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THE WAYS IN WHICH THE METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMMES IMPROVES ON POPPER'S METHODOLOGY

1. INTRODUCTION

A theory is scientific rather than pseudoscientific if it is capable of receiving genuine 'support' from the 'facts'. One scientific theory is better than another rival theory if it is better supported by the facts than its rivals. Although some would reject the term 'support' and replace it by 'confirm' or 'corroborate', most recent attempts to provide an objective and generally applicable criterion of scientific merit have started essentially from these two assumptions. But when does a fact provide genuine support for a theory? And when do the facts support one theory better than another?

Popper's answers to these questions have developed over time in response to criticisms and open problems. But, rather than delve into the intricacies of this development, I shall take as definitive the answers developed in the preceding chapter. I shall (in Sections 2 and 3) contrast these Popperian answers with those provided by the methodology of scientific research programmes and I shall argue that these latter answers are better.[1]

The methodology of research programmes in providing new answers to these fundamental questions *corrects* Popper's methodology in two important ways. It also recognises, as we shall see, the programmatic aspect of scientific achievements and it imports this aspect into methodological appraisals. In these two ways it *goes beyond* Popper's methodology. I deal with this in Section 4.

Finally, having given an outline of the general features of this methodology, I consider (in Section 5) the account it gives of scientific revolutions. I shall try to show that it can explain the rationale of certain scientific developments about which Popper's theory of corroboration remains silent.[2]

2. WHEN DOES A FACT SUPPORT A THEORY?

According to Popper's theory of corroboration, that a description of a 'fact' is deducible from some theory (together with suitable auxiliary

A unified bibliography can be found on pp. 379–383.

G. Radnitzky and G. Andersson (eds.), Progress and Rationality in Science, 45–70.

assumptions) does not guarantee that that 'fact' supports the theory.[3] In his approach an empirically accepted consequence of a theory supports the theory only if it describes the outcome of a 'severe test' of it. According to Popper, a test of a theory is *not* severe if the theory (together with 'background knowledge') predicts the same outcome as is predicted by 'background knowledge' alone.[4] Thus, for Popper empirical support is not a simple two-place relation between theory and evidence, but rather a three-place relation between theory, evidence and 'background knowledge'. Simplifying slightly, the relation holds whenever the evidence is implied by the theory but not by 'background knowledge'.[5]

Of what does 'background knowledge' consist? According to Popper it consists of all those statements provisionally accepted by the scientific community as unproblematic at the time of the test (though any part of it may come in for critical revision later).[6] With this characterisation of background knowledge, the Popperian account of empirical support says that a theory is supported by any fact which it describes correctly and which was first discovered as a result of testing this theory; and that a fact which was already known before the theory's proposal does not support it.[7]

One of the arguments in favour of this account of factual support with its strong temporal or historical element has been that it captures more of scientists' intuitive decisions about confirmation in particular cases. There are many cases, for example, in which each of two rival theories has had some factual statement as a consequence and yet most scientists have intuitively regarded the fact concerned as genuinely supporting only *one* of the theories. I agree that it is more successful in this respect than earlier atemporal theories of 'confirmation'.[8] But I shall argue that it is not so successful in this respect as the methodology of scientific research programmes. I shall now describe two cases where two theories had the same empirical consequence, acceptance of which was regarded as supporting only *one* of them; and I will show that the Popperian account sketched above captures scientists' intuitions in only one of the two cases.

The first case is one often cited by Lakatos. According to him,[9] the Cartesians kept managing to produce theories which 'explained' *post hoc* those facts with which their earlier theories had not dealt but which had been predicted by Newton's theory. Most scientists, however regarded these facts as supporting Newton's theory but not the Cartesian theories.

The second case concerns the perihelion of Mercury. The facts about the precession of Mercury's perihelion are regarded as strongly supporting the general theory of relativity but not as supporting Newton's theory – despite the fact that this phenomenon can now be fully explained classically by making suitable subsidiary assumptions.[10]

The Popperian corroboration theory can easily explain scientists' intuitive judgements in the first case. When Newton's theory was first proposed, it predicted certain hitherto unknown effects. The observation of these effects thus supported Newton's theory. Then, however, knowledge of these effects became part of 'background knowledge'. Thus no theory which was proposed subsequently could derive support from these facts. This applies in particular to the Cartesian theories mentioned.

However, in the case of the perihelion of Mercury, Popperian corroboration theory *fails* to capture scientists' intuitions about empirical support. For the facts about Mercury were known (*i.e.* were part of background knowledge) long before the proposal of either the general theory of relativity or the classical theory which explains them. And this means that the facts about Mercury's perihelion can, according to the Popperian account, support neither the modified classical theory *nor* the general theory of relativity. Yet these facts are widely acknowledged to constitute one of the few important pieces of evidence in favour of the latter theory.

Other cases can be cited in which the Popperian account runs counter to scientists' intuitions. For example, since the Michelson-Morley result had been known since about 1887 it could support *neither* Lorentz's 1904 theory of corresponding states *nor* Einstein's 1905 relativity theory. Similarly, on the Popperian account the fact that plane-polarised light twice internally reflected at a certain angle in a glass rhomb will emerge circularly polarised cannot support Fresnel's wave theory, since Fresnel had discovered it experimentally prior to proposing the theory which explained it.[11] Yet this was widely regarded as one of the most impressive pieces of support for Fresnel's wave theory.

Let us take a more detailed look at the important methodological features of examples of this sort. The precession of Mercury's perihelion can be explained using Newtonian theory by providing certain free parameters and then assigning particular values to them. This assignment in fact reflects some (so far non-independently testable) assumptions about the distribution of mass within the Sun.

Newtonian theory provides us with no independent way of assigning values to these parameters. The Newtonian explanation of Mercury's perihelion is actually arrived at by looking at the facts of Mercury's orbit and using them to work out what values those parameters *must* have in order for Newtonian theory to yield these facts. Of course once these parameter values have been filled in in this way, Newtonian theory does indeed entail the facts about Mercury's orbit but this is more like a consistency proof than a genuine prediction.[12] Mercury's perihelion is not regarded as supporting classical theory; but the reason behind this assessment is not simply that the facts about it were already known (included in 'background knowledge') but that they were known *and used in the construction of the theory.* On the other hand the general theory of relativity was arrived at through considerations which were quite independent of any facts about Mercury's orbit.[13] It nevertheless explains these facts and there seems no good reason not to regard them as supporting this theory.

