puzzles' which belong, not to the current paradigm, but to some *earlier predecessors*? And is it not also the case that anomalous facts are often *introduced* by the critics of a paradigm, rather than *used by them* as a starting point for criticism? And if that is true, does it not follow that it is proliferation rather than the pattern normalcy-proliferation-normalcy that characterizes

7. A PLEA FOR HEDONISM

It seems, then, that the interplay between tenacity and proliferation which we described in our little methodological fairytale is also an essential feature of the actual development of science. It seems that it is not the puzzle-solving activity that is responsible for the growth of our knowledge but the active interplay of various tenaciously held views. Moreover, it is the invention of new ideas and the attempt to secure for them a worthy place in the competition that leads to the overthrow of old and familiar paradigms. Such inventing goes on all the time. Yet it is only during revolutions that the attention turns to it. This change of attention does not reflect any profound structural change (such as for example a transition from puzzle solving to philosophical speculation and testing of foundations). It is nothing but a change of interest and of publicity.

This is the picture of science that emerges from our brief analysis. Is it an attractive picture? Does it make the pursuit of science worthwhile? Is the presence of such a discipline, the fact that we have to live with it, study it, understand it, beneficial to us, or is it perhaps liable to corrupt our understanding and diminish our pleasure?

It is very difficult nowadays to approach such questions in the right spirit. What is worthwhile and what is not are to such a large extent determined by the existing institutions and forms of life that we hardly ever arrive at a proper evaluation of these institutions themselves.¹ The sciences especially are surrounded by an aura of excellence which checks any inquiry into their beneficial effect. Phrases such as 'search for the truth', or 'highest aim of mankind' are liberally used. Undoubtedly they ennoble their object, but they also remove it from the domain of critical discussion (Kuhn has gone one step further in this direction, conferring some dignity even on the most boring and most pedestrian part of the scientific enterprise: normal science). Yet why should a product of human ingenuity be allowed to put an end to the very same questions to which it owes its existence? Why should the existence of this product prevent us from asking the most important question of all, the question to what extent the happiness of individual human beings, and to what extent their freedom, has been increased? Progress has always been achieved by probing well-entrenched and well-founded forms of life with unpopular and unfounded values. This is how man gradually freed himself from fear and from the science? So that Kuhn's position would be not only methodologically untenable (see the previous section) but also historically false?

¹ Modern analytic philosophers are trying to show that such evaluation is even *logically impossible*. In this they are but the followers of Hegel—except that they lack his knowledge, his comprehensiveness and his wit.

tyranny of unexamined systems. Our question therefore is: what values shall we choose to probe the sciences of today?

It seems to me that the happiness and the full development of an individual human being is now as ever the highest possible value. This value does not exclude the values which flow from institutionalized forms of life (truth; valour; self-negation; etc.). It rather encourages them *but only* to the extent to which they can contribute to the advance of some individual. What is excluded is the use of institutionalized values for the condemnation, or perhaps even the elimination, of those who prefer to arrange their lives in a different way. What is excluded is the attempt to 'educate' children in a manner that makes them lose their manifold talents so that they become restricted to a narrow domain of thought, action, emotion. Adopting this basic value we want a methodology and a set of institutions which enable us to lose as little as possible of what we are capable of doing and which force us as little as possible to deviate from our natural inclinations.

Now the brief methodological fairytale which we have sketched in section 6, says that a science that tries to develop our ideas and that uses rational means for the elimination of even the most fundamental conjectures must use a principle of tenacity together with a principle of proliferation. It must be allowed to *retain* ideas in the face of difficulties; and it must be allowed to introduce *new ideas* even if the popular views should appear to be fully justified and without blemish. We have also found that actual science, or at least the part of actual science that is responsible for change and for progress, is not very different from the ideal outlined in the fairytale. But this is a happy coincidence indeed! We are now in full agreement with our wishes as expressed above! Proliferation means that there is no need to suppress even the most outlandish product of the human brain. *Everyone may follow his inclinations* and science, conceived as a critical enterprise, will profit from such an activity. Tenacity: this means that one is encouraged not just to follow one's inclinations, but to develop them further, to raise them, with the help of criticism (which involves a comparison with the existing alternatives) to a higher level of articulation *and thereby to raise their defence to a higher level of consciousness*. The interplay between proliferation and tenacity also amounts to the continuation, on a new level, of the biological development of the species and it may even increase the tendency for useful *biological* mutations. It may be the only possible means of preventing our species from stagnation. This I regard as the final and the most important argument against a 'mature' science as described by Kuhn. Such an enterprise is not only ill-conceived and non-existent; its defence is also incompatible with a humanitarian outlook.

8. AN ALTERNATIVE: THE LAKATOS MODEL OF SCIENTIFIC CHANGE

Let me now present in its entirety the picture of science which I think should replace Kuhn's account.

This picture is the synthesis of the following two discoveries. First, it contains Popper's discovery that science is advanced by a critical discussion of alternative views. Secondly, it contains Kuhn's discovery of the function of tenacity which he has expressed, mistakenly I think, by postulating tenacious *periods*. The synthesis consists in Lakatos's assertion (which is developed in his own comments on Kuhn) that proliferation and tenacity do not belong to *successive* periods of the history of science, but are always *copresent*.¹

When speaking of 'discoveries' I do not mean to say that the ideas mentioned are entirely new, or that they now appear in a new form. Quite the contrary. Some of these ideas are as old as the hills. The idea that knowledge can be advanced by a struggle of alternative views and that it depends on proliferation was first put forth by the Presocratics (this has been emphasized by Popper himself), and it was developed into a general philosophy by Mill (especially in *On Liberty*). The idea that a struggle of alternatives is decisive for *science*, too, was introduced by Mach (*Erkenntnis und Irrtum*) and Boltzmann (see his *Populaerwissenschaftliche Vorlesungen*), mainly under the impact of Darwinism. The need for tenacity was emphasized by those dialectical materialists who objected to extreme 'idealistic' flights of fancy. And the synthesis, finally, is the very essence of dialectical materialism in the form in which it appears in the writings of Engels, Lenin, and Trotsky. Little of this is known to the 'analytic' or 'empiricist' philosophers of today who are still very much under the influence of the Vienna Circle. Considering this narrow, though quite 'modern' context we may therefore speak of genuine though quite belated, 'discoveries'.

