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"KUHN AND LAKATOS REVISITED"

The Essential Tension. Selected Studies in Scientific Tradition and Change. By Thomas S. Kuhn. Chicago & London: The University of Chicago Press, 1977. Pp. xxiii+366. $18.50/£12.95.

Philosophical Papers, Volume i: **The Methodology of Scientific Research Programmes.** By Imre Lakatos. Edited by John Worrall and Gregory Currie. Cambridge: Cambridge University Press, 1978. Pp. viii+250. £9.00.

By a happy coincidence Imre Lakatos's philosophical papers, gathered into two volumes, have appeared nearly simultaneously with a collection of some of Thomas Kuhn's hitherto widely scattered writings on the history and philosophy of science. These volumes offer more than simply yet another confrontation between their authors' widely canvassed views, for they may very well introduce new modulations into the debate which will be of special interest to historians of science. The Kuhn and Lakatos on view here often bear little resemblance to their respective public images, and the differences may be consequential for one's understanding of the real points of contention between them.

Students and colleagues of Kuhn (and here I declare an interest) will be delighted to find the man himself, not some spectre projected from responses to *Structure of scientific revolutions* (hereafter *SSR*). He emerges as a historian of science possessed of daring synthetic imagination and great sensitivity to the problems of the field, both institutional and intellectual.

As for Lakatos, his essays, when read separately at different times in the past, always seemed to capture some of his well-known personal brilliance. Read at one go, the essays suggest a different picture (Lakatos₂, the real Lakatos?). The sheer repetitiveness of his message begins to betray a certain tendentiousness and a worrying historical naivety. The vaunted wit tends toward petulance; the finesse seems to degenerate into argumentative slight of hand; his noble philosophical quest for rationality veers toward the mystifications of Popper's 'third world'.

The essential tension consists of an important autobiographical preface followed by fourteen papers, all but two of which have appeared previously, roughly divided into 'Historiographic' and 'Metahistorical studies'. I intend to concentrate upon the latter, which span the years 1957–73, for they disclose much about the growth of Kuhn's central theses not revealed in the text of *SSR*. They also highlight and help explain some of the peculiarities of that work; and, moreover, they bespeak a working heuristic for the study of the dynamics of scientific practice, more plausible and more flexible than that in *SSR*, and more open to development in dialogue with the sources. The six historiographical papers are clearly meant to be propaedeutic to the metahistory, but the three most properly historical of these papers actually achieve little in this regard. The early 'Concepts of cause in the development of physics' bears the stamp of Kuhn's initial attempts at the sympathetic and non-whiggish 'unpacking' of outmoded systems of physical thought. It adds little to one's understanding of Kuhn's metahistory, better pointed up elsewhere. 'Energy conservation as an example of simultaneous discovery' is a famous and ambitious piece whose manifest and hidden significances are best discussed after an analysis of the metahistory. The third study and the most recent paper included here, 'Mathematical versus experimental traditions in the development of physical science', is a bold attempt to give new shape to the history of physics from the Greeks to the advent of classical physics in the mid-nineteenth century. Because of its wide scope, and because it raises issues well outside the normal terms of Kuhn's philosophy of science, any critical discussion of this paper would exhaust this review, and so I leave it to the careful scrutiny of its intended audience.[1]

The three remaining historiographical papers—'The relations between the history and the philosophy of science' (hitherto unpublished), 'The relations between history and the history of science', and 'The history of science'—are all loosely concerned with the status and role of history of science as a discipline, and its relation to the fields of science, history, and philosophy. They contain fascinating and rather homespun reflections upon Kuhn's experience as a teacher and scholar and they should be read by all historians of science, who are probably similarly besieged. Nowhere is the gap between the real and the mythical Kuhn more apparent. He literally agonizes over the nature (and future) of the history of science, and he shows that his very least ambition is to establish the literal text of *SSR* as a party line for the writing of history. These papers also suggest that Kuhn's main project is not to build a methodology, a theory of appraisal, or an account of rationality; that he is first and foremost a historian interested in constructing usable and corrigible heuristic interpretative schemas for the thinking and writing of the history of science. All this, I think, is crucial for understanding Kuhn's metahistory.

The metahistorical studies open with three classic papers written prior to *SSR*: 'The historical structure of scientific discovery' (hereafter HSSD), 'The function of measurement in modern physical science' (hereafter FMMPS), and 'The essential tension: tradition and innovation in scientific research'. The first of these was finished in late 1961, well after the other two. Kuhn states, however, that the core ideas it contains pre-date the composition of the paper on 'Energy conservation' in 1957. Presumably this is why it opens the metahistory. Indeed, a close reading suggests that it was more seminal than even Kuhn suspects.

