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II—CRITICISM AND THE METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMMES

By IMRE LAKATOS

§*1. Introduction: Kuhn versus Popper.*

For centuries knowledge meant proven knowledge—proven either by the power of the intellect or by the evidence of the senses. Wisdom and intellectual integrity demanded that one must desist from unproven utterances and minimise, even in thought, the gap between speculation and established knowledge. The proving power of the intellect or the senses was questioned by the sceptics more than two thousand years ago; but they were browbeaten into confusion by the glory of Newtonian physics.

Einstein's results again turned the tables and now very few philosophers or scientists still think that scientific knowledge is, or can be, proven knowledge. But few realise that with this the whole classical structure of intellectual values falls in ruins and has to be replaced: it is not enough simply to water down the ideal of proven truth to the ideals of 'probable truth' or 'truth by consensus'.

Popper's distinction lies primarily in his having grasped the full implications of the collapse of the best-corroborated scientific theory of all times: Newtonian mechanics and the Newtonian theory of gravitation. In his view virtue lies not in caution in avoiding errors but in ruthlessness in eliminating them. Boldness in conjectures on the one hand and austerity in refutations on the other: this is Popper's recipe. Intellectual honesty then consists not in trying to entrench, or establish, one's position but in specifying precisely the conditions under which one is willing to give one's position up. Marxists and Freudians refuse to specify such conditions: this is the hallmark of their intellectual dishonesty. *Belief* may be a regretfully unavoidable biological weakness to be kept under the control of criticism: but *commitment* is for Popper an outright crime.

Kuhn thinks otherwise. He too rejects the idea that science grows by accumulation of eternal truths. He too takes his main inspiration from Einstein's overthrow of Newtonian physics. His main problem, too, is *scientific revolution*. But according to Popper, science is 'revolution in permanence', and criticism the heart of the scientific enterprise; while according to Kuhn, revolution is exceptional and, indeed, extrascientific, and criticism, in 'normal times', is anathema. Indeed, for him the transition from criticism to commitment marks the point where progress —and 'normal' science—begins. For him the idea that on 'refutation' one can demand the rejection, the elimination of a theory, is 'naïve' falsificationism. Criticism of the dominant theory and proposal of new theories are only allowed in the rare moments of 'crisis'. This last Kuhnian thesis has been widely criticised and I shall not discuss it. My concern is rather that Kuhn, having recognised the failure both of justificationism and falsificationism in providing rational accounts of scientific growth, seems now to fall back on irrationalism. While for Popper scientific change is rational or at least rationally reconstructible and thus falls in the realm of the *logic of discovery*, according to Kuhn scientific change—from one 'paradigm' to another—is a mystical conversion which is not and cannot be governed by rules of reason: it falls totally within the realm of the *(social) psychology of discovery*.[1]

The clash between Popper and Kuhn is then not merely over a technical point in epistemology. The clash is over our central intellectual values, about the role and value of theories and criticism in the growth of knowledge in the post-Einsteinian period. The methodological implications of the competing positions reach beyond theoretical physics to the underdeveloped social sciences and even further into moral and political philosophy[1a].

In this paper I shall first show that in Popper's philosophy two different positions are conflated. Kuhn understands only Popper, the naïve falsificationist (I shall call him Popper_1), and his criticism of Popper_1 is correct. I shall even strengthen it. But Kuhn does not understand a more sophisticated Popper—Popper_2—whose rationality goes beyond naïve falsificationism. I shall try to explain Popper_2's position and strengthen it, mainly by stripping it of naïve falsificationism. This improved Popperian position may escape Kuhn's strictures and provide a rational explanation of scientific revolution.

§2. *A clarification: $\textit{Popper}_0$, $\textit{Popper}_1$, $\textit{Popper}_2$.*

Let us see the conflicting theses in some detail. I start with a discussion of three frequently conflated positions whose authors I shall call Popper_0, Popper_1, and Popper_2.

[1] *Cp.* his [1969]. For an ambiguity in this Kuhnian position cf. *below*, p. 183, footnote 90.

[1a] According to Popper the number, faith or vocal energy of the protagonists of a theory—whether scientific or political—are irrelevant, for they have nothing to do with the truth-content of that theory. Kuhn (like Polanyi) suggests that strength of commitment matters more than (possibly even constitutes) truth in science: and thereby lends—no doubt unintendedly—respectability to the political *credo* of contemporary religious maniacs ('student revolutionaries').

(a) $Popper_0$ and dogmatic falsificationism. The empirical basis

According to $Popper_0$,[2] Newton's theory of gravity is better than Descartes's because Descartes's theory was refuted—proved false—by the *fact* that planets move in near-elliptical paths, and because Newton's theory explained everything that Descartes's theory had explained, and also explained the refuting facts. Analogously, according to $Popper_0$, Newton's theory was, in turn, refuted—proved false—by the anomalous perihelion of Mercury, while Einstein's explained that too. Thus science proceeds by bold speculations, followed by hard, conclusive refutations and followed again by still bolder, new, and, at least at the start, unrefuted speculations.

According to $Popper_0$, although science cannot *prove*, it can *disprove:* it 'can perform with complete logical certainty [the act of] repudiation of what is false', that is, there is an absolutely firm empirical basis of facts which can be used to disprove theories.

$Popper_0$'s position—as Popper constantly stresses—is untenable: 'no conclusive disproof of a theory can ever be produced'.[4] If one insists that 'refutation' consists in strict disproof, one 'will never benefit from experience, and never learn from it how wrong [one is]'.[5] So we may just as well forget about $Popper_0$.[6]

(b) $Popper_1$ and 'naïve' falsificationism. The 'empirical basis'.

Kuhn's $Popper_1$ is much more sophisticated than Ayer's, Nagel's and Medawar's naïve $Popper_0$. Kuhn knows that Popper consistently condemns $Popper_0$. But according to Kuhn this does not make any real difference. For, Kuhn contends, $Popper_1$,

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[2] $Popper_0$ is the imaginary author of a vulgarised version of Popperian philosophy of science, a phantom created by Ayer, Medawar, Nagel and others. I discuss him only because he is much more widely known than the more sophisticated $Popper_1$ and $Popper_2$.

[3] Medawar [1967], p. 144. Medawar calls this 'one of the strongest ideas in Popper's methodology' and approves of it. Nagel, reviewing Medawar's book, criticises Medawar for 'endorsing Popper's claims' ([1968], p. 70.)

[4] Popper [1934], section 9.

[5] *Ibid.*

[6] The strawman $Popper_0$ was originally invented by Ayer. Moreover, he invented also the myth that according to Popper 'definite confutability' was a criterion not only of the empirical but also of the meaningful character of a proposition. (Cp. his [1936], ch, I, p. 38 of the second edition. For a recent criticism of $Popper_0$ cp. Juhos [1966]).

'having barred conclusive disproof, has provided no substitute for it, and the relation he does employ remains that of logical falsification.'[7] Therefore, Kuhn concludes, 'though he is not a naïve falsificationist, Sir Karl [that is, Popper_1] may....legitimately be treated as one'.[8]

No doubt, Kuhn has a point there. Popper_1, unlike Popper_0, is real. Let us scrutinise Popper_1's philosophy of science.

Popper_1's philosophy of science is based on bold (that is, highly falsifiable) conjectures weeded out by hard refutations. For this he needs a 'logically' distinguishable set of 'observational statements' or 'potential falsifiers', or 'basic propositions'. These are distinguished syntactically: since they must be able to negate logically and spatio-temporally universal statements, without following from them, they must be spatio-temporally singular existential statements, like 'there is a planet in the spatio-temporal region *k*'.[9] But these propositions must also have a 'pragmatic' distinction: there has to be an experimental procedure, 'relevant technique', available and accepted at the time, with the help of which one can reach a *decision* about their truth-value; and finally there must be a strong logic, such that, if their truth-value is decided, this logic may establish whether they are consistent with the theory or not. In the latter case, the theory is refuted and ruthlessly rejected. But then the theory, in order to be criticisable and thus scientific, must also be neatly organised in a deductive model. The set of basic propositions, describing the possible worlds which theories forbid, constitutes their '*empirical content*', which is the crucial part of the '*empirical basis*'.[10]

The empirical basis, which provides the launching pad for refutations, consists then of statements on a *lower* level than the ones tested. In Popper's view this circumstance is of crucial

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[7] Kuhn [1969].

[8] *Ibid.*

[9] Popper [1934], section 28.

[10] Cp. Chapter V of Popper [1934]: 'The problem of the empirical basis'. Curiously, many philosophers overlooked Popper_1's important qualification that a basic statement has no power to refute anything without the support of a well-corroborated falsifying hypothesis. (Cp. section 22 of his [1934] on the contrast of falsifiabilty and falsification.)

importance: 'Only if the asymmetry between verification and falsification is taken into account—that asymmetry *which results from the logical relation between theories and basic statements*—is it possible to avoid the pitfalls of the problem of induction.'[11] And, according to Popper$_1$'s logic of research, if there is a clash between a low-level, 'observational' hypothesis and a higher-level theory—shown in his 'deductive model'—the theory must be rejected, eliminated from the body of science. Moreover, Popper$_1$ rules that although such empirical refutation does not *prove* that the refuted theory is *false*, the elimination, nevertheless, is methodologically conclusive: 'In general we regard an inter-subjectively testable falsification as final (provided it is well tested)....A corroborative appraisal made at a later date....can replace a positive degree of corroboration by a negative one, *but not vice versa*.'[12] Or, as Weyl put it in similar vein: 'I wish to record my unbounded admiration for the work of the experimenter in his struggle to wrest interpretable facts from an unyielding Nature who knows so well how to meet our theories with a decisive *No*—or with an inaudible *Yes*.'[13]

John Oulton Wisdom took up Popper$_1$'s rule and even generalised it to the clash between *any* higher and lower level theory: he suggested that, for instance, if a 'metaphysical' theory clashes with a highly corroborated theory, it must be rejected, eliminated, or in other words, that a highly corroborated scientific theory *refutes* a metaphysical theory which is inconsistent with it.[14]

It is this position which, I assume, Kuhn means by 'naïve falsificationism'. And, indeed, Popper$_1$'s naïve falsificationism is untenable. Well-corroborated falsifying hypotheses are frequently themselves refuted, and the falsified theory reinstated. Agassi gives a good example: the demise and subsequent resuscitation of Prout's theory.[15] According to Popper$_1$, 'once a hypothesis has been proposed and tested, and has proved its mettle, it

[11] Popper [1934], section 81; my italics.