According to Kuhn mature science is a *succession* of normal periods and of revolutions. Normal periods are monistic; scientists try to solve puzzles resulting from the attempt to see the world in terms of a single paradigm. Revolutions are pluralistic until a new paradigm emerges that gains sufficient support to serve as the basis for a new normal period.

This account leaves unanswered the problem how the transition from a normal period to a revolution is brought about. In section 6 we indicated

¹ Lakatos's analysis, I think, can be further improved by abandoning the distinction between theories and research programmes (cf. *above*, p. 203, footnote 2) and by allowing for incommensurability (jumps from quantity to quality in the language of dialectical materialism). Improved in this way it would be a truly dialectical account of the development of our knowledge.

how the transition could be achieved in a reasonable manner: one compares the central paradigm with alternative theories. Professor Kuhn seems to be of the same opinion. Moreover he points out that this is what actually happens. Proliferation sets in already *before* a revolution and is instrumental in bringing it about. But this means that the original account is faulty. Proliferation does not *start* with a revolution; it *precedes* it. A little imagination and a little more historical research then shows that proliferation not only *immediately precedes* revolutions, but that it is there *all the time*. Science as we know it is not a temporal succession of normal periods and of periods of proliferation; it is their *juxtaposition*.

Seen in this way the transition from pre-science to science does not *replace* the uninhibited proliferation and the universal criticism of the former by the puzzle-solving tradition of a normal science. It *supplements* it by this activity or, to express it even better, mature science *unites* two very different traditions which are often separate, the tradition of a pluralistic philosophical criticism and a more practical (and less humanitarian—see section 8) tradition which explores the potentialities of a given material (of a theory; of a piece of matter) without being deterred by the difficulties that might arise and without regard to alternative ways of thinking (and acting). We have learned from Professor Popper that the first tradition is closely connected with the cosmology of the Presocratics. The second tradition is best exemplified by the attitude of the members of a closed society towards their basic myth. Kuhn has conjectured that mature science consists in the *succession* of these two different patterns of thought and action. He is right in so far as he has noticed the normal, or conservative, or anti-humanitarian element. This is a genuine discovery. He is wrong as he has misrepresented the relation of this element to the more philosophical (i.e. critical) procedures. I suggest in accordance with Lakatos's model that the correct relation is one of *simultaneity* and *interaction*. I shall therefore speak of the normal *component* and the philosophical *component* of science and not of the normal *period* and the *period* of revolution.

It seems to me that such an account overcomes many difficulties, both logical and factual, which make Kuhn's point of view so fascinating but at the same time so unsatisfactory.¹ In considering it one should not be

¹ To take but one example, Kuhn writes (*this volume*, p. 6) that 'it is for the normal, not the extraordinary practice of science that professionals are trained; if they are nevertheless eminently successful in displacing and replacing the theories on which normal science depends, that is an oddity which must be explained'. It is certainly an oddity in Kuhn's account. In our account we only need to draw attention to the fact that revolutions are mostly made by members of the philosophical component who, while aware of the normal practice, are also able to think in a different way (in the case of Einstein the self-professed ability to escape from the normal training was essential for his freedom of thought and for his discoveries).

misled by the fact that the normal component almost always outweighs its philosophical part. For what we are investigating is not the size of a certain element of science, but its *function* (a single man can revolutionize an epoch). Nor must we be overly impressed by the fact that most scientists would *regard* the 'philosophical' component as lying outside science proper and that they could *support* this attitude by pointing to their own lack of philosophical acumen. For it is not *they* who carry out fundamental improvement but those who further the *active interaction* of the normal and the philosophical component (this interaction consists almost always in the criticism of what is well entrenched and unphilosophical by what is peripheral and philosophical). Now, granting all this, why is it that there seems to exist a definite fluctuation in the state of science? If science consists of the constant interaction of a normal and a philosophical part; if it is this interaction which advances it; then why do the revolutionary elements become visible only on such rare occasions? Is not this simple historical fact sufficient to support Kuhn's account over mine? Is it not typical philosophical sophistry to deny what is such an obvious historical fact?

I think that the answer to this question is obvious. The normal component is large and well entrenched. Hence, a change of the normal component is very noticeable. So is the resistance of the normal component to change. This resistance becomes especially strong and noticeable in periods where a change seems to be imminent. It is directed against the philosophical component and brings it into public consciousness. The younger generation, always eager for new things, seizes upon the new material and studies it avidly. Journalists, always on the lookout for headlines—the more absurd, the better—publicize the new discoveries (which are those elements of the philosophical component which most radically disagree with the current views while still possessing some plausibility and perhaps even some factual support). These are some reasons for the differences which we perceive. I do not think that one should look for anything more profound.

Now as regards the change of the normal component itself there is no reason to expect that it will follow a clearly recognizable and logical pattern. Kuhn like other philosophers before him (I am here mainly thinking of Hegel) assumes that a tremendous historical change must exhibit a logic of its own and that the change of an idea must be reasonable in the sense that there exists a link between the *fact* of change and the *content* of the idea changing. This is a plausible assumption as long as one is dealing with reasonable people: changes in the *philosophical component* most likely *can* be explained as the result of clear and unambiguous *arguments*. But to assume that people who habitually resist change; who frown at any criticism

of things dear to them; and whose highest aim is to solve puzzles on a basis that is neither known nor understood; to assume that *such* people will change their allegiance in a reasonable fashion is carrying optimism and the quest for rationality too far. The normal elements, i.e. those elements which have the support of the majority, may change because the younger generation cannot be bothered to follow their elders; or because some public figure has changed his mind; or because some influential member of the establishment has died and has failed (perhaps because of his suspicious nature) to leave behind a strong and influential school, or because a powerful and non-scientific institution pushes thought in a definite direction.¹ Revolutions, then, are the outward manifestation of a change of the normal component that cannot be accounted for in any reasonable fashion. They are substance for anecdotes though they magnify and make visible the more rational elements of science, thus teaching us what science *could* be if there were more reasonable people around.