What interested Kuhn was the notion that significant scientific discoveries are not simple 'events' in which a new fact or law is slotted into the rising edifice of scientific knowledge. Significant discoveries arise from complex historical processes and they alter the structure of knowledge through which they are produced, rather than simply adding to it. First, a striking difficulty must stand out against a well-developed framework of theory, theory-guided technique, and expectation. 'Anomalies', Kuhn was already writing, 'do not emerge from the normal course of scientific research until both instruments and concepts have developed sufficiently to make their emergence likely and to make the anomaly which results recognizable as a violation of expectation' (pp. 173–4). There follows a struggle, theoretical and experimental, to render the anomaly law-like and to fit it into the accepted categories of explanation. This work issues in what we might term a 'feed-back' effect upon the web of techniques and concepts against which the anomaly arose (pp. 175 ff.). The effect can be part of a large 'upheaval' of established 'theory and practice', such as the discovery of oxygen in the chemical revolution; or it can be subtle, like the supposed effect of the discovery of Uranus upon the *expectation* that further similar patterns of anomaly would henceforth best be explained by postulation of additional planets, rather than by invoking some other explanation.

As M. D. King observed some time ago, this paper issued a revolutionary challenge to positivist philosophy of science and to orthodox Mertonian sociology of science.[2] It threatened the former by questioning the idea of the simple unilinear and cumulative development of scientific knowledge. It more implicitly threatened the latter by hinting that the proper object of study in the sociology of science is not the cluster of overriding Mertonian values supposedly constitutive of all science, but rather the institutional, social, and political processes by which explanatory frames are produced, maintained, and altered by significant discovery. Kuhn, I would suggest, never fully realized the radical potential of HSSD. This may seem paradoxical in the light of Kuhn's subsequent career and the notoriety of *SSR*; yet I think it was precisely Kuhn's subsequent work on revolutionary theory change which foreclosed the richest epistemological and sociological implications of HSSD.

What is most notable in HSSD is the way Kuhn initially focused upon

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a concept of 'significant discovery' which cut across his later division between normal and revolutionary science.[3] The fluidity and scope of the concept of 'significant discovery' meant that Kuhn effectively stood at a turning point at which there were two main alternatives for the future development of his ideas.

One might have explored in more detail, as Kuhn did not, the notion that the feed-back of significant discovery can be exerted on any one or more of the elements analytically discernible within a living research tradition; e.g. the central concepts and their systematization, the (theory-bound) techniques and procedures of research, the standards of relevance of different sorts of problems, the norms of adequacy of proffered solutions, the number and weighting of the aims and goals of research. The metaphor of normal science as 'puzzle solving' could then have been replaced by the view that even relatively normal research involves frequent significant alteration of the rules of play—if it is 'puzzle solving', the pieces, the rules, and the ultimate 'picture' are all continually open to non-trivial revision.[4] Moreover, one might have come to see that scientific actors are always in what could be termed a hermeneutical situation; they always have to make interpretations and informed judgements about how the elements in the framework of practice are to be revised in the light of some work itself judged to be 'significant'. This in turn could have large implications for the sociology and social history of science. Rather than supplying an understanding of the social forms within which can thrive some autonomous method productive of cumulative truth, sociological analysis would be essential for understanding the very creation, content, and dynamics of scientific knowledge. One would see that processes such as (1) recognizing and evaluating 'important' problems; (2) judging (and enforcing judgements of) the 'significance' of proffered solutions; and (3) negotiating the nature of the feed-back adjustments to be made, are all inescapably social processes, inextricably bound up with the particular cognitive, evaluative, and power political structures of the discipline. I suggest that if one passed through these sorts of stages one would then approach, by means of a historical short-cut, the very sorts of enriched, cognitively- and sociologically-sensitive heuristic pictures of mature science which one now finds in such sophisticated 'post-Kuhnian' historian-sociologists of science as Ravetz, Dolby, Mulkay and Edge, to name a few British examples.[5]

Kuhn's own thought followed the other main alternative. He assimilated all significant change to catastrophic revolutionary alteration of theory and practice, to change of 'world-views' in fact. Correlatively, the hitherto subordinate and unarticulated notion of tradition-bound practice and expectation was worked into the concept of normal science as 'paradigm'-guided puzzle solving. These moves from HSSD created Kuhnian philosophy of science and the terms of the future debate over progress, rationality, and revolution. They simultaneously tended to obscure the possibilities of the first alternative line of development, though at times hints of them appeared between the lines.[6]

The autobiographical remarks in the preface help explain why Kuhn followed this path. Trained as a theoretical physicist, his initiation into the history of science came when, as a member of the Society of Fellows at Harvard, he was asked to give some lectures on the history of physics.[7] Slumbering in an untutored whiggism, Kuhn could not make sense of Aristotle's often apparently nonsensical and downright erroneous statements. Then, enlightenment: Kuhn grasped intuitively what he would soon see proposed systematically in the writings of Koyré and Metzger—outmoded philosophical, scientific, and cosmological systems each have their own *sui generis* inner rationality, cogency, and coherence, which the historian must penetrate. Kuhn, starting to work seriously in the history of science, presumably gathered that if sciences are such hermetically sealed thought-worlds, then significant change in science must be more problematic than usually thought. HSSD probably reflects such early concerns. But I would guess that Kuhn's burgeoning fascination with this basically Collingwoodian metaphysics, soon buttressed by excursions into B. L. Whorf, N. R. Hanson, and Gestalt psychology, carried him beyond HSSD and deformed the concept of 'significant discovery' into that of revolutionary collapse of scientific world-views. Once he was moving this way, his crucial achievement was not the notion of revolutionary change, for that was largely derivative, the cause rather than the consequence of his outgrowing HSSD. Instead it was the invention of 'normal science'. This conception was necessary to differentiate science from the other sorts of enterprises—philosophy, ideology, metaphysics—which could also be treated in Collingwoodian terms. Science, unlike these, made progress, at least in normal periods, and its revolutions were if anything less irrational, since they grew out of normal science and resolved themselves around new solutions to anomalies focused by the previous normal dispensation.