The classical explanation (using the Lorentz-Fitzgerald contraction hypothesis) of the Michelson-Morley result will serve as a further example. The basis of the widespread opinion that the null result of this experiment, although entailed by the Lorentz-Fitzgerald hypothesis, does not support that hypothesis is the widespread belief that the hypothesis was *arrived at* in the following way: In response to the null result classical mechanics was provided with a new parameter reflecting the amount of contraction a rod undergoes in moving through the ether. The value of this parameter was fixed by working out precisely what value it needed to have for classical mechanics to explain the known experimental result.[14] Again the methodologically important feature of this account is not just that the result was known (the result was known before 1905 yet is generally taken to support relativity theory) but that it was known *and used in the construction of this theory* – a crucial component of this theory was 'read off' from the result.

Should we not rule, then, that of the empirically accepted logical consequences of a theory those, and only those, used in the construction of the theory fail to count in its support? It is precisely this suggestion that the methodology of scientific research programmes incorporates. This methodology embodies the simple rule that one can't use the same fact twice: once in the construction of a theory and then again in its support. But any fact which the theory explains but which it was not in this way pre-arranged to explain supports the theory *whether or not the fact was known prior to the theory's proposal.* Theoreticians should not automatically be penalised, as Popper's corroboration theory would penalise them, for the earlier successes of their experimentalist colleagues..

This new account of the empirical support relation captures scientists' intuitions in all of the historical cases we have discussed. For example, the facts predicted by Newton's theory and subsequently accommodated within Cartesian theory provide the latter with no support, not because the facts were known prior to the construction of the Cartesian theory which explains them, but because they were *used* in the construction of that theory. The facts about circularly polarised light produced by total reflection of plane polarised light *do* support Fresnel's wave theory of reflection *despite* being known before the proposal of that theory because the theory was arrived at by considerations (of theoretical and mathematical kinds) completely independent of those facts.[15]

In the light of this new characterisation of empirical support, it is not difficult to see what was wrong with Popper's characterisation. Popper's introduction of 'background knowledge' into considerations of empirical support indicates that he had correctly identified the major problem with the requirement of simple testability.[16] This is that the requirement is too easy to satisfy: it is easy (and riskless) to make a theory 'testable' (to provide it with 'potential falsifiers') if one already knows how the tests will turn out (so that no one knows in advance that none of the 'potential falsifiers' are 'actual falsifiers'). Similarly, if a theory T is presently accepted and some new evidence e crops up which is not predicted by T, then it is generally trivially easy to use T and e to generate a new theory T' which does entail e. Popper's theory of corroboration excludes such cheap success. But it constitutes an *over*-solution of this problem – the proposed solution is too coarse-grained. There is no justification for regarding a fact as incapable of supporting any theory proposed subsequent to the fact's discovery. There is every justification for regarding a fact used in the construction of a theory as not capable of supporting the theory.[17]

Of course, if a fact was unknown at the time of the proposal of some theory then it could not have been used in the construction of the theory. Thus not being part of the theory's 'background knowledge' is a *sufficient* condition for the fact to support a theory which explains it. But it is not a necessary condition.[18] This is why Popper's account of corroboration captures scientists' intuitions in *some* of the examples cited; but when it does so it is for the wrong reason – the question of whether some fact was or was not known when some theory was proposed is *in itself* irrelevant to the question of whether or not the fact supports the theory.

The methodology of scientific research programmes regards a theory as supported by any fact, a 'correct' description of which it implies, provided the fact was not used in the construction of the theory. This seems a quite modest proposal and it seems to be the obvious solution to the problem posed by the ease with which *ad hoc* explanations of *given* facts may be generated. The proposal does, however, have the effect of bringing questions of how a theory was arrived at, questions of 'heuristic', into the methodological assessment of the empirical merits of a theory. This has certain consequences which appear unacceptable at first sight.[19]

Foremost amongst these seemingly unacceptable consequences is the following: according to this conception of empirical support it is possible for a theory arrived at in one way to be supported by a fact while the *same* theory arrived at in a different way is not supported by the *same* fact. This certainly sounds implausible and is in fact taken, in a recent article by Musgrave,[20] as a *reductio ad absurdum* of this whole approach to empirical support. The air of paradox about this consequence however, stems only from the fact that we are used to speaking of a fact supporting a theory whereas this new proposal speaks of a fact supporting a theory arrived at in a certain way. This new conception (like indeed the old Popperian one) makes empirical support a *three*-place, rather than a two-place, relation. Here two of the places are filled, as before, by a theory and a factual statement; but the third place is filled, not by background knowledge, but by the set of those factual statements used in the construction of the theory. The relation holds if and only if the factual statement is implied by the theory but is not a member of the set of factual statements used in the construction of the theory. It is possible, therefore, for the relation to hold for a given factual statement and a given theory constructed in one way, but not for the *same* fact and the *same* theory constructed in another way. (Just as, in the Popperian account, a theory may be supported by a fact in one historical situation whereas the same fact fails to support the same theory in another historical situation – because 'background knowledge' has changed.)[21] And this, far from being unacceptable, is precisely the consequence we want, for we have seen that the main problem which this new approach to empirical support was meant to solve is that posed by cases in which each of two theories implies a correct description of some fact which we intuitively want to regard as supporting only one of them. We have seen that this arises when one theory is nicely adjusted to imply some of its rival's empirical content. But if it is possible for one theory to 'explain' *post hoc* some of its rival's content in this way, it may be possible for it to explain *all* of it. In that case the two theories would become, if not identical, at least empirically equivalent. Yet we should regard one of them as supported by the facts it predicted and the other (despite its empirical equivalence to the first) as not supported by the facts it was adjusted to fit. The only extra difficulty in the case where a theory becomes identical *via* such *ad hoc* adjustments to its rival is one of formulation. To avoid paradox we now have to make the three-place character of the empirical support relation explicit by speaking of evidence supporting a *theory as arrived at in a certain way* (or as supporting a theory together with a heuristic) rather than as simply supporting a theory.

Perhaps some doubts will remain about this proposal: does it not, for instance, make empirical support a 'person relative' affair?[22] This is a very reasonable fear which is encouraged by Zahar's occasional use of such formulations as 'a theory is not supported by those facts it was *devised* to explain'. Such phrases make it sound as if two scientists might introduce the same theory, which, however, is supported differently according to which scientist proposed it, since each of them introduced it to explain different facts. But it is not a person-relative, but a *heuristic*-relative affair; and the heuristic considerations which led to the construction of a theory can be objectively specified as we shall see from some examples *below*.[23]

I shall return to the question of heuristics later.[24] For the moment, having answered the question "When does a fact support a theory?", I turn to the question "When is one theory better supported by the facts than another?".