9. THE ROLE OF REASON IN SCIENCE

(1) So far I have *criticized* Kuhn from a point of view which is almost identical with that of Lakatos. (There are some slight differences, such as my reluctance to separate theories and research programmes,² but they will be disregarded. When speaking of 'theories' I always mean theories and/or research programmes.) I now want to *defend* Kuhn against Lakatos. More specifically, I want to argue that science both is, and should be, more irrational than Lakatos and Feyerabend₁ (the Popperian₃ author of

¹ It is plausible to assume that *one* of the causes for the transition to mature science with its various 'quasi-independent' traditions is to be sought in the decree of the Roman Catholic Church against the Copernican point of view. 'This must be taken into account by those who try to explain the special development of the many individual sciences and the absence of a conscious and secure philosophical background by regarding it as a peculiarity of seventeenth-century Italian culture. . . . Such an interpretation assumes . . . that the condemnation of Galileo was but an *external* pressure which could not possibly have influenced the development of spiritual matters. However the Roman Judgement was regarded as a restriction of consciousness that could be broken only on pain of life and salvation. . . . The development of individual disciplines was allowed. Nobody was prevented from searching the heavens, from exploring physical phenomena, from thinking mathematically . . . and from furthering the material culture by such a pursuit. Priests and religious orders, even the Jesuits who were responsible for Galileo's fate, diligently pursued these restricted tasks. But individual conscience as well as the omnipresent 'directeurs de conscience', the officials, the schools, the churches, the state watched carefully this simple fight for knowledge in order that no one might dare to use its results for philosophical speculation'. (Leonardo Olshki [1927], p. 400). *This is how 'mature science' came into being*, at least in the Roman countries. Cf. also chapter IX of Wohlwill's [1926] where the development after Galileo's death is sketched in some detail.

² Cf. *above*, p. 203, footnote 2.

the preceding sections of this paper and of 'Problems of Empiricism') are prepared to admit.¹

This transition from criticism to defence does not mean that I have changed my mind. Nor can it be completely explained by my cynicism *vis-à-vis* the business of philosophy of science. It is rather connected with the nature of science itself, with its complexity, with the fact that it has different aspects, that it cannot be readily separated from the remainder of history, that it has always utilized and continues to utilize every talent and every folly of man. Contrary arguments bring out the different features it contains, they challenge us to make a decision, they challenge us to either *accept* this many-faced monster and be devoured by it, or else to *change* it in accordance with our wishes. Let us now see what can be said against the Lakatos model of scientific growth.

(2) Naive falsificationism judges (i.e. accepts, or condemns) a theory as soon as it is introduced into the discussion. Lakatos gives a theory time, he permits it to develop, he permits it to show its hidden strength, and he judges it only 'in the long run'. The 'critical standards' he employs provide for an interval of hesitation. They are applied 'with hindsight'.² They are applied *after* the occurrence of either 'progressive' or of 'degenerating' problem shifts.

Now it is easy to see that standards of this kind have practical force only if they are combined with a *time limit* (what looks like a degenerating problem shift may be the beginning of a much longer period of advance). But introduce the time limit and the argument against naive falsificationism reappears with only a minor modification (if you are permitted to wait, why not wait a little longer?) Thus the standards which Lakatos wants to defend are either *vacuous*—one does not know when to apply them—or they can be *criticized* on grounds very similar to those which led to them in the first place.

In these circumstances one can do one of the following two things. One can *stop* appealing to permanent standards which remain in force throughout history and govern every single period of scientific development and every transition from one period to another. Or one can retain such standards as a *verbal ornament*, as a memorial to happier times when it was still thought possible to run a complex and often catastrophic business like science by following a few simple and 'rational' rules. It seems that Lakatos wants to choose the second alternative.

¹ The indices are intended as an ironical criticism of Lakatos [1968b] where the practice of splitting a guy into three was first introduced. (Also cf. *this volume*, p. 181.) This practice has created a lot of confusion and has slowed down philosophers in their attempt to find the weak spots of critical rationalism.

² *This volume*, pp. 134, 158, and 173.

(3) Choosing the second alternative means abandoning permanent standards *in fact* though retaining them *in words*. *In fact*, Lakatos's position now seems to be identical with the position of Popper as summarized in a (because self-destructive) marvellous addendum of the fourth edition of the *Open Society*.¹ According to Popper we do not 'need any . . . definite frame of reference for our criticism', we may revise even the most fundamental rules and drop the most fundamental demands if the need for a different measure of excellence should arise.² Is such a position irrational? Does it imply that science is irrational? *Yes and no*. *Yes*—because there no longer exists a single set of rules that will guide us through all the twists and turns of the history of thought (science), either as participants, or as historians who want to reconstruct its course. One can of course *force* history into such a pattern, but the results will always be poorer and much less interesting than were the actual events. *No*—because each particular episode is rational in the sense that some of its features can be explained in terms of reasons which were either accepted at the same time as its occurrence, or invented in the course of its development. *Yes*—because even these logical reasons which change from age to age are never sufficient to explain *all* the important features of a particular episode. We must add accidents, prejudices, material conditions (such as the existence of a particular type of glass in one country and not in another), the vicissitudes of married life, oversight, superficiality, pride, and many other things in order to get a complete picture. *No*—because transported into the climate of the period under consideration and endowed with a lively and curious intelligence we might have had still more to say, we might have tried to overcome accidents, and to 'rationalize' even the most whimsical sequence of events. But—and now we come to a decisive point—how is the transition from certain standards to other standards to be achieved? More especially, what happens to our standards (as opposed to our theories) during a period of revolution? Are they changed in the Popperian manner, by a critical discussion of alternatives, or are there processes which defy a rational analysis? This is one of the questions raised by Kuhn. Let us see what answer we can give to it!

(4) That standards are not always adopted on the basis of argument has been emphasized by Popper himself. Children, he says, 'learn to imitate others . . . and so learn to look upon standards of behaviour as if they consisted of fixed, "given" rules . . . and such things as sympathy and imagination may play an important role in this development'.³ Similar considerations apply to those grownups who want to continue learning and

¹ Popper [1961], p. 388.

² *Loc. cit.* p. 390.