The reader can trace the vestiges of most of this in FMMPS, where normal science takes shape, and in 'The essential tension' (the essay of 1959), where the 'paradigm' began its career. Though Kuhn states that the relations between the metahistorical essays and *SSR* are obvious (p. xvi), there are points here which are likely to be missed by readers of *SSR* even now. It is first of all absolutely crucial to see that the conception of normal science was forged entirely within the context of thinking about highly elaborated mathematico-experimental sciences. Here the research puzzles are solely quantitative puzzles and they mainly involve attempts to improve the precise degree of 'fit' between mathematicized theory and quantified experiment. The basic dynamic attributed to normal science in bringing on crisis and revolution depends upon its quantitative character, for the main and most reliable signal of an anomaly is failure to solve a problem to the currently agreed standard of precision. For the Kuhn of FMMPS anomalies are matters of decimal places; they are not matters of wild or fantastic effects. Moreover, normal science is progressive mainly in extending the range of the phenomena it can command precisely, and in improving the degree of that precision. Finally, the value placed on increasing precision may often be enough to justify a radical shift of conceptual framework. In short, nothing about the original version of normal science is untouched by considerations of precision and quantification. This raises the question of why Kuhn and his critics and supporters alike tend to eschew the general problem of whether the concept of normal science is transferable to qualitative and classificatory forms of research? One does tend to ask whether a given science 'fits Kuhn's model', but this usually depends upon having already drained the terminology of normal science of its original quantitatively biased connotations.

In a similar fashion the genesis of the 'paradigm' can still be informative about *SSR*. In the preface to *The essential tension* Kuhn explains how, having seen in FMMPS that normal science is about consensus, that it is a 'mopping-up operation that consolidates the ground made available by the most recent theoretical breakthrough', he then needed to specify of what the consensus consists. After he failed to find the shared explicit definitions of concepts and correspondence rules preached by philosophers of science, he struck upon shared exemplary problem solutions, which teach by example the meaning and function of concepts, and which serve as models for further 'mopping-up' researches. This was the original sense of 'paradigm'. Soon afterwards it began a cancerous growth. Every element or object about which there is consensus in normal science became a paradigm or part of one: textbooks, problem sets, metaphysical commitments, models, symbolically expressed law-sketches, and so on. But Kuhn is perhaps not in a position to point out that his concentration on shared elements enfeebled the claimed sociological character of the concept of normal science. Since normal research had been made rather trivial and mechanical, sociology of science would consist in identifying the agreed elements facilitating practice and those who hold them. But if normal science is relatively dynamic and is qualitatively altered by the significant results it generates, then sociology must also address the problems of how a shifting consensus is produced, maintained, and frequently re-negotiated.

Two papers published after *SSR*, 'Logic of discovery or psychology of research' and 'Second thoughts on paradigms', find Kuhn defending himself against the critics. The first essay derived from the 1965 Bedford College symposium on 'Criticism and the growth of knowledge'.[8] In it he assails Popperian falsificationism as a framework for understanding the lived dynamics of scientific research. His message is that because Popper does not deploy the normal/revolutionary science distinction his philosophy falls into a number of systematic interpretative errors.[9] The argument works rather well because Kuhn can play his strong suit, a general grasp of the tradition-bound character of research in mature scientific fields. When, however, he has to defend and clarify normal science and paradigms, he is less convincing, as 'Second thoughts on paradigms' demonstrates.

The latter paper aims to elucidate the overgrown concept of a paradigm and to develop its original meaning as an exemplary discipline-founding (or -altering) technical achievement. On the first issue all Kuhn really accomplishes is the unpacking of some of the elements originally conflated in the concept. The exemplary technical achievement reappears as an 'exemplar' and it, along with some of the other elements—metaphysics, models, textbooks, symbolic representations—are dubbed the 'disciplinary matrix', an ordered array of discipline-specific elements. This may well help readers of *SSR*, but it does nothing to lift Kuhn out of the confines of his limited post-HSSD sociological problematic. He is still reshuffling the same pack of cards.