[12] Popper [1943], section 82; my italics.

[13] Quoted in Popper [1934], section 85, with Popper$_1$'s comment: 'I fully agree'.

[14] Wisdom [1963].

[15] Agassi [1966], §5.

may not be allowed to drop out without "good reason". A "good reason" may be for instance....*the falsification of one of the consequences of the hypothesis*'.[16] But even if Keplerian ellipses had *refuted* the Cartesian theory of vortices, only Newton's theory made us *reject* it; even if Mercury's perihelion *refuted* Newtonian gravitation, only Einstein's theory made us *reject* it. All that a refutation does is to enhance the problematical tension of the body of science and indicate the urgent need of revising it—in some yet unspecified way.[17] But refutation alone is not sufficient reason to eliminate the 'refuted' theory. *Naïve falsification consists exactly of the conflation of refutation and rejection* (*or elimination*), and it is this thesis which is at the heart of Popper$_1$'s methodology. Naïve falsificationism is an age-old tradition, as old and as influential as justificationism. They have both thoroughly impregnated ordinary language: *this is why my separation of 'refutation' on the one hand and 'rejection'* (*or elimination*) *on the other, may sound paradoxical.*[18] Therefore it will be worth-while to see more clearly why 'naïve falsificationism' is naïve but, also, what is the *rationale* behind it.

First I shall criticise what I take to be the most important plank of naïve falsificationism: the 'mono-theoretical model of criticism'. This is a reconstruction of the critical situation *within* the deductive structure of the 'tested theory'—that is, as a matter purely for the theory under test and *its* 'basic statements'. In a Popperian$_1$ test-situation one single theory is confronted by potential falsifiers supplied by some authoritative experimental scientist.

But in the 'experimental techniques' of the scientist *theories* are involved—as Popper constantly stresses.[19] Let us take an example. Let us imagine that a big radio-star is discovered with a system of radio-star satellites orbiting it. We should like to test

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[16] Popper [1934], section 11; my italics.

[17] Indeed 'the dogmatic attitude of sticking to a theory as long as possible is of considerable significance' (Popper [1940], footnote 1).

[18] One of the main points of my [1963–4] is exactly that justificationism and naïve falsificationism are equally detrimental. Also cp. my [1968], p. 397, footnote 1.

[19] E.g., [1934], end of section 26; Popper enlarges on this passage e.g. in his [1968a], pp. 291–2.

some gravitational theory on this planetary system—a matter of considerable interest. Now let us imagine that Jodrell Bank succeeds in providing a set of space-time coordinates of the planets which is inconsistent with the theory. We shall take these statements as potential falsifiers; of course, these basic statements are not 'observational' in the usual sense—only " 'observational' "[20]; they describe planets that neither the human eye nor optical instruments can reach: their truth-value is arrived at by an 'experimental technique'. But this 'experimental technique' is based on a more or less well-corroborated theory (of radio-optics) which, by the way, has nothing to do with the gravitational theory that is being tested; a theory which therefore does not appear at all in the 'deductive model' based only on the tested theory and its initial conditions. Calling these statements 'observational' is a manner of speech for saying that in the context of our problem, that is, in testing our gravitional theory, we use radio-optics uncritically, as 'background knowledge'.[21]

(This situation does not really differ from Galileo's 'observation' of Jupiter's planets: moreover, as his theologian contemporaries rightly pointed out, he relied on a virtually non-existent optical theory—which then was less corroborated, and even less articulated, than present-day radio-optics. And again, calling the reports of our human eye 'observational' only indicates that we 'rely' on some physiological theory of human vision.[22])

But what if our best gravitational theory is refuted by the 'experimental techniques' of Jodrell Bank? Shall we accept this as the 'overthrow' of our theory? Why not interpret the result rather as the overthrow of radio-optics?

One can easily see that when we devise an experiment in order to test, to criticise a theory, we always have to use some 'observational theories' or 'touchstone theories' (or 'interpretative theories') uncritically if we want to make its 'falsification' possible. *Naïve*

[20] Popper, correctly, puts them in quotes; cp. his [1934], Section 28.
[21] Popper defines 'background knowledge' as 'all those things which we accept (tentatively) as unproblematic while we are testing the theory'. ([1963], p. 390.)
[22] For a fascinating discussion cp. Feyerabend [1969].

falsificationism demands, therefore, that at least in a given critical situation, the body of science be divided into two, the problematic and the unproblematic (the unproblematic is usually understood to be the well-corroborated). But this demand is irrational and dogmatic. Often 'unproblematic background knowledge' is not even well-corroborated, and the clue to progress may lie in its overthrow. And even if it is well-corroborated, nothing prevents us from inferring from a negative result to its falsehood. If we perform an experiment, it depends on our methodological decision which theory we regard as the touchstone theory and which one as being under test; but this decision will determine in *which* deductive model we shall direct the *modus tollens*. Thus a 'potential falsifier', B, of T_1, given some touchstone theory T_2, may refute T_1; but the same statement B, if regarded as a potential falsifier of T_2, given T_1 as touchstone theory, refutes T_2.

In a mono-theoretical model we either regard the higher-level theory as an *explanatory theory to be judged by the 'facts'* delivered from outside: in case of a clash we reject the explanation; or we regard the higher-level theory as an *interpretative theory to judge the 'facts'* delivered from outside: in case of a clash we reject the 'facts' as 'monsters'. There is no other possibility. Of course, in a pluralistic model we have more than these two alternatives. Thus the Popperian mono-theoretical model is a poor model for the critic: several theories—more or less deductively organised—are soldered to each other in 'testing'. But then we face a new *problem* in method: at which theory do we choose to direct the *modus tollens* in case of a 'refutation'?[23] All that experiments can show is an *inconsistency* of the theories involved—explicitly, or 'implicitly' as a hidden lemma—in the interpretation and explanation of the experiment, an inconsistency between the theories which were used as touchstone theories and the theory which was tested. How should we decide which theory to replace in order to remove this inconsistency?[24]

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[23] This problem is, of course, raised by the Duhem-Quine argument—*and left unsolved.*

[24] Of course, by removing any particular inconsistency *in the body of science* we never make it completely consistent.

The problem becomes still more difficult if we realise that the theories we criticise always contain, in addition, a '*ceteris paribus*'. This '*hidden background knowledge*' makes mono-theoretical refutation utterly irrelevant.

Of course, these considerations are not altogether new. Neurath criticised Popper_1's naïve falsificationism already in 1935 (he called it 'pseudo-rational falsificationism').[25] He pointed out that real science is not articulated in deductive models but only in loose 'encyclopedias' and that there is no reason why inconsistencies should not be removed by removing even well-corroborated 'falsifying hypotheses' rather than high-level theories, in the hope that the progress of science will show how the inconsistency can be properly solved. Indeed Neurath argues that submission to the tyranny of well-corroborated low-level hypotheses in some cases (which Neurath does *not* specify) becomes 'an obstacle to scientific progress'.[26] As Hempel put it later: 'A conflict between a highly-confirmed theory and an occasional recalcitrant experiential sentence may well be resolved by revoking the latter rather than by sacrificing the former'.[27] Hempel admits that he has no 'more fundamental standard' for deciding *which* to 'sacrifice'.[28] But Popper_1 anticipated Neurath's and Hempel's counter-arguments and made out a dramatic case against them already in 1934. *Just as Russell used to argue that one had to choose between inductivist justificationism and irrationalism,*[29] *Popper_1 argued that one had to choose between naïve falsificationism and irrationalism.* He warned that Neurath's methodology would make science unempirical and therefore irrational: 'We need a set of rules to limit the arbitrariness of "deleting" (or else "accepting") a protocol

[25] Neurath [1935].

[26] *Ibid.*, p. 356.

[27] Hempel [1952]. Agassi, in his [1966], follows Neurath and Hempel, esp. pp. 16 ff. It is rather amusing that, in making this point, he thinks he is fighting 'the whole literature concerning the methods of science'.

[28] Hempel [1952], p. 622. Hempel's crisp 'theses on empirical certainity' do nothing but refurbish Neurath's (and some of Popper's) old arguments (against Carnap, I suppose); but, deplorably, he does not mention either his predecessors or his adversaries.