3. ARE THERE ANY SITUATIONS IN SCIENCE WHICH ARE NOT 'MESSY'?

In the previous chapter Watkins claims that Popper's theory of corroboration can provide principles for discriminating between theories in certain 'clear-cut' cases, while for other 'messy' cases (in which, for example, both theories are refuted) other principles of theory-comparison may have to be invoked. The trouble with this suggestion is that, when looked at in any detail, most cases in the history of science turn out to be 'messy'. Even the best scientific theories (like Newton's) were inconsistent with accepted experimental results when first proposed, and remained inconsistent with experimental results (though not necessarily the same ones) until they were replaced. Thus it turns out that nearly all theories during the whole history of science have had the lowest possible degree of Popperian corroboration (*minus* one). This means that scientists' choices between theories cannot be accounted for on the basis of Popperian degree of corroboration appraisals. For example, in the 1830s say, both the latest wave theory of light and the latest corpuscular theory of light were refuted. Thus both had Popperian degree of corroboration *minus* 1, yet the wave theory was more or less universally regarded as vastly superior to its rival.

The methodology of scientific research programmes by contrast can easily accommodate this particular intuitive appraisal and other similar ones. According to this methodology one theory is better than its rival if it is supported by more facts than its rival (in the sense of empirical support outlined in Section 2) – and this condition may hold even if both theories are refuted. This, as Lakatos pointed out, transfers the methodological spotlight away from refutations and focusses it on verifications of excess content (*i.e.* that part of a theory's empirical content which it does not share with its rival).

That refutations do not play such a big role in the development of science had of course been forcefully pointed out by Kuhn.[25] He showed that the typical response of a theoretician to an inconsistency between the theories he accepts and accepted experimental results is *not* to regard the theory as ruled out by the experimental result but rather to regard the result as an 'anomaly' which he hopes and expects will be 'dissolved' by further research.

There is a well known *epistemological* rationale for scientists not getting too excited about refutations and for holding on to a theory despite clashes between it and the experimental evidence. This rationale, which was already in large part provided by Duhem, is taken into full account by the methodology of research programmes. Admitting the fallibility and 'theory laden' character of 'observation statements' (as they are usually considered) the point may be expressed as follows.

Assume that some observational consequence has been drawn from a theory and that the best available observational techniques indicate that this consequence is false. In the derivation of the testable consequences at least some statements of 'initial conditions' will have been assumed as premisses in addition to the theory itself. There are then three distinct possible explanations of this clash between theory and experiment, only one of which is the falsity of the theory itself. The other two possibilities are that one of the statements of initial conditions is false, and that the ascription of falsity to the observational consequence is itself false. Both the ascription of truth to the statements of initial conditions and of falsity to the observational consequences are after all, decisions based on observational theories.[26] These ascriptions may be incorrect because the theories on which they are based are false.

But can't we at least narrow down the possibilities of resolving a clash between theory and experiment by requiring that the statement describing the experimental outcome be of such low level that we can hardly suppose that the truth-value we ascribed to it is wrong? For example, we might require, as Poincaré did, that our observation statements be about 'meter readings' – concurrences of needles with points on a scale. There are two points to be made about this suggestion. The first is that our inability to conceive of the possibility of our having made false ascriptions of truth values to such sentences may be due only to our lack of imagination.[27] The second, and more important, point is that if we adopt this suggestion we shall pay for the increased likelihood (in some informal sense) of our decision about the observation statement being practically incorrigible, by having to add extra premisses to the theory in order for statements of the required level to be derivable. We may, for example, be able to derive a statement about a body's *temperature* just from some theory of heat and suitable initial conditions; but in order to derive predictions about, say, heights of mercury columns in tubes we shall have to add all sorts of auxiliary assumptions about the thermal properties of glass and mercury, about coefficients of expansion and so on. Similarly we may be able to derive some prediction about the position of a planet just from some astronomical theory and initial conditions; but to derive predictions about, say, the position of a spot on a photographic plate exposed in a camera in a suitably inclined telescope we shall have to invoke extra premisses about the chemical properties of photographic emulsion, about optics, about the working of the telescope, about atmospheric refraction and so on. In other words in meeting the requirement that our observation statements be very low level we shall simply articulate and add as extra premisses in our deductive test-structure those observational theories on which we had hitherto implicitly relied. But then of course even if we deduced an experimental consequence whose falsity we regarded as beyond dispute, it is perfectly possible to blame, not the theory under test, but one of the necessary auxiliary assumptions.

But then clashes between theory and experiment constitute not experimental disproofs of theories, nor even straightforward inconsistencies between the theory under test and experimental results. Rather such clashes constitute inconsistencies between experimental results and a whole group of theories. It clearly may therefore be reasonable to regard such a clash as not particularly endangering the theory under test. For one may expect the 'anomaly' to be 'dissolved'. This simply means that one expects that some change will be made to the auxiliary assumptions so that consistency with the experimental result is restored.

Indeed there are plenty of well-known examples from the history of science in which such expectations were satisfied. Some of the predictions of Newton's theory were refuted by Flamsteed's data, but this clash was resolved not by giving up the theory but by Newton's 'revising' Flamsteed's data by providing him with a new theory of atmospheric refraction. The facts about the orbit of the planet Uranus refuted the Newtonian prediction, but this resulted not in the rejection of Newton's theory but in the rejection of an auxiliary assumption about the total force acting on Uranus.

The methodology of scientific research programmes can deal very simply with these examples and with the Duhemian methodological point which they underline. This methodology allows that clashes between theory and experiment occur all the time and that they will normally be resolved by assuming the theory true and using the clash as an indication that some auxiliary assumption or observational theory needs to be replaced.[28] In this way the 'protective belt' or auxiliary theories surrounding the 'hard core' theory will be articulated and modified.[29]

There is, however, a problem here, a problem to whose discovery at least the more naive forms of falsificationism would never have led (although it is a problem towards whose solution the sophisticated form of falsificationism outlined by Watkins *above* goes a long way). There are many cases in which the practice of defending theories from refutation by the modification and elaboration of protective hypotheses has been intuitively regarded as unsatisfactory – even as the tell-tale characteristic of pseudo-science. An example which Imre Lakatos used to give in his lectures was that of Marxism. He claimed that the scientific unacceptability of Marxism stems not from its failure to make any testable predictions at all (it made many such predictions, for example, that the working classes would be impoverished absolutely and that once a society has been through its revolution and become socialist it would be free from further revolutions); nor from the fact that these predictions were unsuccessful (even the best scientific theories made unsuccessful predictions). Rather, Lakatos alleged, Marxism's pseudoscientific character is betrayed by its proponents' reaction to the lack of success of its predictions. Marxists explain away this lack of success in various ways rather than regard it as a refutation of Marxism itself.[30] But then, as we have seen, this was also the reaction of Newtonians in analogous circumstances. When is it scientifically satisfactory to blame inconsistencies between fact and theory on auxiliary assumptions and when is it unsatisfactory? What, if anything, distinguishes the 1846 Newtonian who claims that the unexplained perturbations in Uranus's orbit are not genuine refutations of Newton's theory but rather of some auxiliary assumption, from the 1956 Marxist who claims that the events in Hungary in that year are not genuine refutations of Marxist theory but rather of some auxiliary assumptions?