The attempt to explain 'exemplars' and why they make a difference to the course of scientific practice is marginally more successful, but not I fear fully convincing even to sympathetic readers. In Kuhn's example of knowledge gained through 'exemplars', 'little Johnny' acquires a rudimentary conceptual schema, categories of geese, ducks, and swans, by a process of ostension of instances, and without the mediation of explicit definitions, lists of criteria, or correspondence rules. So far, so good, perhaps. Kuhn then argues that little Johnny, so 'programmed' will react differently to anomalous and borderline instances from someone who deployed explicit definitions and criteria of classification. Furthermore, in many cases Johnny will create fewer conceptual difficulties for himself in understanding the world. But what can we make of this for the case of actual science? There, on Kuhn's own showing, possession of exemplars does not exhaust the disciplinary matrix, so there is no such stark contrast between purely exemplar-mediated knowledge and definition- and correspondence-rule-mediated knowledge. The judging and interpreting normal scientist is hardly just juggling unarticulated exemplars.

Only in the hitherto unpublished 'Objectivity, value judgment and theory choice' (1973) does Kuhn step beyond *SSR* to develop his position in a significant new way.[10] He endeavours to show that Kuhnian theory change is not an irrational process and that though there is no decision algorithm there is a good deal of rational argument and persuasion structured in terms of certain 'values' shared within the community. The values in question are not the Mertonian ones of 'universalism', 'communism', 'disinterestedness' and 'organized scepticism'. Surprisingly, they are the sorts of things philosophers have often thought of as objectively identifiable criteria of the 'goodness' of a theory, e.g. 'accuracy', 'consistency' (both intra- and inter-theoretical), 'scope', 'simplicity', and 'fruitfulness'. Kuhn's potentially vastly important claim is that 'the simple', 'the consistent', 'the precise', and 'the fruitful' are not simply given or perceived in a situation. Scientists are committed to them as ideals, and must judge and interpret whether and to what degree a given state of affairs instantiates or promises them. Typically, different scientific communities will differ in the selection and relative weighing and interpretation of these values, and even within a normal scientific tradition scientists (or groups thereof) may hold slightly but consequentially differing constellations, interpretations, and weighings of them. No meta-scientific logic or method can dictate that a given selection will lead to greater progress.[11] Values, their choice, weighing and interpretation do alter over time (in historically explicable ways) but generally they change more slowly than do conceptual structures.

These contentions (and I have elaborated them a bit beyond Kuhn's text for the purposes of argument) amount to a radical claim for the thoroughgoing historicity of scientific practice: scientists cannot avoid making judgements which depend upon the skilled interpretation of the current state of play in the light of values the number, nature and weighing of which are themselves historically evolved, socially maintained and yet in principle always re-negotiable. Scientific knowledge is inextricable from this tissue of judgements and interpretations; in fact they may be identified with it. Lakatos, one could infer, must be in gross error to presume that scientific rationality could or should consist in a non-interpretative, almost algorithmic, decision procedure. But neither does all this render scientific rationality somehow subjective or irrational. One could indeed say that it makes scientific rationality a typically human form of rationality, that is, a species of value-conditioned practical judgement. At this point the implications of the paper link up with that long-lost first alternative issuing from HSSD. For if Kuhn were to elaborate these themes, he would, I believe, wind up with conclusions of the following sort: revolutions cannot consist in Gestalt shifts and in conversions without persuasion (and judgement); and normal science cannot be trivial puzzle-solving, but rather is a tissue of judgements and decisions about the matters (1) to (3) mentioned above on p. 304. But Kuhn, it must be said, shows no inclination further to exploit these themes.[12]

Like 'Objectivity, value judgment and theory choice', the one remaining metahistorical study, 'A function for thought experiments', addresses a much wider realm of issues than Kuhn cares explicitly to acknowledge, and in so doing it threatens some of the grand themes of *SSR*. Kuhn argues persuasively that thought experiments do not merely reveal inconsistencies in previously held conceptual schemes. Instead they must be understood to concern the relation of concepts to nature; they 'can alter one's knowledge of the world'. In this respect they are, as Kuhn points out, very similar to that class of significant real experiments which occasion considerable conceptual transformations. The remarkable point is that the kinds of transformation discussed, in a Galilean thought experiment and in examples of Piaget's real experiments with children, are not catastrophic Gestalt shifts, but rather conceptual 'reforms' or 'revisions'.[13] Kuhn, in other words, has unwittingly veered close to the pre-*SSR* notion of 'significant discovery'. No chasm of an ineffable Gestalt shift—'now a rabbit now a wolf'—separates the earlier and later concepts; the direction and extent of conceptual alteration is intelligible on the basis of the earlier state. This essay is virtually the only place in the Kuhnian corpus where the inner semantic processes involved in a concept change are discussed, and they hardly seem to fit the more extreme formulations in *SSR*. Kuhn, however, virtually admits that he missed the significance of the ideas in this paper when he writes that it had 'little influence' on *SSR* (p. xx). That, we may conclude, is much to be regretted, and it has significantly conditioned the post-*SSR* debates.