[29] 'I do not see any way out of a dogmatic assertion that we know the inductive principle, or some equivalent; the only alternative is to throw over almost everything that is regarded as knowledge by science and common sense'. (Russell [1943], p. 683).

sentence. Neurath fails to give any such rules and thus unwittingly throws empiricism overboard...Every system becomes defensible if one is allowed (as everybody is, in Neurath's view) simply to "delete" a protocol sentence if it is inconvenient'.[30]

I think that Popper's argument *does* show that unless we have *some* rule of logic of research to guide us in such a situation, we must fall back on psychologistic irrationalism. But Popper_1's naïve falsificationism offers an untenable solution, based on his idea of empirical basis and mono-theoretical deductive method. Popper frequently emphasises the fallibility of the empirical basis; he even says that his only intention in introducing this term at all was 'to give an ironical emphasis' to his thesis that the empirical basis is not as firm as Popper_0 would have it.[31] He even stressed that since this basis is fallible it should be criticisable.[32] But if the criticisability of the empirical basis is to be taken seriously, do we not 'throw empiricism overboard'? Therefore, explained Popper, we *must* accept the majority verdict of the scientists' jury in the truth value of basic statements if we do not want to turn 'the soaring edifice of science' into a 'new Babel'.[33] The only possibility Popper specified—in 1957—for a *modification* of the empirical basis is the one when a new 'deep' theory T_1 is proposed, inconsistent with the reigning theory of the time T_0 but consistent with the empirical basis at least within the limits of observational error. T_0 will still be 'approximately true'—but by developing *new* experimental techniques for testing T_1 against T_0, we make the empirical basis more precise and a decision can be reached. This happened, according to Popper, when Newton's theory superseded Kepler's[34]. When Newton's theory was put forward, Kepler's laws had not been refuted, the discrepancies could not yet be observed. These discrepancies, however, stimulated a

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[30] Popper [1934], Section 26.

[31] Popper [1963], p. 387.

[32] 'Observational evidence statements...are contaminated by theories. Thus they must not be accepted uncritically'. Popper [1968a], p. 292. Also cp. his [1934], Section 29.

[33] Popper [1934], Sections 29 and 30.

[34] Popper [1957].

sharpened specification of the empirical basis and this *improved* (but not *overthrown*) empirical basis decided the matter in favour of Newton's theory.[35]

But even this argument left some of Popper's colleagues unsatisfied. Agassi, for instance, as I already mentioned, discussed an example where a low-level falsifying hypothesis was later not just *improved* but *overthrown*—while the original theory prevailed. His example was Prout's theory T ('all atoms are compounds of hydrogen atoms and thus "atomic weights" of all chemical elements must be expressible as whole numbers') and Stas' 'refutation' R ('the atomic weight of chlorine is 35.5').[36] As we know, in the end T prevailed over R. Agassi claimed that his example showed that we may 'stick to the hypothesis in the face of known facts in the hope that the facts will adjust themselves to theory rather than the other way round'.

But *how* can facts 'adjust themselves'? Agassi's answer, in spite of his valuable hints, is not clear; and, indeed, the answer cannot be given without a radical revision of Popper's monotheoretical deductive model of criticism.

The first stage of any serious criticism of a scientific theory is to reconstruct its deductive structure. Let us do this in the case of Prout's theory *vis à vis* Stas' refutation. First of all, we have to realise that in the formulation we just quoted T and R were *not* inconsistent. (Physicists rarely articulate their theories sufficiently to be pinned down and caught by the critic.) In order to make them inconsistent we put them in the following form. T: 'the atomic weight of all pure (homogeneous) chemical elements are multiples of the atomic weight of hydrogen', and R: 'chlorine is a pure (homogeneous) chemical element and its atomic weight is 35.5'. The last statement is in the form of a falsifying hypothesis which, if well-corroborated, would allow us to use basic statements of the form B: 'Chlorine X is a pure (homogeneous) chemical

[35] Cp. Popper [1957] and Popper [1963], p. 186. Popper, in several places, expounds the historical myth that Kepler's and Galileo's theories were not 'refuted' before Newton; e.g. his [1963], pp. 246 and 256. For the true story cp. my [1969].

[36] Agassi [1966], p. 18.

element and its atomic weight is 35.5'—where X is the proper name of a 'piece' of chlorine determined, say, by its space-time co-ordinates.

But how well-corroborated is R? The first component of it says that R_1: 'chlorine is a pure chemical element'. This was the verdict of the rigorous application of the 'experimental techniques' of the day.

Let us have a closer look at the fine-structure of R_1. In particular let us consider T': 'if all chemical purifying procedures $p_1, p_2, \ldots p_{17}$ are applied to a gas, what remains will be pure chlorine'. The 'careful experimenter' carefully applied all 17 procedures. Therefore what remained *must be* pure chlorine: this a 'hard fact' in virtue of T'. He *interpreted* what he saw 'in the light of T^1': the result was R_1.

But what if T' (the touchstone theory—or, rather, *interpretative theory*) is false? Why not argue that from T it follows that atomic weights must be whole numbers? Then *this* will be a 'hard fact' in virtue of T, and T' will be overthrown. Perhaps additional new purifying procedures must be invented and applied.

The problem is then *not* when we should stick to a '*theory*' in the face of '*known facts*' and when the other way round. The problem is *not* what to do when 'theories' clash with 'facts'. Such a 'clash' is only suggested by the mono-theoretical deductive model. Whether a proposition is a '*fact*' or a '*theory*' depends on our methodological decision. 'Empirical basis' is a mono-theoretical notion, it is *relative* to some mono-theoretical deductive structure. In the pluralistic model the clash is between two high-level theories: an *interpretative theory* to provide the facts and an *explanatory theory* to explain them; and the interpretative theory may be on quite as high a level as the explanatory theory. The problem is not whether a refutation is real or not. The problem is how to repair an inconsistency between the 'explanatory theory' under test and the—explicit or hidden—'interpretative' theories; or, if you wish, *the problem is which theory to consider as the interpretative one which provides the 'hard' facts and which the explanatory one which 'tentatively' explains them.* Thus experiments do not overthrow theories, as Popper_1 has it, but only increase the problem-fever of the body of science. *No theory forbids some state of affairs specificable in advance; it is not that we propose a theory and Nature may shout NO. Rather, we propose a maze of theories, and Nature may shout INCONSISTENT.*[37]

The problem is then shifted from the problem of replacing a theory refuted by 'facts' to the new problem of how to resolve inconsistencies between closely associated theories. Which of the mutually inconsistent theories should be eliminated? This problem can be solved in a novel way with the help of Popper_2's ideas.

(*c*) *Popper_2 and growth.*

Popper_1's famous slogans are: 'make sincere attempts to refute your theories', 'we learn from our mistakes', 'a refutation is a victory'. The whole scientific effort is geared towards producing counterexamples, and as Kuhn correctly says, Popperian_1 progress consists of repeated overthrows of theories. The concentration on 'refutation' and the conflation of 'refutation' and rejection: these are Popper_1's central tenets.

Popper_2 concentrates on *growth*, not on *refutation*. His problem is how to appraise *which is the best among competing possibly false theories*. He discards the *justificationist solution* that a theory is better than its rival if it is proved, while its rival unproved: all theories are equally unproved (and, of course, equally unprovable). He discards the (neo-justificationist) *probabilistic solution* that a theory is better than its rival if it is more probable in the light of observational evidence: he shows that all theories are equally improbable. He discards the *instrumentalist solution* that a theory is better than its rival if it is 'simpler' than its rival from the point of view of intellectual comfort. Instead of all these he proposes

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[37] Let me here answer a possible objection: 'Surely we do not need Nature to tell us that a set of theories is *inconsistent*. Inconsistency—unlike falsehood—can be ascertained without Nature's help.' But Nature's actual 'NO' in a mono-theoretical methodology takes the form of an asserted 'potential falsifier', that is a sentence which, in this way of speech, we claim Nature had uttered and which is the *negation of our theory*. Nature's actual 'INCONSISTENT' in a pluralistic methodology takes the form of a 'factual' statement couched in the light of one of the theories involved, which we claim Nature had uttered and which, if added to our proposed theories, yields an *inconsistent system*.

his own solution: a theory is better than its rival (*a*) if it has more empirical content, that is if it forbids more 'observable' states of affairs, and (*b*) if some of this excess content is corroborated, that is, if the theory produces novel facts.[38]

In contrast to $Popper_2$'s powerful rules of acceptance, $Popper_1$'s naïve falsificationism still appears in $Popper_2$'s rules of elimination which still identify refutation and rejection. But we have already shown that naïve falsificationism is untenable; let us now adduce a final crucial argument against it.

Since all theories are born refuted[39], bare 'refutations' can play no dramatic role in science. If new theories emerge in the midst of an ocean of counterexamples, slowly digesting—or even producing and digesting—their refutations, the injunction to 'make sincere attempts to refute your theories', falls completely flat. A corroborated falsifying hypothesis does not have sufficient power to enable a counterexample to eliminate a theory. If it had, we would eliminate all science instantly. A counterexample, in order to *reject*, to *eliminate* a theory, needs more powerful support than that which a lower-level falsifying hypothesis can provide: it needs the support of a theory with more corroborated content, with wider explanatory power. *There must be no elimination without the acceptance of a better theory.*[40] $Popper_1$'s and $Popper_2$'s problem was when is an unrefuted theory better than a refuted rival one. The problem now shifts to the problem when is a theory better than its rival if both are known to be refuted. Now $Popper_2$'s thesis—inherited from $Popper_1$—that theories are *either* corroborated *or* refuted is false. But for

[38] $Popper_1$'s and $Popper_2$'s philosophy can be best described in terms of (tentative) prior and posterior 'acceptance' ($acceptance_1$ and $acceptance_2$) and 'elimination' of theories. *According to $Popper_1$*, we '$accept_1$' any theory which is refutable ('prior acceptance') and '$accept_2$' it to the degree that it stands up to our 'sincere attempts' to refute it. We eliminate it on mere refutation (on the $acceptance_2$ of a 'falsifying hypothesis'). *According to $Popper_2$*, we $accept_1$ a theory which has excess empirical content over its rival or predecessor; we $accept_2$ it if some of this excess content is corroborated; we eliminate it on mere refutation. For a detailed discussion of '$acceptability_1$' and '$acceptability_2$' cp. my [1968], pp. 375–90.