Many have thought that one can only leave it to 'scientific commonsense' to distinguish between acceptable and unacceptable defences of a theory from refutation. But the methodology of scientific research programmes claims to provide a characterisation of this distinction which is generally applicable, objective and explicit. It says that what distinguishes the 1846 Newtonian from the 1956 Marxist is that the latter resolves the clash between his theory and the facts by shifting to a theory which is supported by no more facts than is his previous theory. He may, for example, simply dismiss the Hungarian revolt as not a genuine counter-revolution without giving a more explicit general account of what *would* constitute a genuine revolution – an account which would make his new set of assumptions more, rather than less, testable. Or he may explain away the fact that in the West the working classes have been neither absolutely nor relatively impoverished by invoking a theory of imperialism, *whose only extra empirical support is the non-impoverishment of the Western working classes.* On the other hand, the 1846 Newtonian does not content himself with the claim that the irregularities in Uranus's orbit do not refute Newtonian theory, but rather some (unspecified) auxiliary or observational assumption. Nor even with simply specifying the faulty auxiliary assumption and replacing it with a new assumption. Instead he replaces the faulty assumption with a new assumption of a special kind – one which makes the new total theory capable of receiving genuine support from more facts than the previous total theory. Here, of course, one extra empirical prediction concerned the existence of a hitherto unknown planet. This prediction was subsequently confirmed. Thus the Newtonian's shift was from one set of assumptions to another set which received support from more facts; whereas the Marxist's shift was to a set of assumptions which was incapable of receiving support from more facts than its predecessor. In brief, the difference between Marxism and Newtonianism, is the difference between a *degenerating and a progressive programme.*

One theory is better than another, according to the methodology of research programmes, if it is given genuine support by more facts, whether or not both theories are refuted. If a research programme produces a series of theories such that each theory is better, in this sense, than its predecessor, then the programme is (empirically) progressive. If one research programme produces a theory which is better in this sense than the latest theory produced by its rival then that programme has (for the moment at least) *superseded* it rival.

I should like to highlight two features of this solution of the problem of when one scientific theory is better than another. The first is that it has implicit in it a solution of the so-called Duhem-Quine problem.[31] The problem is: which of the group of theoretical assumptions needed to deduce observational results should one replace (or should regard as falsified) in the event of one of the observational consequences being accepted as false? The solution is: Replace any of the group of the assumptions that you like – the best modification is the one which produces the theoretical system which constitutes most progress over its predecessor. (Which assumption should be regarded as falsified in the event of a clash between theory and observation can only be decided with hindsight: that assumption is falsified which is eventually most successfully replaced.)

The second feature of this definition of progress that I should like to mention is this. It might seem that we can do without all this talk of 'genuine' support from facts and simply characterise one theory as better than a rival if it has excess empirical content over the rival.[32] However, Marxism supplemented by its theory of imperialist exploitation has excess empirical content over its predecessor theory. It predicts that the working classes will not be absolutely impoverished. In other words, it has as excess content precisely the fact which refuted its predecessor and which it was introduced to explain. On the straight-forward excess empirical content criterion, therefore, Marxism would have to be pronounced progressive. And so would almost any series of theories produced by the most blatant *ad hoc* maneouvres. (The only exceptions would be cases of *ad hoc* reduction in content – truly exceptional cases). On the criterion of empirical support developed in my Section 2, on the other hand, such excess 'verifications' do *not* provide genuine empirical support.[33]

We have seen that the methodology of scientific research programmes attempts to correct the Popperian theory of corroboration both over its characterisation of the empirical support relation and over the conditions under which one theory is scientifically superior to another. Involved in both these corrections, however, has been an element whose introduction marks two ways in which the methodology of research programmes *goes beyond* Popper's methodology. These are its recognition of the programmatic aspect of major scientific developments and its importation of this programmatic aspect into methodological appraisals.

4. THE METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMMES: THE IMPORTANCE OF HEURISTICS

Most clashes between theory and observation are not resolved by giving up the theory under test but rather by modifying one of the auxiliary assumptions needed to deduce the observation result from the theory. This possibility was pointed to by Duhem and the prevalence of its employment in science was pointed out by Kuhn. The methodology of research programmes provides criteria for *evaluating* various possible shifts from one set of assumptions to another. But it also points out that these shifts are not made in a simple trial and error fashion, but are guided by general considerations of an objective and analysable nature.

Indeed, perhaps the most basic difference between the methodology of scientific research programmes and other methodologies (in particular Popper's) is its recognition of the programmatic character of major scientific achievements. It is an historical fact that some important theoretical innovations in science were quickly succeeded by the articulation of a series of scientific theories, related in certain ways to, but *not* implied by, the first theory. So, for example, Einstein's 1905 special theory of relativity was quickly followed by the invention of related theories by Planck (the relativistic law of motion) and by others (for example, the famous $E = mc^2$ law relating mass and energy). These theories bear strong enough resemblances to Einstein's 1905 original to be called 'relativistic theories', but they are not simply deductive consequences of Einstein's original theory. Similarly, Fresnel's invention of the wave theory of the diffraction of light was quickly followed by several other theoretical breakthroughs in optics (mainly by Fresnel himself). These succeeding theories were clearly related to the wave theory of diffraction (as evinced by the fact that they too were called 'wave theories' of various groups of phenomena), yet they were by no means implied by Fresnel's original theory. (For example, Fresnel's wave theory of double refraction is certainly logically independent of his theory of diffraction.)

Facts like these must seem remarkable coincidences to anyone who holds a straightforward trial-and-error, conjectures-and-refutations view of the development of science, for this view can assign no reason why theoretical breakthroughs should come in bursts. The methodology of research programmes, on the other hand, explains these facts, for, according to it, major scientific achievements consist not merely of a set of statements about the world, but *also* of a set of ideas about how to 'fill in', make more precise, draw consequences from,[34] these statements, and also about how to elaborate on them, introduce new assumptions so that they apply to new fields, and how to modify them when difficulties arise. Lakatos called this set of ideas the *positive heuristic* of a programme. But then if major scientific achievements come in the form of programmes, it is easy to see why theoretical innovations often come in bursts – for once a programme exists different scientists may pursue its heuristic and produce new theories. Thus Einstein invented more than a theory in 1905, he invented a programme. By pursuing the heuristic of this programme further theories were produced. These theories were related to, but not logically implied by, Einstein's original *theory*. Similarly what Fresnel invented in the early nineteenth century was not merely a theory but rather a programme. Through pursuing the programme a variety of new theories were produced.