Finally we can return to look briefly at the paper on 'Energy conservation as an example of simultaneous discovery'.[14] Despite the title it is not about the simultaneous discovery of the first law of thermodynamics. It is actually an inquiry into the surprising clustering between 1830 and 1850 of about a dozen partially contrasting and partially overlapping conceptual and experimental initiatives from the creative use of some of which thermodynamics soon was to take shape.[15] Kuhn states that it has been included mainly to illustrate how external influences, in this case *Naturphilosophie* and an emergent science of heat engines, can have a role in the process of scientific development. So leaving issues of detail and accuracy aside, one might be disappointed to find that his explanations are unexceptional in form. Nor are they developed in any sophisticated way from the side of social and economic history. All this suggests that what Kuhn really thinks important here is the study of the relation of external factors to the formation of 'exemplars'; that is, the study of how in the creation of 'exemplars' broad and qualitative discourses partially motivate and inform the product, and how in turn they are deformed and altered.[16] This is apparent in Kuhn's interest in how a Mayer or Colding, influenced by *Naturphilosophie*, broke through and out of that discourse by quantifying his speculations in a narrow and well defined experimental-technical context, hence conjuring science or proto-science out of a qualitatively different species of discourse.

'Energy conservation . . .', like 'A function for thought experiments', issues a promissory note about how to theorize about the micro-mechanics of the formation of exemplary concepts in experimental contexts. They are as close as Kuhn has ever come to the detailed consideration of science- or paradigm-creation. Nothing should be higher on the agenda of post-Kuhnian metahistory than to clarify these issues by 'tacking' between cases studies and theorizing.

It seems to me that *The essential tension* unintentionally reveals two different but related sets of themes in Kuhn's work. The major set comprises the grand and well debated theses of *SSR*: normal/revolutionary science, disciplinary matrices, Gestalt shifts, conversion without real choice. The minor set consists of more disparate heuristic and metahistorical insights, sometimes implicit and sometimes explicit in these essays, but always sitting uneasily with the major themes and yet sometimes confused with them. Minor themes include (1) the doctrine of 'significant discovery' and complex feed-back, which cuts across the normal/revolutionary science dichotomy; (2) the view of theory choice, and of normal science, which stresses the role of the actor as a skilled interpreter and negotiator; (3) a sketched theory of conceptual alterations which avoids recourse to the metaphor of dramatic, ineffable Gestalt switches;

and (4) a whisper of a thesis that a fully historicized paradigm concept demands an account of the social processes of production, maintenance, negotiation and enforcement of the (continually alterable) elements of the disciplinary matrix. The metahistorical studies show how Kuhn at various times stumbled upon these themes and turned away from them toward the major themes of *SSR*. Those like Lakatos who have tried to construct alternative theories of scientific change have in large measure been responding to the major themes. Hence the ensuing debates have been framed in terms of those elements of Kuhn's thought which are, on this view, less plausible and less fruitful than the suppressed minor themes. To judge by the first volume of Lakatos's papers, the consequences of this can be disastrous for both history and philosophy of science.

The volume contains five papers on the 'methodology of scientific research programmes' (hereafter MSRP), the main product of the last ten years of Lakatos's life.[17] MSRP represents a sustained attempt to develop on a post-Popperian basis a theory of appraisal of scientific theories capable of showing in what the rationality of theory change and the progress of science consist. I propose to use my reading of *The essential tension* as the basis for a neo-Kuhnian assessment of MSRP, to ask whether it offers a plausible or even possible framework for understanding how research is carried on, and how scientists exercise recognizably human forms of rationality. This reverses the usual critical procedure of assessing Kuhn as if he were primarily a 'methodologist' and theorist of appraisal criteria. I shall limit myself to discussing material in 'Falsification and the methodology of scientific research programmes' and 'History of science and its rational reconstructions' which occupy over half the volume, and I shall end with some remarks on 'Why did Copernicus' research programme supercede Ptolemy's?' (written with Elie Zahar).

According to Lakatos a 'research programme' consists of (1) the 'hard core' of basic explanatory laws and metaphysical commitments; (2) a negative heuristic injunction not to direct potential falsifiers at (1) but rather at (3) a protective belt of auxiliary hypotheses constructed in the light of a 'positive heuristic', which 'defines problems, . . . foresees anomalies and turns them victoriously into examples, all according to a preconceived plan' (p. 111). Research consists in the elaboration of a linear series of these hypotheses (or 'theories' as Lakatos terms them). The programme is evaluated as a whole in terms of the tendency of this series to evince progress or degeneration. A given theory T_{i+1} is 'theoretically progressive' if it accounts for the unrefuted content of T_i and also predicts some novel, unexpected, striking fact. If that fact is corroborated, T_{i+1} is 'empirically progressive' as well. A research programme is 'progressing' if the series $T_1 \ldots T_n$ is consistently 'theoretically progressive', and intermittently 'empirically progressive'. It is stagnating if it can give only *post hoc* explanations of chance discoveries or discoveries predicted in a rival programme. In a world of rational actors, consistently progressive programmes should replace stagnating ones, which are eliminated or 'shelved'.