[39] The truth of this remark will be obvious to the historian of science. As Kuhn aptly says: 'If any and every failure to fit were ground for theory rejection, all theories ought to be rejected at all times.' ([1962], p. 145).

[40] For a slight qualification cp. my [1968], pp. 385–6.

Popper$_2$'s position this thesis is irrelevant since *the two (prior and posterior) comparative appraisals of acceptance of Popper$_2$ apply also to refuted theories.* The first appraisal compares empirical contents which have nothing to do with truth or falsity; the second appraisal compares novel corroborated content: but we can make this comparison irrespective of their—old or new—refuted content.[41] Indeed, we know that *both* the Cartesian and Newtonian theories were known to be refuted at the same time when the latter superseded the former: and we also know that *both* the Newtonian and Einsteinian theories were known to be refuted when the latter superseded the former.[42])

Let me now introduce a couple of new terms for Popper$_2$'s logic of discovery. Since Popper$_2$ always appraises theories by comparing them the appraisal is rather of a *series of theories* than of an isolated *theory*. Now let us call a series of successive theories each of which is acceptable$_1$—that is, each of which has higher content than its predecessor—a (*theoretically*) *progressive problem-shift*. Let us call a series of theories each of which is also acceptable$_2$—that is, each of which produces '*facts*' not entailed by its predecessor—an (*empirically*) *progressive shift*. But if theories are falsified all the time, they are problematic all the time, and therefore we may speak of progressive *problem-shifts*. If the problem-shift is not progressive. we call it *degenerating*. If we put forward a theory to resolve a contradiction between a previous theory and a counterexample in such a way that the new theory, instead of offering a—content-increasing—scientific *explanation*[43], only offers a—content-decreasing—linguistic *reinterpretation*, the contradiction is resolved in a merely semantical, unscientific way. Popper$_2$ forbids the use of such unscientific content-decreasing stratagems.[44] *Then Popper's (Popper$_2$'s) celebrated demarcation*

[41] For a detailed discussion cp. my [1968], especially pp. 384–6.

[42] For a detailed discussion cp. my [1969].

[43] Indeed, in the original manuscript of my [1968] I wrote: 'A theory without excess corroboration has no excess explanatory power; therefore, according to Popper, it is not scientific; *therefore, we should say, it has no explanatory power.*' (p. 386). I cut out the second half of the sentence in fear of sounding too eccentric. I regret it now.

[44] He calls them conventionalist stratagems (Popper [1934], sections 19 and 20). I have discussed, under the heads 'monster-barring', 'exception-barring', 'monster-adjustment' such stratagems as they appear in informal, quasi-empirical mathematics in some detail; cp. my [1963–4]).

criterion can be reformulated as contrasting progressive (scientific) and degenerating (pseudo-scientific) problem-shifts.[45]

Popper originally had only the *theoretical* aspect of problem-shifts in mind, based on acceptance$_1$. If the reader is in doubt about the authenticity of my reformulation of Popper's demarcation criterion, he should re-read the relevant parts of Popper [1934], with Musgrave [1968] as a guide. Popper's ambiguous usage of 'theory' and 'series of theories superseding one another' was, I think, one of the major impediments to his getting his idea across; this ambiguity led to such confusing formulations as 'Marxism [as a series of theories (or as a research-programme)] is irrefutable' and, at the same time, 'Marxism [as a theory] has been refuted.' Not an isolated *theory*, but only a series of theories—or a research programme—can be said to be scientific or unscientific.[46]

But Popper$_2$ can easily get rid of Popper$_1$'s untenable, falsificationist, elimination rule and replace it by a different one which is wedded naturally to his acceptance rules. According to this new rule, if we have two conflicting theories, one explanatory and one interpretative, and we do not know which is which—that is, we do not know which should prevail as the interpretative theory providing the *facts*—we have to try to replace first one, then the other, then possibly both, and opt for that new set-up which represents the most progressive problem-shift, with the biggest increase in corroborated content.

Popper$_2$ can also solve the problem of finding a 'guilty hidden lemma'.[47] For, in the spirit of Popper$_2$, we can admit that the premises form an indefinite—or even infinite—conjunction,

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[45] There are two differences. One is that I improved Popper's conception of an empirical problem-shift (that is, his original conception of 'acceptability$_2$'; cp. my [1968], pp. 388–90). The other is that, according to Popper, a progressive theory *never* adopts a content-decreasing stratagem to absorb a counterexample, it *never* says that 'all bodies are Newtonian except for seventeen anomalous ones'. But since such yet unexplained anomalies always abound I allow such anomalies; an explanation is a step forward (that is 'scientific') if it explains at least *some* previous anomalies which were not explained scientifically by its predecessor. As long as anomalies are regarded as genuine problems, it does not matter much whether we dramatise them as 'refutations' or de-dramatise them as 'exceptions': the difference *then* is only a linguistic one.

[46] Cp. my [1968], especially pp. 378–9.

[47] Cp. *above*, p. 158.

summed up in each finite version by an '*etc.*'; we may also direct the *modus tollens* at this '*etc.*' and replace it by the conjunction of a *newly* articulated premise and a new '*etc.**', thereby trying to bring about a progressive problem-shift. But since in actual scientific theories these '*etc.*' type premises are not written out, one might say that *the target of the arrow of refutation is shaped while the arrow is already in the air.* Criticism does not *assume* a fully articulated deductive structure: it creates it. The true deductive model of explanation is an ever-changing one; one in which propositions keep being added and deleted. *One may not explain what one has set out to explain; one may not refute what one has set out to refute.*[48]

Let us finally mention that the separation by Popper_2 of the notions of (low-level) 'refutation' and growth, soldered together by Popper_1, is so sharp, that according to Popper_2 science can grow without any 'refutations' leading the way. It is perfectly *possible* that theories be put forward 'progressively' in such a rapid succession that the refutation of the n-th appears only as the corroboration of the $n+1$-th. According to Popper_1, the growth of science is *linear*, in the sense that theories are followed by eliminating refutations, and these refutations in turn by new theories.[49] According to Popper_2, the growth of science is pluralistic: '[Elimination] depends on [the condition] that sufficiently many and sufficiently different theories are offered'.[50] This pluralistic aspect of Popper_2's philosophy was elaborated and further developed by Paul Feyerabend.[51]

However, even our improved Popper_2 has left the problem of the remarkable *continuity* in science unsolved. Scientists (and, as I have shown, mathematicians too) tend to ignore counterexamples, or as they prefer to call them, 'recalcitrant' or 'residual' instances;

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[48] I discussed the problem of 'hidden' background knowledge in my [1963–4], esp. pp. 224–6.

[49] E.g. Popper [1934], section 85, p. 279 of the 1959 English translation.

[50] Popper [1940] and Popper [1968e], p. 96.

[51] Feyerabend acknowledges that he learned the gist of his 'principle of proliferation' from Popper_2's lectures which he attended in 1948 and 1952. (Feyerabend [1962], p. 32.)

and elaborate and apply their theories regardless. Their theories frequently show remarkable *tenacity*.[52] They usually claim, as even Popper_2 tells us in horror, 'that the experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding.'[53] Such people, says Popper_2, 'are adopting the very reverse of that critical attitude which... is the proper one for the scientist'.[54] But it is widely acknowledged that 'the dogmatic attitude of sticking to a theory as long as possible is of considerable significance. Without it we could never find out what is in a theory—we should give the theory up before we had a real opportunity of finding out its strength; and in consequence no theory would ever be able to play its role of bringing order into the world, of preparing us for future events, of drawing our attention to events we should otherwise never observe.'[55] This dogmatic attitude in science—which would explain its stable periods—was described by Kuhn as a prime feature of 'normal science'.[56] Does the dogmatism of 'normal science' prevent growth? I shall, in §3, develop a sort of fine-structure of Popper_2's methodology, and show, with its help, that there is good, progressive normal science and that there is bad, degenerating normal science and Popper_2's demarcation criterion —in a slightly improved form—can be used to enable us to draw a line between them.

§3. *Scientific research programmes: negative and positive heuristic.*

I have discussed progressive and degenerating problem-shifts in series of successive theories. But in history of science we find a *continuity* which connects such series. This continuity evolves from a genuine *research programme* adumbrated at the start. The

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[52] Feyerabend coined the term 'principle of tenacity', for this phenomenon. Cp. his [1967], p. 177.

[53] Popper [1934], section 9.

[54] *Ibid.*

[55] Popper [1940], first footnote. We find a similar remark also in his [1969]. But these remarks cannot be regarded as anything but a reluctant admission of an undigested anomaly in the Popperian research programme.