³ *Loc. cit.* p. 390.

who are intent on expanding both their knowledge and their sensibility. We certainly cannot assume that what is possible in the case of children—to slide, on the smallest provocation, into entirely new reaction patterns—should be beyond the reach of adults and inaccessible to one of the most outstanding adult activities, science. Moreover, it is likely that catastrophic changes, frequent disappointment of expectations, crises in the development of our knowledge will change and, perhaps, multiply reaction patterns (including patterns of argumentation) just as an ecological crisis multiplies mutations. This may be an entirely *natural* process, like growing in size, and the only function of rational discourse may consist in increasing the mental tension that precedes *and causes* the behavioural outburst. Now—is this not exactly the kind of change we may expect at periods of scientific revolution? Does it not restrict the effectiveness of arguments (except as a causative agent leading to developments very different from what is demanded by their *content*)? Does not the occurrence of such a change show that science which, after all, is part of the evolution of man is not entirely rational and cannot be entirely rational? For if there are events, not necessarily arguments which *cause* us to adopt new standards, will it then not be up to the defenders of the status quo to provide, not just arguments, but also *contrary causes*? And if the old forms of argumentation turn out to be too weak a contrary cause, must they then not either give up, or resort to stronger and more 'irrational' means? (It is very difficult, and perhaps entirely impossible, to combat the effects of brainwashing by argument.) Even the most puritanical rationalist will then be forced to leave argument and to use, say, *propaganda* not because some of his arguments have ceased to be *valid*, but because the *psychological conditions* which enable him to effectively argue in this manner and thereby to influence others have disappeared. And what is the use of an argument that leaves people unmoved?

(5) Considering questions such as these a Popperian will reply that new standards may indeed be discovered, invented, accepted, imparted upon others in a very irrational manner, but that there always remains the possibility to criticize them *after* they have been adopted and that it is this possibility which keeps our knowledge rational. 'What, then, are we to trust?' asks Popper after a survey of possible sources for standards.¹ 'What are we to accept? The answer is: whatever we accept we should trust only tentatively, always remembering that we are in possession, at best, of partial truth (or rightness), and that we are bound to make at least some mistake or misjudgement somewhere—not only with respect to facts but also with respect to the adopted standards; secondly, we should trust (even

¹ *Loc. cit.* p. 391.

tentatively) our intuition only if it has been arrived at as the result of many attempts to use our imagination; of many mistakes, of many tests, of many doubts, and of searching criticism.'

Now this reference to tests and to criticism which is supposed to guarantee the rationality of science and, perhaps, of our entire life may be either to *well defined procedures* without which a criticism or test cannot be said to have taken place, or it may be purely *abstract* so that it is left to us to fill it now with this, and now with that concrete content. The first case has just been discussed. In the second case we have but a verbal ornament, just as Lakatos's defence of his own 'objective standards' turned out to be a verbal ornament. The questions of section 4 remain unanswered in either case.

(6) In a way even this situation has been described by Popper who says that 'rationalism is necessarily far from comprehensive or self-contained'.¹ But the question raised by Kuhn is not whether *there are* limits to our reason; the question is *where* these limits are *situated*. Are they outside the sciences so that science itself remains entirely rational, or are irrational changes an essential part of even the most rational enterprise that has been invented by man? Does the historical phenomenon 'science' contain ingredients which defy a rational analysis? Can the abstract aim to come closer to the truth be reached in an entirely rational manner, or is it perhaps inaccessible to those who decide to rely on argument only? These are the problems to which we must now address ourselves.

(7) Considering these further problems Popper and Lakatos reject 'mob psychology'² and assert the rational character of *all* science. According to Popper it is possible to arrive at a judgement as to which of two theories is closer to the truth, even if the theories should be separated by a catastrophic upheaval such as a scientific revolution. (A theory *T* is closer to the truth than another theory, *T'*, if the class of the true consequences of *T'*, the so-called truth content of *T'*, exceeds the class of true consequences of *T* without an increase in the falsity content.) According to Lakatos the apparently unreasonable features of science occur only in the material world and in the world of (psychological) thought; they are absent from the 'world of ideas, [from] Plato's and Popper's "third world"'.³ It is in this third world that the growth of knowledge takes place and that a rational judgement of all aspects of science becomes possible. It must be pointed out, however, that the scientist is unfortunately dealing with the world of matter and of (psychological) thought also and that the rules which create order in the third world may be entirely inappropriate for creating order

¹ Popper [1945], chapter 24.

³ *This volume*, p. 180.

² *This volume*, p. 178.

in the brains of living human beings (unless these brains and their structural features are put into the third world, a point that does not become clear from Popper's account).¹ The numerous deviations from the straight path of rationality which we observe in actual science may well be *necessary* if we want to achieve progress with the brittle and unreliable material (instruments; brains; etc.) at our disposal.

However there is no need to pursue this objection further. There is no need to argue that real science may differ from its third world image *in precisely those respects* which make progress possible.² For the Popperian model of an approach to the truth breaks down even if we confine ourselves to ideas entirely. It breaks down because there are *incommensurable theories*.

(8) With the discussion of incommensurability, I come to a point of Kuhn's philosophy which I wholeheartedly accept. I am referring to his assertion that succeeding paradigms can be evaluated only with difficulty and that they may be altogether incomparable, at least as far as the more familiar standards of comparison are concerned (they may be readily comparable in other respects). I do not know who of us was the first to use the term 'incommensurable' in the sense that is at issue here. It occurs in Kuhn's '*Structure of Scientific Revolutions*' and in my essay 'Explanation, Reduction, and Empiricism' both of which appeared in 1962. I still remember marvelling at the pre-established harmony that made us not only defend similar ideas but use exactly the same words for expressing them. The coincidence is of course far from mysterious. I had read earlier drafts of Kuhn's book and had discussed their content with Kuhn. In these discussions we both agreed that new theories, while often better and more detailed than their predecessors were not always rich enough to deal with *all* the problems to which the predecessor had given a definite and precise answer. The growth of knowledge or, more specifically, the replacement of one comprehensive theory by another involves losses as well as gains. Kuhn was fond of comparing the scientific world view of the seventeenth century with the Aristotelian philosophy, while I used more recent examples such as the theory of relativity and the quantum theory. We also saw that it might be extremely difficult to compare successive theories in

¹ I am here referring to Popper [1968a] and Popper [1968b]. In the first paper birdnests are assigned to the 'Third World' (p. 341) and an interaction is assumed between them and the remaining worlds. They are assigned to the Third World *because of their function*. But then stones and rivers can be found in this third world, too, for a bird may sit on a stone, or take a bath in a river. As a matter of fact, everything that is noticed by some organism (and therefore plays a role in his *Umwelt*) will be found in the third world which will therefore contain the whole material world and all the mistakes mankind has made. It will also contain 'mob psychology'.