Let us now look at this through neo-Kuhnian spectacles, by trying to imagine what a Lakatosian research programme looks like to an actor within it. First consider the hard core and positive heuristic upon whose stability the possibility of assessment rests. An armchair philosopher can at his leisure announce *ex post facto* what the hard core and positive heuristic were, and then issue *pronunciamenti* about the progress or degeneration of the programme. Actors are not so lucky, as Lakatos must concede: hard cores are often slow to mature; one man's hard core is another's positive heuristic; sometimes the two are conceded not to have existed for historical actors in the sense retrospectively required.[18] Lakatos seems to think these small problems; in fact, they have massive implications. They hint at the embarrassing fact that whatever elements function as hard core and positive heuristic are more or less open to debate, revision, and alteration by the relevant community in the course of research. Lakatos is reconstructing the situational rationality of the actor in terms which for the actor do not exist. Fluidity in the hard core and positive heuristic and debate about them are symptoms of what actual scientific practice is about, as Kuhnian historiography would want to reconstruct it.[19] They also begin to indicate that Lakatos' historiography is not so much inadequate as not historiography at all.

The force of such points is magnified when one recalls that real research is not typically unilinear in the sense defined. Research in a field or tradition proceeds along a number of frequently ramifying fronts, growing from local paradigmatic achievements produced in the course of research. Hence the state of the field (and the preferred directions of research) can look different from various points on the research frontier. Debate about preferred directions and about the significance of research accomplishments will effectively be debate about what, for the moment, should function locally as 'the hard core' and 'the positive heuristic'. This multiplies the problems about the fluidity of the terms and their being the objects of judgement and negotiation. Actors are less plausibly Lakatosian figures than Kuhnian ones, moving about in a situation very like that envisioned in Kuhn's minor themes.

What, then, does a Lakatosian actor spend his time doing? Lakatos knows that scientists do many more things than do his rationally reconstructed ones; but for the MSRP it is only important that the actor engage in the following tasks:

(1) He constructs hard cores and positive heuristics. If this is taken to mean what he must do to initiate a programme, then it cannot be treated in MSRP; yet it is in principle so tractable in Kuhn, Bachelard, or in a host of historians one might read. If it means the ongoing debate about, and alteration of, hard cores and positive heuristics, then that is something Lakatos cannot afford to consider in MSRP, as we have seen.

(2) He constructs his T_is, hoping to predict novel and striking facts.

(3) He tries to corroborate the novel facts in (2).

(4) He initiates 'appeal procedures' against observations which fail to corroborate his predictions. The appeal challenges the validity or progressiveness of the research programme in which appears the theory which guides the observational procedure. Adjudication takes the form of a competition between the latter programme and the programme of the theory whose prediction was not corroborated.

(5) He initiates radical shifts in the positive heuristic. (Cf. n. 19 above).

(6) He engages, mainly through (2) and (3), in a competition between his own and a second research programme. At some point he will have to act in the light of MSRP and declare in favour of the more progressive programme.

Lakatos's own criterion for evaluating historiographical projects roughly is 'how much of the history of science do they reconstruct as rational and progressive?' On this criterion Lakatos's view of scientific practice fails miserably, because it is inadequate and hermeneutically naive. It is inadequate in that it ignores modes of scientific behaviour which are arguably quite rational and can conduce to progress (in one sense of progress or another, including the over-rigid Lakatosian one). It is hermeneutically naive because the sorts of decisions it imputes to actors are vastly oversimplified, to the point of failing to engage at all the question of in what the situationally-constrained rationality of scientists can consist, and hence in what the 'internal' history of science consists.

First, then, it is very plausible that actors seeking theoretical improvements, even without making novel predictions, are acting rationally in a way which conduces to progress. One might be interested in the internal consistency of the theory and/or its relation to any number of (and sectors within some) cognate fields. This may introduce differences in the direction and nature of problem solving, especially when there already exist ramifying paths of research. Lakatos, of course, did not think much of solving problems;[20] one must for some reason solve an existing problem *and* predict a novel fact in the process. This sits oddly with the widely-shared insight that in something like Kuhnian normal science real progress is made, and significant feed-back to theory often occurs, simply through trying both theoretically and experimentally to narrow the gap between prediction and observation in some well-trodden area. Lakatos also consistently refused to see that some solved problems or solved anomalies (and he was willing to identify the two) are the very loci in which radically new modes of theory and practice first take shape. This is related to Lakatos's otherwise inexplicable tendency to view 'crises' as disembodied psychic panics which float free of the realm of research.[21] Kuhn's point is that failure to solve problems judged to be 'significant' attracts both concern and interest, and real work. Correspondingly a convincing and apparently fruitful solution will alleviate concern, further focus interest and paradigmatically facilitate further real work.