A research programme then consists of a fundamental set of statements about the world and of a positive heuristic. The positive heuristic guides the production of specific theories within the programme.[35] Each specific theory will be 'built around' and will imply the fundamental set of statements about the world (the programme's 'hard core'). The programme (which thus issues in a series of theories) is appraised as 'progressive' if the theories in this series are supported by more facts than their predecessors (in the sense of empirical support elaborated in Section 2). Otherwise the programme is 'degenerating'. This means that the theories produced by the programme either make no genuine excess testable predictions (that is they explain only those facts they were introduced to explain) or such extra predictions as they do make are empirically refuted.

I argued in Section 2 that the heuristic path by which a theory was discovered is relevant to the question of empirical support. Given this, there is a clear, though intuitive, sense in which a programme with a powerful heuristic is 'more likely' to make progress (produce theories which derive support from more facts) than is a programme with a weaker heuristic. (Though of course whether or not it actually does make such progress depends on the objective features of the world.) A powerful heuristic will often point to shortcomings in existing theories in the programme and lay down guidelines for their replacement, *quite independently of any empirical difficulties.* Since new theories produced by such programmes are constructed without the help of empirical considerations, any successful predictions they make will provide them with empirical support in our sense.

The two research programmes I have mainly used as examples both had a clearly definable positive heuristic. Elie Zahar had shown in detail how the positive heuristic of the relativity programme was supplied by the requirements that physical laws be covariant and that the new relativistic laws have the corresponding classical laws as 'limiting cases'.[36] (These requirements were in turn underpinned by certain metaphysical assumptions about the world.) The heuristic of the 19th Century wave optics programme was in large part supplied by the assumptions that the all pervasive ether which carries the light waves is an elastic medium with straightforwardly mechanical properties.[37]

Both these programmes had powerful heuristics. Zahar has shown[38] how the heuristic of the relativity programme did indeed guide the construction of relativistic laws, like Planck's relativistic law of motion and the law that $E = mc^2$. An example of how a powerful heuristic can point to shortcomings in existing theories independently of empirical difficulties is provided by the wave optics programme whose heuristic pronounced inadequate Fresnel's theory of the intensity of reflected beams of polarised light *in spite of* this theory's enormous empirical success.[39]

In some other programmes the heuristic is much weaker, often consisting merely of a series of suggestions for dealing with refutations of existing theories. (The programme may originally have involved much stronger heuristic principles which however did not produce theories which were empirically successful and which were therefore dropped.[40]) One programme with a weak heuristic seems to have been the Ptolemaic programme, whose heuristic amounted to the injunction to 'save the phenomena' (and at the same time the 'hard core' geocentric hypothesis) by a combination of as few uniform and circular motions as possible.[41] This heuristic condemns one to wait for anomalous 'phenomena' to present themselves before proceeding to 'save' them. There seem to be many examples of programmes with weak heuristics in the social sciences. Peter Urbach has argued that one such example is provided by the environmentalist programme in human intelligence – at any rate as it has so far developed. The heuristic of this programme (although originally quite strong) seems to have degenerated to the point where it consists merely of a set of suggestions on how to modify existing theories so that they deal with the anomalies faced by these theories (most of which have been thrown up by the rival hereditarian programme).[42]

5. SCIENTIFIC REVOLUTIONS AND THE PROBLEM OF 'KUHN LOSS'

Having characterised the general features of the methodology of scientific research programmes, I should like briefly to present its account of scientific revolutions, and then to show that it can give some sort of answer to a further problem which besets the Popperian corroboration theory outlined in the preceding chapter.

When do scientific revolutions occur according to the methodology which I have been presenting? They occur when a new programme is introduced which challenges some already accepted programme and which becomes and remains more progressive than its accepted rival. The older programme may actually be degenerating, though this need not be true. Corpuscular optics was degenerating in the early 19th Century when Fresnel revived the wave programme; whereas, if Elie Zahar is right, the classical programme was not degenerating in 1905 when Einstein introduced the relativity programme.[43]

Of course the methodology does not predict that, whenever some new programme comes along which it appraises as more progressive than the old one, all scientists will immediately switch to work on the progressive programme. Nor does the methodology pronounce 'irrational' those scientists who, in such circumstances, stick to the old programme.[44] Such a scientist may, in perfect conformity with this methodology, agree that the new programme is, at that moment, superior, but nevertheless declare his intention to work on the old programme in an attempt to improve it so that it becomes even better than the new programme.[45] Nevertheless we should expect that most scientists will join the most progressive and most promising programme, especially if the early attempts to revitalise the old programme fail.[46]

This account of scientific revolutions allows the methodology of scientific research programmes to go some way towards answering a criticism which has been brought against falsificationism and to which that methodology (even in its most sophisticated form outlined in the preceding chapter) can provide no answer. This criticism is based on the phenomenon of 'Kuhn loss' of explanatory content.[47]

One of the central arguments produced by the defenders of the 'incommensurability thesis' (in particular Kuhn and Feyerabend) is that very often the switch which occurs in a scientific revolution from one theory (or rather research programme) to the next has involved losses as well as gains in experimentally accepted explanatory content. In research programme terms: the latest theory produced by one programme may be supported by facts which do not support the latest theory produced by its rival and *vice versa.*

The losses involved may be only temporary. Thus to use one of Feyerabend's favourite examples,[48] while the geocentric theory (together with Aristotelian physics) could explain why objects do not fly off the earth, Copernicus's heliocentric hypothesis could not explain this – at least not until, long after the 'Copernican revolution', the heliocentric hypothesis was conjoined with a new dynamics. But if such losses occur at all then they clearly *may* be permanent, that is they may never be made good by the programme whose adoption incurred them. For this programme may itself be replaced by a further one before the explanatory losses are made good.