² Cf. my [1969].

the usual manner, that is, by an examination of consequence classes. The accepted scheme is as follows (*Fig. 1*): T is superseded by T' . T' explains why T fails where it does (in F); it also explains why T has been at least partly successful (in S); and makes additional predictions, (A). Now if this scheme is to work then there must be statements which follow (with, or without the help of definitions and/or correlation hypotheses) both from T and from T' . But there are cases which invite a comparative judgement without satisfying the conditions just stated. The relation between such theories is as shown in *Fig. 2*.¹ A judgement involving a comparison of content classes is now clearly impossible. For example, T' cannot be said to be either closer to, or farther from, the truth, than T .

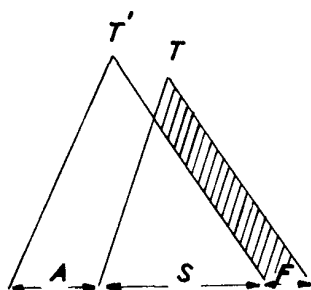


Fig. 1

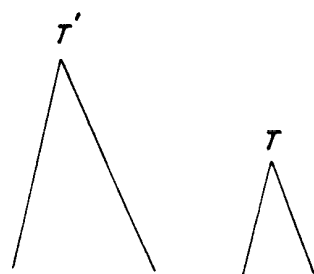


Fig. 2

(9) As an example of two incommensurable theories let us briefly discuss classical celestial mechanics (CM) and the special theory of relativity (SR). To start with one should emphasize that the question 'are CM and SR incommensurable?' is not a complete question. Theories can be interpreted in different ways. They will be commensurable in some interpretations, incomparable in others. Instrumentalism, for example, makes commensurable all those theories which are related to the same observation language and are interpreted on its basis. A realist, on the other hand, wants to give a unified account, both of observable and of unobservable matters, and he will use the most abstract terms of whatever theory he is contemplating for that purpose. This is an entirely natural procedure. SR, so one would be inclined to say, does not just invite us to rethink *unobserved* length, mass, duration; it would seem to entail the relational character of *all* lengths, masses, durations, whether observed or unobserved, observable or unobservable. Now extending the concepts of a new theory T to all its consequences, observational reports included, may change the interpretation of these consequences to such an extent that they disappear from the consequence classes of earlier theories. These earlier theories will then all

¹ The area below T' should be imagined as lying either *in front of* the area below T , or *behind it*, so that there is no overlap.

become incommensurable with T . The relation between SR and CM is a case in point. The concept of length as used in SR and the concept of length as presupposed in CM are different concepts. Both are *relational* concepts, and very complex relational concepts at that (just consider determination of length in terms of the wave length of a specified spectral line). But relativistic length (or relativistic *shape*) involves an element that is absent from the classical concept and is in principle excluded from it.¹ It involves the *relative velocity* of the object concerned in some reference system. It is of course true that the relativistic scheme very often gives us *numbers* which are practically identical with the numbers we get from CM—but this does not make the *concepts* more similar. Even the case $c \rightarrow \infty$ (or $v \rightarrow \infty$) which gives *strictly identical* predictions cannot be used as an argument for showing that the concepts must coincide at least in this case: different magnitudes based on different concepts may give identical

¹ It is possible to base space time frames on this new element only and to avoid contamination by earlier modes of thought. All one has to do is to replace distances by light-times and to treat time intervals in the relativistic fashion, for example, by using the k -calculus. (Cf. chapter II of Synge [1964]. For the k -calculus, cf. Bondi [1967], pp. 29 ff., as well as Bohm [1965], chapter xxvi.) The resulting concepts (of distance, velocity, time, etc.) are a necessary part of relativity in the sense that all further ideas such as the idea of length as defined by the transport of rigid rods must be changed and adapted to them. They therefore suffice for explaining relativity.

Marzke and Wheeler [1963] have given a detailed account of the way in which the theory of relativity can be freed from external ingredients. They adopt the principle, ascribed by them to Bohr and Rosenfeld, 'that every proper theory should provide in and by itself its own means for defining the quantities with which it deals. According to this principle classical general relativity should admit to calibrations of space and time that are altogether free of any reference to the quantum of action [for atomic clocks, or minimal distances]' or to 'rigid rods' as described by, say, the non relativistic theory of elasticity (p. 48). They proceed to construct clocks and meters which use the properties of light and of inertial particle trajectories only (pp. 53-6). Equality of distances measured by such clocks and meters is intransitive in a classical universe, transitive in a relativistic universe. The results of distance measurements of this kind are invariant to translations in a relativistic universe, not so invariant in a classical universe. Two different events are always separated by a finite distance in a relativistic universe, they are not always so separated in a classical universe. The unity of measurement in the relativistic universe is the interval between the two effective equinoxes of 1900 and it can be compared with any interval (spatial or temporal) in an invariant way. No such comparison is possible in the classical case (p. 62). 'The number $3 \cdot 10^8$ never shows itself. The importance of lightrays and the lightcone in the intrinsic geometry of physics comes more directly to the surface. The true function of the speed of light is no longer confused with the trivial task of relating two separate units of interval, the meter and the second, of purely historical and accidental origin' (p. 56). General relativity theory, then, can be shown to 'provide its own means of defining intervals of space and time' (p. 62) and the intervals so defined are incommensurable with classical intervals.

Space forbids to argue this interesting case in detail but it is hoped that those who are turned on by the problem of incommensurability will use Marzke and Wheeler as a basis for concrete discussion.

values on their respective scales without ceasing to be different magnitudes (the same remark applies to the attempt to identify classical mass with relative *rest* mass).¹ This conceptual disparity, if taken seriously, infects even the most 'ordinary' situations: the relativistic concept of a certain shape, such as a table, or of a certain temporal sequence, such as my saying 'yes', will differ from the corresponding classical concept also. It is therefore vain to expect that sufficiently long derivations may eventually return us to the older ideas.² The consequence classes of SR and CM are related as in Fig. 2. A comparison of content and a judgement of verisimilitude cannot be made.³

(10) In what follows I shall discuss a few objections which have been raised, not against this *particular* analysis of the relation between SR and CM, but against the very *possibility*, or *desirability* of incommensurable theories (almost all objections against incommensurability are of this general kind). They express methodological ideas which we must criticize if we want to increase our freedom *vis-à-vis* the sciences.