The charge of hermeneutical naivety is, I think, even more damning. Consider first just one sort of decision made in normal practice, the judgement of the theoretical progressiveness of a T_i, that is, the judgement that a predicted fact is novel or striking. Simply issuing any new T will call into existence a host of consequences, i.e. predictions. Which ones are striking and novel? Ironically Lakatosian philosophy has seen an example of the very sort of debate and redefinition which this must always entail. When Lakatos was having trouble rationalizing the Copernican revolution, he decided to accept Zahar's 'reinterpretation' of 'novel' fact to include low-level generalizations long known to scientists but which played no role in the formulation of the research programme which now can predict them. All this is a dim reflection of the 'fact' that in real research honest men could well differ in their assessments of Lakatosian novelty, assuming they had any reason to indulge in such decisions. Judgements of novelty would presumably differ widely with the weighing and selection of (Kuhnian) values held, all the more so in a ramified field.[22]

The Great Decisions of Lakatosianism, decisions about the progress and degeneration of research programmes, are also in trouble. From the criteria of progressiveness and stagnation it would seem that there follow heuristic rules for how to proceed; when to give up or to stick with a programme. Lakatos is perceptive enough to see that some discretion and 'common sense' are necessary. So we read rules such as: 'allow the older programme a few failures'; 'allow the fledgling programme a period of only *post hoc* achievement'; and wise counsel such as: 'remember that a stagnating programme can stage a "come back" at any moment'.[23] Kuhn and Feyerabend have claimed that the advice is ambiguous at best, and at worst inapplicable. In fact, the fundamental difficulty stems from the curious belief that research programmes can be simply, objectively, and unequivocally judged to be 'progressive' or 'degenerating' because 'public' and agreed-upon 'scores' can be produced reflecting the current success of each programme.[24] This is a profound mystification; no such scores are ever likely to be agreed inter-programmatically, or even intra-programmatically.

Presumably, accountants imported from the 'third world' or LSE would tot up all predictions and corroborations of dramatic and novel facts. For a new research programme to supersede an old one, it would have to match the latter, prediction for prediction, *and* have additional marks in the prediction and corroboration columns. We have already seen that even within a given (ramified) research programme different groups of actors are very likely to make varying assessments of 'novelty', for they have divergent preferred research interests and exemplars. Is it conceivable that members of competing programmes could agree on the scores? Surely they would be acting perfectly rationally in making their own situationally value-laden assessments of what it is important to pursue? Far from being an argument for subjective idiosyncracy, this is a recognition that research as an institution creates and is sustained by the play of judgements.[25] The differential recruitment of scientists to a new paradigm is only rational if it occurs in the light of divergent evaluations; and, as Kuhn insists in *The essential tension*, such divergent evaluations are the way in which the community collectively hedges its bets on novelty and tradition.

Even if novelty were an observation predicate rather than a socially mediated meaning, the scores would still be impossible to formulate. What, for example, counts as a novel fact? If one invents the periodic table, has one predicted inert gases as a class (1 fact), or n inert gases (n facts, including Zaharian facts if any), or $n+1$? Do we use our common sense to decide; that is, do we engage in the processes of negotiation and judgement constitutive of our community? Is a quantitatively improved prediction a new fact, or is it one of those thousands of trivial predictions which lurk in the wings of a theory?[26] To begin to answer, we would need a nuanced understanding of the total context, the state of play. Finally, Lakatos conveniently ignored a crucial and very plausible implication of Kuhn's theory, that competing paradigms often do not credit the existence of certain of each other's problems, predictions, and corroborations. For example, in the short run at least (for well over a century, in fact) post-Lavoisierian chemistry could not recognize or pursue questions about the family resemblance of metals posed and solved by phlogiston theory. This was not primarily a matter of differing Gestalts and incommensurable world-views; rather one of differing yet mutually understandable preferred research interests, exemplars, and capabilities.[27]

In short, even if actors wanted to behave like Lakatosians (and that would hardly produce the sort of activity we now term science) they would still be involved in inescapably more complex contexts of evaluation and decision than Lakatos suggests. As it stands MSRP does not rationally reconstruct history. It can more properly be said to issue mystifying rationalizations of whatever we happen to like (mainly 'the present').

Lakatos would not have agreed. All such criticism is rooted in lack of comprehension of the third world, that transcendent realm in which repose such unchanging true things as mathematical objects and relations, theories, problems, and, yes, MSRP. It is dubious that even numbers exist in a third world;[28] why it should contain Professor Lakatos's method (or was it Professor Popper's?) is a complete mystery. Lakatos says that the power of MSRP to yield rational reconstructions is a strong argument in its favour, and he proceeds on several occasions to score points against some hermeneutical plodders called 'the conventionalist', 'the justificationist' and the (vulgar) 'falsificationist'.[29] Yet I doubt if there is a Lakatosian answer to the challenge posed by even the disparate minor themes of Kuhn.