[56] I described it in informal mathematics; cp. my [1963–4), *passim*, but especially in §8 (*d*) 'Continuous versus critical growth' (pp. 324–30).

programme consists of methodological rules: some tell us what paths of research to avoid (*negative heuristic*), and others what paths to pursue (*positive heuristic*).[57]

Even science as a whole can be regarded as a huge research programme with Popper$_2$'s supreme heuristic rule: 'devise conjectures which have more empirical content than their predecessors'. Such methodological rules may be formulated, as Popper pointed out, as metaphysical principles.[58] For instance, the *universal* anti-conventionalist rule against exception-barring may be stated as the metaphysical principle: 'Nature does not allow exceptions.' This is why Watkins called such rules 'influential metaphysics'.[59]

What I have primarily in mind is not science as a whole, but rather *particular* research-programmes, such as the one known as 'Cartesian metaphysics'. Cartesian metaphysics, that is, the mechanistic theory of the universe—according to which the universe is a huge clockwork with push as the only cause of motion—functions as a powerful heuristic principle: excluding, on the negative side, all scientific theories—like the 'essentialist' version of Newton's theory of action at a distance—which are inconsistent with it (*negative heuristic*) and implying, on the positive side, a 'metaphysical' research-programme to look behind all phenomena (and theories) for explanations based on clockwork mechanisms (*positive heuristic*).[60]

(*a*) *Negative heuristic.*

All scientific research-programmes may be characterised by their '*hard core*'. The negative heuristic of the programme forbids us to direct the *modus tollens* at this 'hard core': it bids us to articulate or even invent with great ingenuity touchstone theories,

[57] By 'path of research' I mean an objective concept describing something in the Platonic 'third world' of ideas: a series of successive theories, each one 'eliminating' its predecessors. For the idea of 'third world' cp. *below* p.35

[58] Popper [1934], sections 11 and 70. I use 'metaphysics' as a technical term, as defined by Popper in his [1934].

[59] Watkins [1958]. Watkins cautions that 'the logical gap between statements and prescriptions in the metaphysical-methodological field is illustrated by the fact that a person may reject a [metaphysical] doctrine in its fact-stating form while subscribing to the prescriptive version of it.' (*Ibid.*, pp. 356-7.)

[60] For this Cartesian research-programme cp. Popper [1958] and Watkins [1958], pp. 350–1.

'auxiliary hypotheses', which build up a *protective belt* around this core, and redirect the *modus tollens* on to *these.* It is this protective belt of auxiliary hypotheses which has to bear the brunt of tests and get adjusted and readjusted, or even completely replaced in the defence of the thus hardened core. A research-programme is successful if in the process it leads to a progressive problem-shift; unsuccessful if it leads to a degenerating problem-shift.

Let us take an example. Newton's gravitational theory was possibly the most successful research-programme ever. When it was first produced, it was submerged in an ocean of counter-examples, 'anomalies', and opposed by the observational theories supporting these anomalies. But Newtonians turned, with brilliant tenacity and ingenuity, one counter-instance after another into corroborating instances. In the process they themselves produced new counter-examples which they again resolved. They 'turned each new difficulty into a new victory of their programme.'[61]

I used to give in my lectures the following imaginary example of Newtonian growth. Let us take Newton's mechanics and the law of gravitation (the hard core C of the programme); and the initial conditions in some planetary system and several observational theories (the protective belt B_1). Let us imagine that a planet p slightly disobeys the theory N_1, made up from C and B_1. Would the Newtonian consider that this refutes C? No. He will suggest changing the hypotheses, say, about the initial conditions and will suggest that there must be a hitherto unknown, very small planet, p', perturbing the orbit of p. He would propose an auxiliary theory of p' describing its orbit, mass, etc. Then he will test the proposed orbit of p', replacing B_1 by B_2. He would try to plan bigger telescopes to make this conjectural orbit of p' discernible, testable. But if it seems that the conjectured planet is not in the reach even of the biggest optical telescopes, he may try some quite new instrument (like a radiotelescope) in order to enable us to 'observe it', that is , to ask—however indirectly—Nature about it. The new observational theory may itself be poorly articulated, but for the time being they will not care. If the new instrument

[61] Laplace [1796], Livre IV, Chap. II.

locates the planet where C & B_2 predicted, the result will be hailed as a victory for the research-programme (arrived at by sacrificing B_1) and, incidentally, also for the new observational theory.

If the planet is not found, would the Newtonian consider that this refutes C? No. Would he consider that this refutes his theory about the disturbing small planet? No. He will suggest, say, that a cloud of cosmic dust must hide the planet from us: B_2 which recorded no such cloud, was in this respect false. He will calculate the location and properties of this cloud (thereby introducing B_3) and send a satellite to test it. If the satellite's instruments (possibly with the help of yet another weakly-tested 'observational theory') record the existence of the conjectured cloud, the result will be hailed as a big victory for the research-programme (arrived at by modifying, or if you wish, sacrificing B_2) and, incidentally, also for the new observational theory.

If the satellite records no such cloud, would the Newtonian consider that this refutes C? No. He might still stick to his imaginary small planet, to his imaginary cloud, and suggest, say, that the cloud *is* there, but the observational theory on which the satellite's experimental techniques were based, were false. B_3 too will be modified and some B_4 proposed...and possibly corroborated!

In this contrived case the successive versions of the research-programme constitute a *consistently progressive theoretical shift:* each step represented an increase in empirical content. This is paired with an *intermittently progressive empirical shift* : not every step produced immediately a new fact.[62] But who would doubt that we described an outstanding success in science? Thus we shall call a shift progressive if it is consistently progressive in empirical content and at least intermittently progressive in corroboration. The term 'intermittently' gives sufficient scope for dogmatic adherence to a programme within the bounds of rationality.

(*b*) *Positive heuristic.*

We should note, however, that even a most rapidly and consistently progressing research-programme can digest its counter-

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[62] For 'theoretical' and 'empirical' shifts cp. *above* p. 164.

examples only piecemeal. But it should not be thought that yet unexplained anomalies—'puzzles', as Kuhn might call them—are taken in random order, and the protective belt built up in an eclectic fashion, without any preconceived order. The order is usually decided in the theoretician's cabinet, independently of the *known* anomalies. This order of research, research policy, is predetermined—more or less—by the *positive heuristic* of the research-programme. While the negative heuristic specifies the '*hard core*' of the programme, the positive heuristic consists of a partially articulated set of suggestions or hints on how to develop the 'refutable variants' of the research-programme, how to modify, sophisticate, the protective belt. The 'core' of a research-programme is 'irrefutable' by the methodological decision of its protagonists: but the 'protective belt' of auxiliary hypotheses can be modified, 'refuted'.[63]

The positive heuristic of the programme saves the scientist from becoming confused by the ocean of anomalies. The positive heuristic sets out a programme which lists a chain of ever more complicated *models* simulating reality: the scientist's attention is riveted on building his model following instructions which are laid down in the positive part of his programme. He ignores the *actual* counter-examples, the available '*data*'.[64] Newton first worked out his programme for a planetary system with a fixed point-like sun and one single point-like planet. It was in this

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[63] This shows how wrong Popper is when he attributes so much weight to *syntactical* refutability. He thinks that there is a sharp division between syntactically refutable and syntactically irrefutable propositions. Syntactically irrefutable propositions cannot be criticised with irresistible force; syntactically refutable propositions must be so criticised. Thus he—and his students—conflate 'metaphysical' in the syntactical and in the methodological sense. But here is an example when a theory is syntactically refutable (at least in Popper's mono-theoretical model!) but we treat it—rationally—as methodologically metaphysical. Popper_2 himself showed that there are propositions, like probabilistic hypotheses, which are syntactically irrefutable, but—by our decision—methodologically refutable; but I show that there are propositions like Newton's theory, which in Popper's mono-theoretical interpretation are refutable, but yet may be treated as methodologically irrefutable, methodologically metaphysical.

[64] If a scientist (or mathematician) has a positive heuristic, he refuses to be drawn into observation. He will 'lie down on his couch, shut his eyes and forget about the data'. (Cp. my [1963–4], esp. pp. 300 ff., where there is a detailed case study for such programme.) Occasionally, of course, he will ask Nature a shrewd question.

model that he derived his inverse square law for Kepler's ellipsis. But this model was forbidden by Newton's own third law of dynamics, therefore the model had to be replaced by one in which both sun and planet revolved round their common centre of gravity. This change was not motivated by any observation (the data did not then suggest here an 'anomaly') but by a theoretical difficulty. Then he worked out the programme for more planets as if there were only heliocentric but no interplanetary forces. Then he worked out the case where the sun and planets were not mass-points but mass-*balls*. This change again did not *need* the observation of an anomaly; rather, infinite density was forbidden by an (inarticulated) touchstone theory, therefore planets *had* to be extended. This change had considerable mathematical difficulties and held up Newton's work—and delayed the publication of the *Principia* by more than a decade.[65] Having solved this 'puzzle', he started work on *spinning balls* and their wobbles. Then he admitted interplanetary forces and started work on *perturbations*. At this point he started to look more anxiously at the facts. Many of them were beautifully explained (qualitatively) by this model, many were not. It was then that he started to work on *bulging* planets, rather than round planets, etc.

Newton despised people who, like Hooke, stumbled on a first naïve model but did not have the tenacity and ability to develop it into a research-programme, and who thought that a first version, a mere aside, constituted already a 'discovery'. He held up publication until his programme had achieved a remarkable progressive shift.

But most, if not all, Newtonian 'puzzles', leading to a series of new variants superseding each other were forseeable at the time of Newton's first naïve model and no doubt Newton and his colleagues *did* foresee them: Newton must have been fully aware of the blatant falsity of his first variants.[66] Nothing shows the existence of a positive heuristic of a research-programme better than this fact: this is why one speaks of 'models' in research-programmes.

[65] Bohr would have postponed the publication of his theory of the atom if not for Rutherford's encouragement to publish its first, unsophisticated and obviously false version.