One of the most popular objections proceeds from the version of realism that I just described in (9). 'A realist', we said, 'wants to give a unified account, both of observable and of unobservable matters, and he will use the most abstract terms of whatever theory he is contemplating for that purpose'. He will use such terms in order to either *give* meaning to observation sentences, or else to *replace* their customary interpretation (for example, he will use the ideas of SR in order to replace the customary CM-interpretation of everyday statements about shapes, temporal sequences, and so on). As against this it is pointed out that theoretical terms receive their interpretation by being connected either with a pre-existing observation language, or with another theory that has already been connected with such an observation language and that they are devoid of content without such a connection. Thus Carnap asserts⁴ that 'there is no independent interpretation for L_T [the language in terms of which a certain theory, or a certain world view, is formulated]. The system T [consisting of the axioms of the theory and the rules of derivation] is itself an uninterpreted postulate

¹ For this point and further arguments, cf. Eddington [1924], p. 33.

² This takes care of an objection which John Watkins has raised on various occasions.

³ For further details, especially concerning the concept of mass, the function of 'bridge laws' or 'correspondence rules', and the two-language model, cf. section IV of my [1965b]. It is clear that, given the situation described in the text, we cannot derive classical mechanics from relativity, not even approximately (for example, we cannot derive the classical law of mass conservation from a corresponding relativistic law). The possibility to connect the formulae of the two disciplines in a manner that might satisfy a pure mathematician (or an instrumentalist) is however not excluded. For an analogous situation in the case of quantum mechanics cf. section 3 of my [1968-9]. Cf. also section 2 of the same article for more general considerations.

⁴ Cf. Carnap [1956], p. 47.

system. [Its] terms obtain only an indirect and incomplete interpretation by the fact that some of them are connected by the [correspondence rules] C with observational terms'. Now, if theoretical terms have no 'independent interpretation' then they cannot be used for correcting the interpretation of the observation statements which is the one and only source of their meaning. It follows that realism as described by us is an impossible doctrine.

The guiding idea behind this objection is that new and abstract languages cannot be introduced in a direct way but must be first connected with an already existing, and presumably stable, observational idiom.¹

This guiding idea is refuted at once by pointing to the way in which children learn to speak and in which anthropologists and linguists learn the unknown language of a newly discovered tribe.

The first example is instructive for other reasons also, for incommensurability plays an important role in the early months of human development. As has been suggested by Piaget and his school,² the child's perception develops through various stages before it reaches its relatively stable adult form. In one stage objects seem to behave very much like after-images³—and they are treated as such: the child follows the object with his eyes until it disappears and he does not make the slightest attempt to recover it even if this would require a minimal physical (or intellectual) effort, an effort moreover, that is already within the child's reach. There is not even a tendency to search—and this is quite appropriate, 'conceptually' speaking. For it would indeed be nonsensical to 'look for' an afterimage. Its 'concept' does not provide for such an operation.

The arrival of the concept, and of the perceptual image, of material objects changes the situation quite dramatically. There occurs a drastic reorientation of behavioural patterns and, so one may conjecture, of thought. Afterimages or things somewhat like them still exist, but they are now difficult to find and must be discovered by special methods (the earlier visual world therefore *literally disappears*). Such methods proceed from a new conceptual scheme (afterimages occur in *humans*, not in the outer physical world, and are tied to them) and cannot lead back to the exact

¹ An even more conservative principle is sometimes used when discussing the possibility of languages with a logic different from our own. Thus Stroud, in his [1968], *discussing*, and not just stating the principle, says that 'any allegedly new possibility must be capable of being fitted into, or understood in terms of, our present conceptual or linguistic apparatus' from which it follows (172) that 'any "alternative" is either something we already understand and can make sense of, or it is no alternative at all'. What is overlooked is that an initially ununderstood alternative may be *learned* in the way in which one learns a new and unfamiliar language, not by *translation*, but by *living* with the members of the community where the language is spoken.

² As an example the reader is invited to consult Piaget [1954].

³ Piaget [1954], pp. 5 ff.

phenomena of the previous stage (these phenomena should therefore be called by a different name, such as 'pseudo-afterimages'). Neither afterimages, nor pseudo-afterimages are given a special position in the new world. For example, they are not treated as *evidence* on which the new notion of a material object is supposed to rest. Nor can they be used to *explain* this notion: afterimages arise *together with it* and are absent from the mind of those who do not yet recognize material objects; and pseudo-afterimages *disappear* as soon as such recognition takes place. It is to be admitted that every stage possesses a kind of observational 'basis' to which one pays special attention and from which one receives a multitude of suggestions. However this basis (1) *changes* from stage to stage; and (2) it is *part* of the conceptual apparatus of a given stage, *not* its one and only source of interpretation.

Considering developments such as these we may suspect that the family of concepts centering upon 'material object' and the family of concepts centering upon 'pseudo-afterimages' are incommensurable in precisely the sense that is at issue here. Is it reasonable to expect that conceptual changes of this kind occur only in childhood? Should we welcome the fact—if it is a fact—that an adult is stuck with a stable perceptual world and an accompanying stable conceptual system which he can modify in many ways but whose general outlines have forever become immobilized? Or is it not more realistic to assume that fundamental changes, entailing incommensurability, are still possible, and that they should be encouraged lest we remain forever excluded from what might be a higher stage of knowledge and of consciousness? Besides, the question of the mobility of the adult stage is at any rate an empirical question which must be attacked by *research* and cannot be settled by methodological *fiat*. An attempt to break through the boundaries of a given conceptual system and to escape the range of 'Popperian spectacles'¹ is an essential part of such research.²

(II) Looking now at the second element of the refutation—anthropological field work—we see that what is anathema here (and for very good

¹ Cf. Lakatos's paper, *this volume*, p. 179, footnote 1.