There seems no doubt that these losses do occur.[49] Indeed that they do not occur more often may sometimes be merely accidental. (I am thinking of cases like the following: it is now accepted as a fact that light exerts a pressure on any screen on which it falls. This is certainly a direct consequence of the corpuscular theory of light (assuming, of course, that light-corpuscles possess inertia). On the other hand, any formalisation of the wave theory of light prior to the 1830s would have predicted no such pressure. The reason why the switch to the wave theory in the 1820s was not recognised as involving the loss of explanatory content in this respect was that experimental techniques were not sufficiently refined to detect this pressure of light. Several attempts had been made to detect it, but though at one time success was claimed, the general consensus by the 1820s was that such pressure had not been detected. Indeed this was taken by some commentators as a crucial experiment in favour of the wave theory.)[50]

Can the shift from one theory (or programme) to another be explained as a shift from one theory to a *better* one despite a loss of explanatory content being involved? Not according to the Popperian theory of corroboration outlined by Watkins *above*. No one, so far as I know, has claimed that there are cases of theory shifts in which *only* losses, but no gains, in explanatory content are achieved. This means that these are not cases of shifts from one theory to what Popper would judge to be a *worse* theory. But if a loss of explanatory content occurs then the two theories involved must be incomparable on this Popperian account for each (correctly) 'answers empirical questions' not answered (or answered incorrectly) by the other.[51]

The methodology of research programmes is, on the contrary, much more flexible in this respect and provides several means for discriminating between theories even in these cases. It is true that if the latest theories produced by two research programmes are such that each of them explains facts not explained by the other, then neither programme has 'superseded' the other. But this does not mean that the methodology must remain silent about these cases. Indeed there are at least two things it can say. It may be that one of the theories has been produced by a progressive research programme whilst the other has been produced by a degenerating research programme. And it may be that one of the programmes is heuristically strong, whilst the other is heuristically weak.

When (as will often happen) these two (intra-programme) appraisals go hand in hand by both favouring the same programme, the methodology of research programmes provides a clear rationale for the preference of one programme over the next even if a loss of explanatory content is involved. After all explanatory gains, as well as losses, are involved in these cases, and so failing to prefer the new theory would also incur 'Kuhn loss'. The question then is whether there are any general grounds for regarding one programme as more likely than the other to make good the explanatory losses incurred in adopting it. The methodology of research programmes supplies such grounds, namely the progressive nature and greater heuristic strength of one of the programmes.

A case in which these two intra-programme appraisals do seem to go hand in hand is the early 19th Century revolution in optics. This revolution which occurred round about the late 1820s to early 1830s did involve losses in explanatory power. For example, the existence of dispersion was a straightforward (non '*ad hoc*') consequence of the corpuscular theory. (Admittedly the assumptions involved ran into difficulties when applied to other phenomena, but this is not the issue here – considered simply as an explanation of dispersion they were impeccable.) On the other hand it was not at all clear in the 1830s how dispersion might be explained wave-theoretically. Nevertheless the wave programme was in the 1830s vastly superior to its corpuscular rival and (almost) universally regarded as such. Moreover this superiority can be accounted for by the methodology of research programmes. First, the wave programme had been made enormously progressive by Fresnel. The latest wave theories of light could explain in a non *ad hoc* way many more facts than their predecessors (for example about diffraction and interference of polarised light); whilst the corpuscular programme could at best capture these facts *ad hoc*. That is, the corpuscular programme was degenerating. Secondly, Fresnel had shown that the heuristic of the wave programme could give almost as precise guidelines for research and the development of specific theories as the heuristic of the corpuscular programme. However the heuristic of the wave programme, unlike that of its rival, *was almost completely unexplored.* Thus most scientists began to plump for the wave programme, which eventually began to make good its 'Kuhn loss'. (Cauchy produced wave theoretical explanations of some aspects of dispersion but this phenomenon was not given a full explanation within the wave programme until Gouy explained it, using Fourier analysis, in 1886.[52])

Most cases of switches between theories neither of which explains all that its rival does may thus lose their mystery if we appraise, not the rival theories, but the rival programmes of which they are the latest products. However since this appraisal has two parts, there may be cases in which these two appraisals diverge. (One programme may happen to have been progressive but to have had a weak heuristic, whilst the other although empirically degenerating had a strong heuristic.) Thus this analysis opens up the possibility that the methodology of research programmes may give a general delineation of those cases, if any, in which there is genuine scientific uncertainty between two approaches – the cases of genuine 'incommensurability'. (Clearly one cannot do this using the ideas about incommensurability promulgated by Kuhn and Feyerabend; for there are cases of theories which are pronounced incommensurable by Kuhn and Feyerabend and yet one of which is *clearly* better than the other.) This intriguing possibility awaits historical investigation.

My analysis has at various points involved the notation of one heuristic being stronger than another. Whether a general characterisation can be given of the strength of a heuristic, and hence of a research programme's objective 'promise' is a question which is taken up by Peter Urbach *below*.

Various of my arguments have been designed to show that both in the ways it corrects Popper's methodology and in the ways it goes beyond it, the methodology of research programmes supplies philosophical evaluations which are closer to the intuitive evaluations made in particular cases by scientists working in the more advanced sciences. This point is illustrated in the next chapter by Elie Zahar using a particular episode from the history of science.

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NOTES

[1] I should add that the answers proposed by the methodology of research programmes have also developed over time. The answers I shall give are not those given in Lakatos [1970]. The idea that heuristic considerations have to be imported into theory-appraisal was developed in discussions between Lakatos, myself and Elie Zahar, who was in this respect the prime mover.

[2] Of course the methodology of research programmes is itself 'Popperian' in a wider sense. Many of the corrections and improvements of the explicit methodology which I have, for the sake of definiteness, labelled as Popper's are themselves Popperian in spirit (and Popper scholars could no doubt find sources for many of them in Popper's own work). I regard the methodology of scientific research programmes as the result of a 'creative shift' within Popper's own philosophical research programme.

[3] The two assumptions that all accepted empirical consequences of a theory confirm it and that any two logically independent empirical consequences confirm it equally form the basis of the 'purely logical' position in the age-old 'weight of evidence' debate. See the recent article by Musgrave ([1974]); cp. Lakatos [1968a], p. 387.

[4] Popper actually introduces logical probability considerations and defines the severity of a test as the probability of its outcome given the theory and background knowledge, *minus* the probability of its outcome given background knowledge alone. But these refinements need not concern us.

[5] Popper himself again introduces probability considerations here. Roughly his idea is that if the theory predicts that the outcome of some test will be *e*, then the more improbable background knowledge makes the occurrence of *e*, the more severe is the test of theory.

[6] See, *e.g.* Popper [1963], p. 390: "'background knowledge' ... is ... all those things which we accept (tentatively) as unproblematic while we are testing the theory".