[66] For a detailed discussion of Newton's research-programme cp. my [1969].

A '*model*' is a set of initial conditions (possibly together with some of the observational theories) which one knows is *bound* to be replaced during the further development of the programme, and one even knows, more or less, exactly how. This shows once more how irrelevant refutations of any specific variant are in a research-programme: their existence is fully expected, the positive heuristic is there as the strategy both to predict (produce) and to digest them. (Not that suprises are excluded; indeed, they are bound to occur. This may first lead to a need for a more *creative* development of the positive heuristic; and later may, with the help of a rival programme, overthrow the research-programme altogether.)

One may formulate the 'positive heuristic' of a research-programme as a 'metaphysical' principle. For instance one may formulate Newton's programme like this: 'the planets are essentially gravitating spinning-tops of roughly spherical shape.' This idea was never *rigidly* maintained: the planets are not *just* gravitational, they have also electro-magnetic, etc., characteristics which even influence their motion. It is better therefore to separate the 'hard core' from the more flexible metaphysical principles expressing the positive heuristic.

Our considerations show that the positive heuristic forges ahead with almost complete disregard to the 'refutations': it may seem that it is the *verifications*[67] rather than the refutations which provide the contact points with reality.[68] Although one must point out that any verification of the $n+1$-th version of the programme is a refutation of the n-th version, we cannot deny that *some* defeats of the subsequent versions are always foreseen: it is the verifications which keep the programme going, recalcitrant instances notwithstanding.

We may appraise research-programmes, even after their 'elimination', for their *heuristic power:* how many new facts they pro-

[67] A 'verification' is a corroboration of excess content in the expanding programme.

[68] Unfortunately I cannot here discuss this point in detail. I do it in a case study on Bohr's research-programme in my [1969]. Another classical example is provided by the story of kinetic gas theory.

duced, how great was 'their capacity to explain their refutations in the course of their growth'.[69]

The dialectic of positive and negative heuristic can be seen very clearly in the case of Prout's research-programme. Prout, in a paper published anonymously in 1815, claimed that the atomic weights of all pure chemical elements are whole numbers. He knew very well that anomalies abounded: but he proposed that these are due to the fact that chemical substances as they ordinarily occurred were *impure*. The protagonists of Prout's theory therefore embarked on a research-programme to separate pure elements. Such a programme would have been meritorious even if Prout's idea had really been completely 'without foundation', as Stas concluded in 1860.[70] Prout's theory defeated the theories previously applied in purification of chemical substances one after the other. Stas, like many others, became tired of the research-programme and gave it up, since the successes were still far from adding up to a final victory.[71] But others were not discouraged. Marignac immediately retorted that 'although [—he is satisfied that—] the experiments of Monsieur Stas are perfectly exact, [there is no proof] that the differences observed between his results and those required by Prout's law cannot be explained by the

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[69] Cp. my [1963–64], pp. 324–30. Unfortunately, in 1963–64 I had not yet distinguished theories and research programmes, and this fact impaired my exposition of a research programme in informal, quasi-empirical mathematics.

[70] Prout was very much aware of the methodological nature of his programme. Let us quote the first lines of his [1815]: 'The author of the following essay submits it to the public with the greatest diffidence; for though he has taken the utmost pains to arrive at the truth, yet he has not that confidence in his abilities as an experimentalist as to induce him to dictate to others far superior to himself in chemical acquirements and fame. He trusts, however, that its importance will be seen, and that some one will undertake to examine it, and thus verify or refute its conclusions. If these should be proved erroneous, still new facts may be brought to light, or old ones better established, by the investigation; but if they should be verified, a new and interesting light will be thrown upon the whole science of chemistry.'

[71] Clerk Maxwell was on Stas' side: he thought it was impossible that there should be two kinds of hydrogen, 'for if some [molecules] were of slightly greater mass than other, we have the means of producing a separation between molecules of different masses, one of which would be somewhat denser than the other. As this cannot be done, we must admit [that all are alike]'. (*Theory of Heat*, 1871.)

imperfect character of experimental methods.'[72] And as Crookes put it in 1886: 'Not a few chemists of admitted eminence consider that we have here [in Prout's theory] an expression of the truth, masked by some residual or collateral phenomena which we have not yet succeeded in eliminating'.[73] That is, there must be some further false hidden assumption in the touchstone theories on which 'experimental techniques' for chemical purification were based and with the help of which atomic weights were calculated: some present 'atomic weights merely represent a mean value'.[74] Indeed, Crookes went on to put this idea in a scientific (content-increasing) form: he proposed concrete new theories of 'fractionation' to serve as a 'sorting Demon'.[75] But, alas, his new observational theories turned out to be as utterly false as they were bold and they were thus eliminated from the (rationally reconstructed) history of science. As it turned out a generation later, there was a very simple hidden assumption which failed the researchers: that two pure elements must be separable by *chemical* methods. The idea, that two different pure elements may behave identically in all *chemical* reactions but can be separated by *physical* methods, required a change—a 'stretching'—of the concept of 'pure element' which constituted a change—a *concept-stretching expansion*—of the programme itself.[76] This revolutionary, highly creative shift was taken only by Rutherford's school.[77] But the *creative step* was in fact only a side-result of progress in a distant research programme; Proutians did not have the courage and imagination to try for instance to build strong centrifugal machines to separate elements.

Let us, however, stress that in the methodology of research-programmes here proposed there was never any reason to *eliminate*

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[72] Marignac [1860].

[73] Crookes [1886].

[74] *Ibid.*

[75] Crookes [1888], p. 491.

[76] For 'concept-stretching' my [1963–4].

[77] The shift is anticipated in Crookes' fascinating [1888] where he indicates that the solution should be sought in a new demarcation between 'physical' and 'chemical'. But the anticipation remained philosophical; it was left to Rutherford and Soddy to develop it, after 1910, into a scientific theory.

Prout's programme: indeed, the programme produced a beautiful progressive shift, even if, in between, there were considerabl, hitches.[78]

It is incredible how much the progress of science was slowed down by justificationism and by naïve falsificationism. (The opposition to atomic theory in the nineteenth century was fostered by both.) An elaboration of this particular influence of bad methodology on science would institute a rewarding research-programme for the historian of science.

(*c*) *A new look at crucial experiments.*

Popper, as we have seen, did not explain some important aspects of *continuity* in the growth of science. But did we not go in our 'anti-falsificationist' approach so far to the other extreme that now we are bound to say that even the celebrated' '*crucial experiments*' have no force to overthrow a research-programme?

The answer is very easy. In the progress of science there is a *proliferation of competing research-programmes.* The first 'naïve' models of competing programmes deal usually with different aspects of the domain (e.g. the first model of Newton's semi-corpuscular optics described light-refraction, the first model of Huyghens' wave optics light interference). As the rival research-programmes expand, they gradually encroach on each other's territory and the n-th version of the first will be blatantly, dramatically inconsistent with the m-th version of the second. The first is defeated in this *battle*, the second wins, But the *war* is not over: any research-programme is allowed a few such defeats. All it needs for a comeback is to produce an $n+1$-th content-increasing version and a verification of some of its novel content.

If such a comeback, after sustained effort, is not forthcoming, the war is lost and the experiment proved, *with hindsight*, 'crucial'. But the resistance may last for a long time, for the defeated programme may hold out with ingenious content-increasing innovations unrewarded with empirical success. It is very difficult to defeat a research-programme supported by talented, imaginative

[78] These hitches inevitably induce most individual scientists to shelve or altogether jettison the programme and join other research-programmes where the positive heuristic offers cheaper successes; but only naïve falsificationists regard the programme *objectively* eliminated.

scientists. Alternatively, stubborn protagonists of the defeated programme may offer *ad hoc* explanations of the experiments or a shrewd *ad hoc* 'reduction' of the victorious programme to the defeated one. But such efforts we should reject as unscientific.[79]

This position explains why crucial experiments are seen to be crucial only decades later, as in the case of Kepler's ellipses which were admitted as crucial evidence for Newton and against Descartes only about 100 years after Newton's claim. Young claimed that his double-slit experiment in 1802 was a crucial experiment between the corpuscular and the wave programmes of optics; but his claim was only acknowledged much later, after Fresnel carried on the wave programme much further progressively and the Newtonians could not match it. Frequently an anomaly, which has been known for decades if not for centuries, gets its title of crucial experiment after a long period of development of rival programmes. Examples abound: the Michelson-Morley experiment was seen to defeat Maxwell only after a long stretch of degeneration in Maxwell's and a long stretch of progress in Einstein's programme[80]. Michelson's refutation of the Balmer series was ignored for a generation until Bohr's triumphant research-programme backed it up[81]. Brownian motion was there for nearly a century in the middle of the battlefield before it was *seen* to defeat the phenomenological research-programme and turn the war in favour of the atomists.

(*d*) *A note on 'metaphysical research-programmes'*.

An idea of a research-programme which is akin to my concept of 'scientific research-programme' was put forward by Popper,

[79] For an example cp *below*, p. 180, footnote 83.

[80] Polanyi tells us with gusto how, in 1925, in his presidential address to the American Physical Society, Miller announced that, Michelson's and Morley's reports notwithstanding, he had 'overwhelming evidence' for an ether-drift; yet the audience remained committed to Einstein's theory. Polanyi draws the conclusion that no '"objectivist" framework' can account for the scientist's acceptance or rejection of theories (Polanyi [1958], pp. 12–17) But my reconstruction makes the tenacity of the Einsteinian research programme in the face of alleged contrary evidence a completely *rational* phenomenon and thereby undermines Polanyi's 'post-critical'-mystical message.
Incidentally, Polanyi anticipated Kuhn on some important points.