² For the condition of research formulated in the last sentence, cf. section 8 of my [1965a]. For the role of observation cf. section 7 of the same article. For the application of Piaget's work to physics and, more especially, to the theory of relativity, cf. the appendix of Bohm [1965]. Bohm and Schumacher have also carried out an analysis of the different informal structures which underlie our theories. One of the main results of their work is that Bohr and Einstein argued from incommensurable points of view. Seen in this way the case of Einstein, Podolski and Rosen cannot refute the Copenhagen interpretation, and it cannot be refuted by it. The situation is rather that we have two theories, one permitting us to formulate the Einstein-Podolski-Rosen thought-experiment, the other not providing the machinery necessary for such a formulation so that we must find independent means of deciding which one to adopt. For further comments on this problem, cf. section 9 of my [1968-9].

reasons) is still a fundamental principle for the contemporary representatives of the philosophy of the Vienna Circle. According to Carnap, Feigl, Nagel, and others the terms of a theory receive their interpretation, in an indirect fashion, by being related to a different conceptual system which is either an older theory, or an observation language.¹ Older theories, or observation languages are adopted not because of their theoretical excellence (they cannot possibly be: the older theories are usually refuted). They are adopted because they are 'used by a certain language community as a means of communication'.² According to this method, the phrase 'having much larger relativistic mass than . . .' is partially interpreted by first connecting it with some *prerelativistic terms* (classical terms; common-sense terms) which are 'commonly understood' (presumably as the result of previous teaching in connection with crude weighing methods). This is even worse than the once quite popular demand to clarify doubtful points by translating them into Latin. For while Latin was chosen because of its precision and clarity and also because it was conceptually richer than the slowly evolving vulgar idioms, the choice of an observation language or of an older theory as a basis for interpretation is due to the fact that they are 'antedecently understood', it is due to their *popularity*. Besides, if prerelativistic terms which are pretty far removed from reality—especially in view of the fact that they come from an incorrect theory—can be taught ostensibly, for example, with the help of crude weighing methods (and we must assume that they can be so taught, or the whole scheme collapses) then why should we not introduce the relativistic terms *directly*, and *without* assistance from the terms of some other idiom? Finally, it is but plain commonsense that the teaching, or the learning, of new and unknown languages must not be contaminated by external material. Linguists remind us that a perfect translation is never possible, even if we use complex contextual definitions. This is one of the reasons for the importance of *field work* where new languages are learned *from scratch* and for the rejection, as inadequate, of any account that relies on (complete, or partial) translation. *Yet just what is anathema in linguistics is now taken for granted by logical empiricists*, a mythical 'observation language' replacing the English of the translators. Let us commence field work in this domain also and let us study the language of new theories not in the definition factories of the double language model, but in the company of those metaphysicians, experimenters, theoreticians, playwrights, courtesans, who have constructed new world views! This finishes our discussion of the guiding principle of the first objection against realism and the possibility of incommensurable theories.

¹ For what follows, cf. also my [1966].

² Carnap [1956], p. 40. Cf. also Hempel [1966], pp. 74 ff.

(12) Next I shall deal with a mixed bag of asides which have never been presented in a systematic fashion and which can be disposed of in a few words.

To start with, there is the suspicion that observations which are interpreted in terms of a new theory can no longer be used to refute that theory. The suspicion is allayed by pointing out that the predictions of a theory depend on its postulates, the associated grammatical rules *as well as* on initial conditions, while the meaning of the primitive notions depends on the postulates (and the associated grammatical rules) only: it is possible to refute a theory by an experience that is entirely interpreted in its terms.

Another point that is often made is that there exist *crucial experiments* which refute one or two allegedly incommensurable theories and confirm the other, for example: the Michelson–Morley experiment, the variation of the mass of elementary particles, the transversal Doppler effect refute CM and confirm SR. The answer to this problem is not difficult either: adopting the point of view of relativity we find that the experiments *which of course will now be described in relativistic terms*, using the relativistic notions of length, duration, speed, and so on,¹ are relevant to the theory and we shall also find that they support the theory. Adopting CM (with, or without an aether) we again find that the experiments (which are now described in the very different terms of classical physics, roughly in the manner in which Lorentz described them) are relevant, but we also find that they *undermine* (the conjunction of classical electrodynamics and of) CM. Why should it be necessary to possess terminology that allows us to say that it is the *same* experiment which confirms one theory and refutes the other? But did we not ourselves use such terminology? Well, for one thing it should be easy, though somewhat laborious, to express what was just said *without* asserting identity. Secondly, the identification is of course not contrary to our thesis, for we are now not *using* the terms of either relativity, or of classical physics, as is done in a test, but are *referring to* them and their relation to the physical world. The language in which *this* discourse is carried out can be classical, or relativistic, or ordinary. It is no good insisting that scientists act as if the situation were much less complicated. If they act that way, then they are either instrumentalists (see above, section 9) or mistaken: many scientists are nowadays interested in *formulae* while we are discussing *interpretations*. It is also possible that being well acquainted with both CM and SR they change back and forth between these theories with such speed that they seem to remain within a single domain of discourse.

(13) It is also said that in admitting incommensurability into science we

¹ For examples of such descriptions cf. Synge [1964].

can no longer decide whether a new view explains what it is supposed to explain or whether it does not wander off into different fields. For example, we would not know whether a newly invented physical theory is still dealing with problems of space and time or whether its author has not by mistake made a biological assertion. But there is no need to possess such knowledge. For once the fact of incommensurability has been admitted the question which underlies the objection does not arise (conceptual progress often makes it impossible to ask certain questions; thus we can no longer ask for the absolute velocity of an object—at least as long as we take relatively seriously). Yet is this not a serious loss for science? Not at all! Progress was made by the very same ‘wandering off into different fields’ whose undecidability now so greatly exercises the critic: Aristotle saw the world as a *superorganism*, that is, as a *biological* entity, while one essential element of the new science of Descartes, Galileo, and of their followers in medicine and in biology is its exclusively *mechanistic* outlook. Are such developments to be forbidden? And if they are not, then what is left of the complaint?