There is a slight difficulty here. Popper requires the background knowledge to a theory to be consistent with it. But as he himself points out it is one mark of a very good theory if it corrects (*i.e.* is inconsistent with) previously accepted factual statements. This means that one cannot know in advance of the proposal of a theory what its background knowledge will be! Popper requires that the previously accepted factual statements contradicted by the theory drop out of background knowledge. Thus a theory is no more severely tested by a test whose result it predicts to be different from the result predicted by background knowledge, than it is in a case where background knowledge remains silent about the result. Indeed, on this account, a theory receives *less* credit for successfully contradicting accepted knowledge than it receives for successfully going against a result which accepted knowledge makes 'highly probable'. This is surely contrary to the spirit of the Popperian programme.

On an historical note, this counter-intuitive consequence of Popper's corroboration theory seems to me to have arisen because of the attempt to make background knowledge serve two distinct purposes. It was originally meant to consist of those extra assumptions, both singular and universal, required in the deduction of testable consequences from a scientific theory (see *below*, pp. 52–4). This is indicated in the quotation from Popper *above*. However it was then pressed into service to eliminate trivial confirmations (or corroborations) of theories – a theory should not get credit for simply predicting something that was already part of background knowledge. Indeed, speaking informally, in Popper's definition of the severity of a test whose outcome is e for an hypothesis h given background knowledge b, which definition makes the severity depend on $p(e, h \cdot b)$ *minus* $p(e, b)$, b is playing one role in the first probability function (it is there the set of those extra assumptions we have to make in order to derive e from h), and the second role in the second probability function (there it is the set of already accepted knowledge). In what follows we are essentially investigating how successfully background knowledge performs its second role – that of ruling out trivial confirmations of theories.

[7] Lakatos's [1970] account is essentially the same as this.

[8] Although there are certain general intuitions which favour the atemporal, logical approach – why should we attribute more weight to one of a theory's consequences than to others (beyond the weighting by logical strength)? Perhaps the best way to look at the situation is that the logical, atemporal, confirmation theorists were trying to solve one problem. (Roughly: "To what extent is the truth of a theory guaranteed by the evidence we have available?") While the Popperian approach (and that of the methodology of research programmes) is directed to another problem. (Roughly: "What sort of predictions should a theory make in order for it to have contributed to the growth of knowledge?") The realisation that the two approaches are directed to different problems would perhaps have clarified some aspects of Musgrave's [1974] paper.

[9] See for example his [1971], p. 104.

[10] See *e.g.* Adler, Bazin and Schiffer [1965].

[11] See Whittaker [1910], pp. 135–6.

[12] In fact if we let $N(\lambda)$ be Newton's theory with the parameter λ unspecified, and e

the statement about Mercury's perihelion what such a procedure establishes is the truth of the sentence

$$\exists\lambda(N(\lambda)\rightarrow e).$$

I should add that it is, of course, better for a theory for it to be consistent with the facts rather than inconsistent with them. (Indeed some consistency proofs have been important factors in the acceptance of theories. This has been when it had been thought that *no possible form* of some theory could be consistent with some fact. This was true for example of the wave theory of light and rectilinear propagation. Here the consistency proof was provided by Fresnel.) But it is still better for a theory to *predict* a fact. The new characterisation of empirical support which I introduce *below* really amounts to a warning not to confuse consistency proofs with predictions.

[13] See especially Zahar [1973], §3.1.

[14] Elie Zahar in his [1973] argues that this widespread belief is ill-founded, for there were completely independent reasons within Lorentz's programme for giving the specific value to this parameter. Thus the Michelson-Morley result *did* in fact support Lorentz's programme. But of course Zahar would agree that *had* Lorentz's explanation been arrived at in the way described in the text it would not have been supported by the Michelson result.

For the sake of logical clarity I should add that although I speak here of classical theory being provided with a new parameter, in a sense the parameter was already implicit. That is, it was already assumed that there was *no* contraction of rigid rods. If some such assumption had not already been made, this new assumption (unless it introduced inconsistency) could not affect the theory's predictions.

[15] I should make it clear that the methodology of research programmes does not condemn the practice of, for example, reading off the values of parameters from some experimental results. This happens in all the best research programmes. For example, the wave theory arrives at the values of the wavelength λ_i of various kinds of monochromatic light by predicting various interference fringe spacings as functions of λ_i and then reading off the value of λ_i from the observed fringe spacings. The methodology merely states that having *used* these facts to construct their theory, wave theoreticians must look to *other* facts to support the theory. (See my [1975], for the details of the Fresnel case.)

[16] In fact Popper's discussion of conventionalist strategems indicates that he had already spotted the problem in 1934. He meets the problem (more or less) head on in his [1957] paper on 'The Aim of Science'. The problem had often been discovered before. For example, Duhem recognised that it is not difficult to construct 'purely artificial' theoretical systems, but 'we see in the hypotheses on which [such a system] rests, statements skillfully worked out so that they represent the experimental laws already known' (Duhem [1906], p. 28); it is only by avoiding such artificial systems that we can hope to progress toward the 'natural classification'.

[17] This justification can for example be based on Popper's requirement that a theory be given credit only when it has 'stuck out its neck'.

[18] This point is made as a criticism of Lakatos's [1970] criterion of scientific progress by Zahar on p. 102 of his [1973].

[19] Indeed it may have been the seeming unacceptability of these consequences which

prevented those who had spotted the real problem of *ad hoc* explanations from adopting this rather obvious solution of the problem.

[20] Musgrave [1974].

[21] Whereas the Popperian account makes the empirical support relation a three place relation ES(h, e, b), between a hypothesis, some evidence and background knowledge, our new account makes it a three place relation, ES(h, e, b') where b' is only the background knowledge *used in the construction of a theory.*

[22] This is really the basis of Musgrave's claim (see *above*, p. 50) that this approach to empirical support reduces to absurdity.

[23] See pp. 60–1: Whether some fact was used in the construction of a theory is an objective matter – quite separate from any question about whether the theory's inventor knew or 'was aware of' the fact. In the above case of the two scientists who introduce the same theory, if one has to use some fact in order to construct his theory, whilst the second does not, then the second scientist has shown that there are theoretical considerations which *are* supported by this fact (although the first scientist was not aware of it).

Thus in *deciding* whether some fact according to this new account supports a theory one will ask such questions as "Did x's programme give him independent reasons for fixing this parameter in this theory at this value or did its value have to be 'read off' from some observations?" And *not* such questions as "Did x know of this fact or have this fact in mind when he developed this theory?".

[24] *Below*, p. 58ff.

[25] Kuhn [1962]. Similar points were made by Agassi in his attack on what he calls Boyle's rule (see Agassi [1966]) and by Feyerabend (see for example his [1963] and his [1975]).