[81] For a discussion of the mutual support between 'theories' and 'facts' from the point of view of 'inductive logic' cp. the chapter 'Theoretical support for predictions versus evidential support for theories', in my [1968].

Agassi and Watkins in the 1950s, But none of them exploded naïve falsificationism and therefore they associated 'tenacity' with *syntactical irrefutability*, that is, in their terminology, with 'metaphysical' statements like 'all-some' statements and purely existential statements. If a theory, like Newton's theory of gravitation, seemed—in their misconceived mono-theoretical model—*syntactically refutable*, that is 'empirical', they could not accept it as *methodologically irrefutable*, that is, 'non-empirical' or 'metaphysical'. Therefore they conceived the hard core of research-programmes as necessarily 'metaphysical' in its 'logical form'. Agassi concentrated his attention on vague 'metaphysical frameworks' forming an influential 'background' to scientific theories. These frameworks, he stressed, provide the main guide for directing the scientists' attention to certain problems rather than to others; he summarised his ideas in his excellent [1964]. Watkins was more interested in the methodological role of syntactically irrefutable statements which occur *within* a syntactically refutable theory; his [1958] is a crystal-clear exposition of his thesis. Popper himself stressed the heuristic importance of 'influential metaphysics' already in his [1934], and was regarded by some members of the Vienna Circle as a champion of dangerous metaphysics.[82] When his interest in the rôle of metaphysics was rekindled by the dialectic of his own development and also by the stimulus of Watkins and Agassi, he wrote a most interesting 'Metaphysical Epilogue' to his *Postscript: After Twenty Years*—in galleys since 1957. But the writings of Popper, Watkins and Agassi on this subject all contain a certain *conflation of syntactical and methodological irrefutability*. Watkins elaborated beautifully the role of the metaphysical parts of a scientific theory, but it does not seem to have occurred to him that a scientific theory may have a metaphysical part which, although *syntactically* refutable, is *methodologically* irrefutable, and thus it may provide a core as hard as some syntactically irrefutable statements. The reason for this

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[82] Carnap and Hempel were trying, in their reviews of the book, to defend Popper against this charge (cp. Carnap [1935] and Hempel [1937]). Hempel wrote: '[Popper] stresses strongly certain features of his approach which are common with the approach of somewhat metaphysically oriented thinkers. It is to be hoped that this valuable work will not be misinterpreted as if it meant to allow for a new, perhaps even logically defensible, metaphysics.'

oversight, I think, was primarily due to two specific weaknesses in Popperian analysis: (1) the conflation of 'theory' and 'research-programme' (I have shown that the application of 'scientificness' or 'empiricalness' to theories, instead of to 'research-programmes', was a category mistake) and (2) the relegation of background theories into the limbo of 'universally accepted experimental techniques' and their exclusion from the critical deductive model of the theory under test.

In my approach, incidentally, one can easily solve the problem of appraisal of (syntactically) metaphysical theories—and the problem of their retention and elimination. It follows from my approach that it is rational to retain a syntactically metaphysical theory as the 'hard core' of a programme as long as its associated positive heuristic produces a progressive problem-shift. Let us take, for instance, Cartesian metaphysics *C*: 'in *all* natural processes *there is* a clockwork mechanism regulated by (*a priori*) animating principles'. This, in Popper's analysis, would be syntactically irrefutable: it clashes with no 'basic statement' expressible in terms of this principle. But it may clash with a refutable theory like *N*: 'gravitation is a force equal to

$$f\,\frac{m_1 m_2}{r^2}$$

which acts at a distance'. But the clash only occurs if we interpret 'action at a distance' in an 'essentialist' fashion. Such interpretation would exclude an interpretation of 'action at a distance' in terms of push-forces with the help of some ether-model or, in particular, some vortex-model. But we may interpret 'action at a distance' in a nominalistic way and then why not reinterpret it in terms of Cartesian metaphysics? Newton himself and several French physicists of the eighteenth century did exactly this. If an auxiliary theory which performs the reinterpretation produces novel facts (that is, it is 'independently testable') the metaphysics should be regarded as good, scientific, empirical metaphysics, generating a progressive problem-shift. A progressive metaphysical theory produces a sustained progressive shift in its protective belt of auxiliary theories. If the reduction of the theory to the metaphysical framework does not produce new empirical content, let alone novel facts, then the reduction represents a degenerating problem-shift—it is a mere linguistic exercise. The Cartesian efforts to bolster up their metaphysics to interpret Newtonian gravitation in its terms, is an outstanding example for such merely linguistic reduction.[83]

Thus we do not eliminate a metaphysical theory—as Wisdom suggests—if it clashes with a well-corroborated scientific theory. This would be a generalisation of naïve falsificationism. We eliminate it if it produces a degenerating shift in the long run and there is a better, rival, metaphysics to replace it.[84] The methodology of a research-programme with a 'metaphysical' core does not differ from the methodology of one with a 'refutable' core except for the logical level of the inconsistencies which are the driving force of the programme.

But the choice of logical form in articulating theories depends to a large extent on our methodological decision. For instance, we may formulate Cartesian metaphysics as 'all objects are clockworks'. A basic statement contradicting this would be: '*a* is an object and it is not a clockwork'. The question is whether according to the 'experimental techniques', or rather, to the interpretative theories of the day, '*x* is not a clockwork' can be 'established' or not. Thus the rational choice of the logical form of theories depends on the state of our knowledge; for instance, a metaphysical 'all-some' statement, of today may become, with the change in the level of observational theories a scientific 'all statement' tomorrow. I already argued that only series of theories and not theories should be demarcated into scientific and non-

[83] This phenomenon was described in a beautiful paper by Whewell ([1856]); but he could not explain it methodologically. Instead of recognising the victory of the *progressive* Newtonian programme over the *degenerating* Cartesian programme, he thought this was the victory of proven truth over falsity. For details cp. my [1969]: for a general discussion of the demarcation between progressive and degenerating reduction cp. Popper [1968b]).

[84] The best rational reconstruction of Newton's famous '*hypotheses non fingo*' is probably; 'I reject degenerating problem-shifts which are devised to preserve some theory whether it be syntactically metaphysical or otherwise'.

scientific; now I argue that even the logical form of a theory can only be rationally chosen on the basis of a critical appraisal of the state of the research programme in which it is embedded.

§4. *Conclusion: the Popperian versus the Kuhnian research programme.*

Let us now return to the Kuhn-Popper controversy.

We have shown that Kuhn is right in objecting to $Popper_1$'s naïve falsificationism, but he does not appreciate the strength of $Popper_2$'s position. Kuhn objects not only to $Popper_1$'s—or $Popper_2$'s—theory of the growth of science: he objects to the entire Popperian research-programme, he excludes *any* possibility of a rational reconstruction of the scientific enterprise. In a succinct comparison of Hume, Carnap and Popper, Watkins points out that the growth of science is inductive and irrational according to Hume, inductive and rational according to Carnap, non-inductive and rational according to Popper.[85] But Watkins' comparison can be extended by adding that it is non-inductive and irrational according to Kuhn. There can be no logic, but only psychology of discovery.[86] For instance, in Kuhn's conception, anomalies, inconsistencies always abound in science; science is in an eternal mess. There is no particular rational cause for a 'crisis' which leads to the overthrow of a 'paradigm'. Kuhn's 'crisis' is a psychological concept; it is a contagious panic. But then scientific revolution is irrational, a matter for mob psychology.

The reduction of philosophy of science to psychology of science did not start with Kuhn. An earlier wave of 'psychologism' followed the breakdown of justificationism. For many, justificationism represented the only possible form of rationality: the end of justificationism meant the end of rationality: The collapse of the thesis that scientific theories are provable, that the progress of science is cumulative, made justificationists panic. If 'to discover is to prove', but nothing is provable, then there can be

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[85] Watkins [1968], p. 281

[86] Kuhn [1969].

no discoveries, only discovery-claims. Thus disappointed justificationists—exjustificationists—thought that the elaboration of rational standards was a hopeless enterprise: all that one can do is to study—and imitate—the Scientific Mind, as it is exemplified in famous scientists. After the collapse of Newtonian physics, Popper elaborated new, non-justificationist critical standards. Now some of those who had already learned of the collapse of justificationist rationality now learned, mostly by hearsay, of Popper_1's colourful falsificationist slogans. Finding them untenable, they identified the collapse of Popper_1's rationality with the end of rationality itself. The elaboration of rational standards was again regarded as a hopeless enterprise; the best one can do is to study, they thought once again, the Scientific Mind. Critical philosophy was to be replaced by—as Polanyi called it—'post-critical' philosophy. But the Kuhnian research-programme contains a new feature: we have to study not the mind of the individual scientist but the mind of the Scientific Community. Individual psychology is now replaced by social psychology; imitation of the great scientist by submission to the collective wisdom of the community.