A closely connected objection starts from the notion of *explanation*, or *reduction*, and emphasizes that this notion presupposes continuity of concepts (other notions could be used for starting exactly the same kind of argument). Now to take our above example, relativity is supposed to explain the valid parts of classical physics, hence it cannot be incommensurable with it! The reply is again obvious. Why should the relativist be concerned with the fate of classical mechanics except as part of a historical exercise? There is *only one* task we can legitimately demand of a theory and it is that it should give us a correct account of the *world*. What have the principles of explanation got to do with this demand? Is it not reasonable to assume that a point of view such as the point of view of classical mechanics that has been found wanting in various respects cannot have entirely adequate concepts, and is it not equally reasonable to try replacing its concepts by those of a more successful cosmology? Besides, why should the notion of explanation be burdened by the demand for conceptual continuity? This notion has been found to be too narrow before (demand of derivability) and it had to be widened so as to include partial and statistical connections. Nothing prevents us from widening it still further to admit, say, ‘explanation by equivocation’.

(14) Incommensurable theories, then, can be *refuted* by reference to their own respective kinds of experience (in the absence of commensurable alternatives these refutations are quite weak, however).¹ Their *content* cannot be compared. Nor is it possible to make a judgement of *verisimilitude*

¹ For this point cf. section 1 of my [1965a], as well as my [1965b].

except within the confines of a particular theory. None of the methods which Popper wants to use for rationalizing science can be applied and the one that can be applied, refutation, is greatly reduced in strength. What remains are aesthetic judgements, judgements of taste, and our own subjective wishes. Does this mean that we are ending up in subjectivism? Does this mean that science has become arbitrary, that it has become one element of the general relativism which Popper wants to attack? Let us see.

To start with, it seems to me that an enterprise whose human character can be seen by all is preferable to one that looks 'objective', and impervious to human actions and wishes.¹ The sciences, after all, are our own creation, including all the severe standards they seem to impose upon us. It is good to be constantly reminded of this fact. It is good to be constantly reminded of the fact that science as we know it today is not inescapable and that we may construct a world in which it plays no role whatever (such a world, I venture to suggest, would be more pleasant than the world we live in today). What better reminder is there than the realization that the choice between theories which are sufficiently general to provide us with a comprehensive world view and which are empirically disconnected may become a matter of taste? That the choice of our basic cosmology may become a matter of taste?

Secondly, matters of taste are not completely beyond the reach of argument. Poems, for example, can be compared in grammar, sound structure, imagery, rhythm, and can be evaluated on such a basis (cf. Ezra Pound on progress in poetry).² Even the most elusive mood can be analysed, *and must be* analysed if the purpose is to present it in a manner that can either be enjoyed, or that increases the emotional (cognitive, perceptual) inventory of the reader. Every poet who is not completely irrational compares, improves, argues until he finds the correct formulation of what he wants to say.³ Would it not be marvellous if this process played a role in the sciences also?

Finally, there are more pedestrian ways of explaining the same matter which may be somewhat less repulsive to the ears of a professional philosopher of science. We may consider the *length* of derivations leading from

¹ For this problem of 'alienation' cf. Marx [1844a] and [1844b].

² Popper has repeatedly asserted, both in his lectures, and in his writings that while there is progress in the sciences there is no progress in the arts. He bases his assertion on the belief that the content of succeeding theories can be compared and that a judgement of verisimilitude can be made. The refutation of this belief eliminates an important difference (and perhaps the *only* important difference) between science and the arts and makes it possible to speak of styles and preferences in the first, and of progress in the second.

³ Cf. Brecht [1964], p. 119. In my lectures on the theory of knowledge I usually present and discuss the thesis that finding a new theory for given facts is like finding a new production for a well-known play. For painting, cf. also Gombrich [1960].

the principles of a theory to its observation language, and we may also draw attention to the number of *approximations* made in the course of the derivation (all derivations must be standardized for this purpose so that an unambiguous judgement of length can be made; this standardization concerns the *form* of the derivation, it does not concern the *content* of the concepts used). Smaller length and smaller number of approximations would seem to be preferable. It is not easy to see how this requirement can be made compatible with the demand for simplicity and generality which, so it seems, would tend to increase both parameters. However that may be—there are many ways open to us once the fact of incommensurability is understood, and taken seriously.

(15) I started by pointing out that scientific method, as softened up by Lakatos, is but an ornament which makes us forget that a position of 'anything goes' has in fact been adopted. I then considered the argument that the method of problemshifts, while perhaps useless in the first world might still give a correct account of what goes on in the third world and that it might permit us to view the whole 'third world' through 'Popperian spectacles'. The reply was that there is trouble in the third world also and that the attempt to judge cosmologies by their content may have to be given up. Such a development, far from being undesirable, changes science from a stern and demanding mistress into an attractive and yielding courtesan who tries to anticipate every wish of her lover. Of course, it is up to us to choose either a dragon or a pussy cat for our company. I do not think I need to explain my own preferences.

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Reflections on my Critics¹

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1. *Introduction.*
2. *Methodology: the role of history and sociology.*
3. *Normal Science: its nature and functions.*
4. *Normal Science: its retrieval from history.*
5. *Irrationality and Theory-Choice.*
6. *Incommensurability and Paradigms.*

I. INTRODUCTION

It is now four years since Professor Watkins and I exchanged mutually impenetrable views at the International Colloquium in the Philosophy of Science held at Bedford College, London. Rereading our contributions together with those that have since accreted to them, I am tempted to posit the existence of two Thomas Kuhns. Kuhn₁ is the author of this essay and of an earlier piece in this volume. He also published in 1962 a book called *The Structure of Scientific Revolutions*, the one which he and Miss Masterman discuss above. Kuhn₂ is the author of another book with the same title. It is the one here cited repeatedly by Sir Karl Popper as well as by Professors Feyerabend, Lakatos, Toulmin, and Watkins. That both books bear the same title cannot be altogether accidental, for the views they present often overlap and are, in any case, expressed in the same words. But their central concerns are, I conclude, usually very different. As reported by his critics (his original has unfortunately been unavailable to me), Kuhn₂ seems on occasion to make points that subvert essential aspects of the position outlined by his namesake.

Lacking the wit to extend this introductory fantasy, I will instead explain why I have embarked upon it. Much in this volume testifies to what I described above as the gestalt-switch that divides readers of my *Scientific Revolutions* into two groups. Together with that book, this collection of essays therefore provides an extended example of what I have elsewhere called partial or incomplete communication—the

¹ Though my battle with a publication deadline allowed them almost no time for it, my colleagues C. G. Hempel and R. E. Grandy both managed to read my first manuscript and offer useful suggestions for its improvement, conceptual and stylistic. I am most grateful to them, but they should not be blamed for my views.