[26] The fact that a decision is involved here is particularly well emphasised by Popper (see especially his discussion of Fries's trilemma in his [1934] pp. 93–111).

[27] What for example, if the meter-reader was drunk or has bad reflexes?

[28] In the best research programmes the heuristic may give us some indication *which* auxiliary assumption needs to be replaced.

[29] It was a mistake on Lakatos's part to think that a 'protective belt' could get *constructed* in this way. Simply adding extra assumptions to a theoretical system cannot block the derivation of a false observational consequence.

[30] For an example of a 'degenerating research programme' of whose historical accuracy I am more confident, see Chapter 3 of my [1975]. (The example is Biot's development of the corpuscular optics research programme.)

[31] This was already pointed out by Lakatos (see his [1970], pp. 184–8).

[32] This would reduce it to the sophisticated falsificationist account which is essentially that given by Watkins *above.*

[33] This is one important way in which the criterion of progress I have been advocating differs from the one due to Popper; although of course it owes a good deal to the Popper who rejects 'conventionalist strategems' and the like. Further differences are these: (i) (to repeat what I said *above*, p. 52) Popper's corroboration appraisals cannot distinguish between any shifts between refuted theories (the group of Newtonian assumptions amended to include the new planet was still inconsistent with some observational results, *e.g.* about the Moon); (ii) Popper never applied these ideas to the

Duhem-Quine problem, indeed he twice denied that such a problem exists by denying (without argument) that Duhem had shown the inconclusiveness of falsification (see Popper [1934], p. 78, footnote *, and [1963], p. 112); and (iii) that Popper was occasionally confused on these matters is well illustrated by the fact that there are two entries in the subject index of his [1963], (p. 413): 'Marxism-refuted' and 'Marxism-made irrefutable'; these two claims are rather difficult to reconcile unless one has the idea of various *versions* of a Marxist *research programme*, which versions may differ in refutability – but even then the point is not that Marxism has been made completely irrefutable but that there has been no increase in genuine empirical content (and thus in refutability) in the various theory shifts that have been made in response to refutations of previous theories.

[34] Some philosophers have tended to regard the process of drawing consequences from a theory as automatic and unproblematic, but the mathematical machinery a programme provides for drawing out consequences from its theories is an extremely important part of it.

[35] Perhaps a list of *some* of the things a positive heuristic may include will be helpful. The positive heuristic may include mathematics – for example, how theoretical assumptions should be formulated so that consequences may be drawn from them will be guided by the available mathematics; the heuristic may include hints on how to deal with refutations if they arise (*e.g.* 'Add a new epicycle!'); and it may include directions to exploit analogies with previously worked out theories (*e.g.* much of the heuristic power of the corpuscular optics programme was supplied by the assumption that light consists of particles which obey the ordinary laws of particle mechanics, which laws were already highly developed in the late 18th Century).

[36] See Zahar [1973].

[37] See especially my [1975], though some details are to be found in my [1976].

[38] Zahar [1973].

[39] See Whittaker [1910], pp. 132–6, and my [1975]. For another example (Bohr's early quantum programme), see Lakatos [1970], pp. 140–154. I should add that having a powerful heuristic indicates only that the programme is likely to be progressive in the *theoretical* sense – that it will produce theories with extra potential empirical support – over their predecessors. Whether or not some of this extra content is empirically confirmed – so that the programme is also *empirically progressive* – is in the lap of the experimenters.

[40] This seems to be true of the heuristic guidance offered to various classical programmes by the assumption of the existence of the ether. This guidance was very strong at the time of Fresnel but difficulties presented themselves and it had become very weak by the time of Lorentz (see Zahar [1973]; also Schaffner [1972]).

[41] See Lakatos and Zahar [1975].

[42] For this particular example see Urbach [1974]. When I speak of the strength of a heuristic I am referring to its wide applicability, relatively unexhausted state, and ability to operator independently of facts. There is another sense which one might want to speak of a heuristic's strength, namely how near it approaches to being an algorithm. The heuristic of the Ptolemaic programme was strong in this second sense, but weak in mine.

[43] Zahar argues in his [1973] that the classical programme progressed in Lorentz's

hands at least in the empirical sense: it derived *new* support from the result of the Michelson-Morley experiment. (Zahar argues however that Lorentz's programme was not progressive in all senses for heuristically the classical programme had degenerated.)

[44] After all, if it were 'irrational' to work on a degenerating programme we should have to pronounce irrational all those geniuses who took up some old idea which hitherto no one had successfully developed and who turned it into a progressive research programme. (See Section 5 of my [1976].)

[45] A scientist *would* be pronounced 'irrational' (or rather mistaken) by the methodology if he stuck to the old programme denying that the new programme had any merits not shared by the old one, and thus denying that his own programme needed improvement in order to catch up with the new one. It is in such circumstances that we shall begin to suspect the operation of extra-rational motives.

[46] Paul Feyerabend has claimed that unless some time period is specified such that, if a programme consistently degenerates for that period, it is irrational to work on it any further, then the standards supplied by the methodology of research programmes are mere 'verbal ornaments' which hide the fact that a position of 'Anything goes' has been adopted. But anything does *not* go according to the methodology of research programmes – as I pointed out above it is wrong for a scientist to deny that his programme is doing badly if in fact (*i.e.* according to the methodology of research programmes) it is doing badly. If, however, adopting the position of 'Anything goes' simply amounts to denying the validity of the inference from 'Theory or programme *A* is better than *B*' to 'It is rational to work on *A* but not on *B*' then I think we can safely accept Professor Feyerabend's audacious *sounding* claim.

[47] This phenomenon was not discovered for the first time by Kuhn. Adolf Grünbaum pointed out at a recent conference that the phenomenon was noted by Phillip Frank. Paul Feyerabend tells me that he found the phenomenon noted in the work of poet John Donne.

[48] See *e.g.* Feyerabend [1964].

[49] They occur, however, rather less often than Feyerabend would have us believe. In his [1975] he, for example, counts the loss of content about the specific gravity of phlogiston in the Chemical Revolution as an example of incommensurability. But, *of course*, losses in *theoretical* content occur in revolutions, the interesting question is whether losses in *empirical* content occur.

[50] The details of this story are fascinating. Light pressure was accepted as experimentally detected only *after* Stokes had shown that it could also be predicted on the basis of the version of the wave theory then current.

[51] See Watkins *above*.

[52] Even this explanation was far from uncontroversial. For the controversy see Wood [1905], Chapter vi (this was dropped from subsequent editions of Wood's book).

The notes refer to the unified bibliography for Parts I and III on pp. 379–383.