But Kuhn overlooked Popper_2 and the research programme he initiated. Popper_2 replaced the central problem of classical rationality, *the old problem of foundations*, with the *new problem of growth*, and started to elaborate objective and critical standards of growth. In this paper I have tried to develop this programme a step further. I think this small development is sufficient to escape Kuhn's strictures.[87]

The reconstruction of scientific progress as proliferation of rival research-programmes and progressive and degenerative problem-shifts gives a picture of the scientific enterprise which is in many ways different from the picture provided by its reconstruction as a succession of bold theories and their dramatic overthrows. Its main aspects were developed from Popper_2's ideas and, in

[87] Indeed, my concept of a 'research-programme' may be construed as an objective, 'third-world' reconstruction of Kuhn's concept of 'paradigm': thus the Kuhnian 'Gestalt-switch' can be performed without removing one's Popperian spectacles.

particular, from his ban on 'conventionalist', that is, content-decreasing, stratagems. The main difference, from Popper's original versions is, I think, that in my conception criticism does not—and must not—kill as fast as Popper imagined. *Purely negative, destructive criticism, like 'refutation' or demonstration of an inconsistency does not eliminate a programme.* Criticism of a programme is a long and often frustrating process and one must treat budding programmes leniently.[88] One can, of course, *undermine* a research-programme but only with dogged patience. It is usually only *constructive criticism* which, with the help of rival research programmes, can achieve major successes; but even so, dramatic, spectacular results become visible only with hindsight and rational reconstructoin.

Kuhn certainly showed that psychology of science can reveal important—and indeed sad—truths. But psychology of science is not autonomous; for the—rationally reconstructed—growth of science takes place essentially in the world of ideas, in Plato's and Popper's 'third world'. *Popper's research programme* aims at a description of this third world scientific growth.[89] *Kuhn's research programme* seems to aim at a description of *change* in the ('normal') scientific mind.[90] But the mirror-image of the third world in the mind of the individual—even in the mind of the 'normal'—

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[88] I am afraid that the reluctance of economists to accept Popper's methodology was primarily due to the destructive effect of naïve falsificationism on a budding research-programme.

[89] The modern *loci classici* on this subject are Popper [1968c] and Popper [1968d]; also cp. Toulmin's impressive programme set out in his [1967]. It should be mentioned here that many passages of Popper [1934] and even of [1963] read as descriptions of the second-world (that is, psychological) contrast between the Critical Mind and the Inductivist Mind. But Popper's psychologistic terms can be, to a large extent, reinterpreted in third-world terms.

[90] *Actual* state of minds, beliefs, etc. belong to the second world; states of the *normal* mind belong to a limbo between the second and third. The study of actual scientific minds belongs to *psychology*; the study of the 'normal' (or 'healthy' etc.) mind belongs to a *psychologistic philosophy of science*. There are *two kinds of psychologistic philosophies of science*. According to one kind there can be no philosophy of science: there can be only a psychology of individual scientists. According to the other kind there is a psychology of the 'scientific', 'ideal', or 'normal' mind; this turns philosophy of science into a psychology of this ideal mind and, in addition, offers a psychotherapy for turning one's mind into an ideal one. I discuss this second kind of psychologism in my [1969]. Kuhn does not seem to have noticed this distinction.

scientist is usually a caricature of the original; and to describe this caricature without relating it to the third-world original might well result in a caricature.

This paper is an extract from a much longer manuscript of the same title which I read in spring 1967 in London, Los Angeles, Berkeley, Toronto and at Brandeis, and which was at that time circulated among my friends. The extract is poorer in content but richer in polemic than the original; so much that it is almost like a solution of an exercise in complex function theory: 'construct a conform mapping which turns your friends into enemies and *vice versa.*' I am afraid the paper may have succeeded in solving only the first part of the exercise.

A penultimate draft was helpfully criticised by Colin Howson, Alan Musgrave, Helena Thonemann, John Watkins and John Worrall.

Feyerabend and Kuhn will comment on this paper in Lakatos-Musgrave (*eds.*)*: Criticism and the Growth of Knowledge*, Cambridge University Press, 1969.

BIBLIOGRAPHY

Agassi [1964]: 'Scientific problems and their roots in metaphysics', in Bunge (*ed.*)*: The Critical Approach to Science and Philosophy*, 1964, pp. 189–211.

Agassi [1966]: 'Sensationalism', *Mind*, N. S. Vol. 75, 1966, pp. 1–24.

Ayer [1936]: *Language, Truth and Logic*, 1936; second edition 1946.

Carnap [1935]: Review of Popper's [1934], *Erkenntnis*, Vol. 5, 1935 pp. 290–4.

Crookes [1886]: Presidential address to the Chemistry Section of the British Association, *Report of British Association*, 1886, pp. 558–76.

Crookes [1888]: Report at the Annual General Meeting, *Journal of the Chemical Society*, Vol. 53, 1888, pp. 487–504.

Feyerabend [1962]: 'Explanation, reduction and empiricism' in Feigl-Maxwell (*eds*): *Scientific Explanation, Space and Time, Minnesota Studies the Philosophy of Science*, Vol. 3, 1962, pp. 28–97.

Feyerabend [1967]: 'Bemerkungen zur Geschichte und Systematik des Empirismus', in Weingartner (*ed*): *Grundfragen der Wissenschaften und ihre Wurzeln in der Metaphysik*, 1967.

Feyerabend [1969]: 'Problems of empiricism II' in Colodny (*ed*): *The Nature and Function of Scientific Theory*, 1969.

Hempel [1937]: Review of Popper's [1934], *Deutsche Literaturzeitung*, 1937, pp. 309–14.

Hempel [1952]: 'Some theses on empirical certainty', *The Review of Metaphysics*, Vol. 5, 1952, pp. 620–1.

Juhos [1966]: 'Uber die empirische Induktion, *Studium Generale*, Vol. 19, 1966, pp. 259–72.

Kuhn [1962]: *The Structure of Scientific Revolutions*, 1962.

Kuhn [1969]: 'Logic of discovery or psychology of research', in Lakatos-Musgrave (*eds*): *Criticism and the Growth of Knowledge*, 1969.

Lakatos [1963–4]: 'Proofs and refutations', *The British Journal for the Philosophy of Science*, Vol. 14, 1963–4, pp. 1–25, 120–39, 221–45, 296–342.

Lakatos [1968]: 'Changes in the problem of inductive logic', in Lakatos (*ed*) *The Problem of Inductive Logic*, 1968, pp. 315–417.

Lakatos [1969]: *The Changing Logic of Scientific Discovery*, 1969.

Laplace [1796]: *Exposition du Système du Monde*, 1796.

Marignac [1860]; 'Commentary on Stas' Researches on the mutual relations of atomic weights'; reprinted in *Prout's Hypothesis*, Alembic Club Reprints, No. 20, 1932, pp. 48–58.

Medawar [1967]: *The Art of the Soluble*, 1967.

Musgrave [1968]: 'On a demarcation dispute', in Lakatos-Musgrave: *Problems in the Philosophy of Science*, 1968, pp. 78–88.

Nagel [1968]: 'What is true and false in science: Medawar and the anatomy of research', *Encounter*, 1968, pp. 68–70.

Neurath [1935]: 'Pseudorationalismus der Falsifikation', *Erkenntnis*, Vol. 5, 1935, pp. 353-65.

Polanyi [1958]: *Personal Knowledge. Towards a Post-Critical Philosophy*, 1958.

Popper [1934]: *Logik der Forschung*, 1935, (English edition: Popper [1959])

Popper [1940]: 'What is dialectic?', *Mind*, N. S., Vol. 49, 1940, pp. 403–26; reprinted in Popper [1963], pp. 312–35.

Popper [1957]: 'The aim of science', *Ratio*, Vol, 1, 1957, pp. 24–35.

Popper [1958]: 'Philosophy and Physics', *Atti del XII Congresso Internationale Filosofia*, 1960, pp. 363–74.

Popper [1959]: *The Logic of Scientific Discovery*, 1959.

Popper [1963]: *Conjectures and Refutations*, 1963.

Popper [1968a]: 'Theories, experience and probabilistic intuitions', in Lakatos (*ed*): *The Problem of Inductive Logic*, 1968, pp. 285–303.

Popper [1968b]: 'A realist view of logic, physics and history', in Yourgrau (*ed*): *Logic, Physics and History*, 1968.

Popper [1968c]: 'Epistemology without a knowing subject' in *Proceedings of the Third International Congress for Logic, Methodology and Philosophy of Science*, Amsterdam 1968, pp. 333–73.

Popper [1968d]: 'On the theory of the objective mind', in *Proceedings of the XIV International Congress of Philosophy*, 1968.

Popper [1968e]: 'Remarks on the problems of demarcation and rationality', in Lakatos-Musgrave (*eds*): *Problems in the Philosophy of Science*, 1968.

Popper [1969]: 'Normal science and its dangers', in Lakatos-Musgrave (*eds*): *Criticism and the Growth of Knowledge*, 1969.

Prout [1815]: 'On the relation between the specific gravities of bodies in their gaseous state and the weights of their atoms'. *Annals of Philosophy*, Vol. 6, 1815, pp. 321–30; reprinted in *Prout's Hypothesis*, Alembic Club Reprints, No. 20, 1932.

Russell [1943]: 'Reply to critics', in Schilpp (*ed*): *The Philosophy of Bertrand Russell*, 1943.

Toulmin [1967]: 'The evolutionary development of natural science', *American Scientist*, Vol. 55, 1967, pp. 456–71.

Watkins [1958]: 'Influential and confirmable metaphysics', *Mind*, N. S., Vol. 67, 1958, pp. 344–65.

Watkins [1968]: 'Hume, Carnap and Popper', in Lakatos (*ed*): *The Problem of Inductive Logic*, 1968, pp. 271–82.

Whewell [1856]: 'On the transformation of hypothesis in the history of science', *Transactions of the Cambridge Philosophical Society*, 1856.

Wisdom [1963]: 'The refutability of "irrefutable" laws', *The British Journal for the Philosophy of Science*, Vol. 13, 1962–3, pp. 303–6.

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