The third world provided Lakatos' ultimate defence of his conception of what internal and external history of science should be. The former is to be the objective history of research programmes, their progressiveness and degeneracy, and consequent pattern of replacement and supercession. Actions and decisions in accordance with the objective relations of research programmes are correct and rational, and they result from relatively unclouded vision of the third world and a healthy psychological disposition to follow its lead. Actions and decisions not in accordance with the objective relations and scores of research programmes are non-rational, and are explained by the sociological or psychological distortion of awareness of the third world. One often hears it said that Lakatos's view of internal history is too narrow and too philosophical. That is not really the point. We do need to theorize about the internal practice of science, and we also need a theory of rationality. But what we do not need is history that cannot count as history because it glosses over the actually lived situational rationality and the decision-making of actors (or puts them in the footnotes and calls them commonsense), while attributing to actors unrealizable forms of judgement about non-existent objects.[30]

For historians of science, the *pièce de résistance* of volume i will be the paper written with Zahar on 'Why did Copernicus' research programme supercede Ptolemy's?' In it one can discern most of the difficulties alluded to above. The problem was to show how Kepler and Galileo in particular could have been rational in opting for the Copernican theory. In Lakatos's terms dramatic confirmation of Copernican predictions came only in 1616 (phases of Venus) or in 1838 (stellar parallax). Even the first event was too late to explain Kepler and Galileo's choices. So enter Zahar with his new interpretation of a 'novel fact'. On this basis Copernicanism was born holding some dramatic corroborations, for it predicted the already known 'low-level' generalizations that (i) the planets have stations and retrogressions; (ii) the periods of the superior planets, as seen from the earth are not constant; (iii) if the earth is taken as the origin of a fixed reference frame, each planet must be ascribed a complex motion, one component of which is the annual motion of the sun; and (iv) the elongation of the inferior planets from the sun is bounded and the calculated periods of the planets strictly increase with their calculated relative distances from the sun. Without much ado about the intellectual biographies or contexts of decision of Kepler and Galileo, Lakatos happily concludes that they had MSRP justification for being Copernicans.[31]

Lakatos is no doubt on the right track in seeking to characterize (i) to (iv) in a way which makes them objectively significant for Copernicans. He improves on Kuhn's self-defeating tendency to write about these and other related factors as mainly aesthetic harmonies to which future Copernicans were somehow attuned in a purely subjective, private way. The matters in (i) to (iv) are objective predictions, or if you like they are predicted and confirmed answers to certain kinds of cosmological questions. But there precisely is where MSRP begins to go awry. It is not the case, as Lakatos assumes, that (i) to (iv) were novel facts *simpliciter*. In the sixteenth century they were *cosmological* facts rather than facts within the problematic of work-a-day calculational astronomy interested in producing tables and almanacs. To

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have accepted them as significant for establishing the truth of Copernicanism one must already have been committed to a certain schema of relevance and weighing in which claimed facts of cosmological structure could bear heavily on the choice of an astronomical system (a set of geometrical devices) and vice versa. Moreover one would have to have been willing to accept that novel cosmological facts arise from geometrical arguments and relations, rather than from physical arguments in the established Aristotelian sense of the distinction between physics and mathematics. Anyone impressed by these facts would also have to have weighed as insignificant the physical and dynamical objections to heliocentrism. One's stand on the state of authoritativeness of Aristotelian physics was a crucial element in the situational weighing of 'reasons'. Lakatos ignores all this, reducing the context of decision to a mythical 'flatland' in which facts (or the facts he selects) all have the same relevance because there is no historical reconstruction of the actors' frames of relevance and judgements about the complex relations of astronomy, cosmology, and physics.

Lakatos has virtually nothing to say about Tycho Brahe, whose system, also brilliantly confirmed in 1616 by the same phases of Venus, was the dominant one in the period. Tycho had made a nuanced weighing of the reasons. He could not part company with basic Aristotelian physics (though he did so in parts of his system); yet he saw the astronomical and cosmological value of certain of Copernicus's harmonies of structure, and he built them into his system, thus creating subsidiary puzzles for Tychonians.[32] A Copernican, by contrast, had to have gone very far out on a natural philosophical limb, and to have been very dubious of any claims for Aristotelian cosmology and physics. Some were bolstered by a Platonism which valued arguments from mathematics to reality; or, like Beeckman and his protégé, the young Descartes, they were anti-Aristotelian proto-mechanists eager to replace a physical system they viewed as bankrupt. Considerations of natural philosophy conditioned decisions about astronomy. All this is well known to even the most modest undergraduate, and it arms him with a better interpretative schema than Lakatos deploys here.

Lakatos's entire enterprise in MSRP was so conditioned by the attempt to overcome what he took to be the irrationalism and relativism of Kuhn's major themes, that he produced an implausible historiographical heuristic which makes no contact with Kuhn's minor themes, nor with the state of the best practice in the field. Sensitive reflection on that practice would begin to reveal a metahistory and an account of rationality far superior to MSRP. Lakatos's virtue, it would seem, lies primarily in having shown by example where in the 1970s we should draw the line separating critical historiography from mystification.

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NOTES

[1] First published in *Annales: économies, sociétés, civilisations*, 1975, *30*, 975–98, it is intended for the edification of the general historian as much as for the historian of science. Kuhn hopes to shield the former from errors, for example identifying modern European science with the career of Newton and Newtonianism, or misconstruing the role of 'Baconian' experimentalism in the scientific revolution of the seventeenth century. Along the way Kuhn makes important and controversial claims about the deep structure of that revolution, about the meaning of the Merton thesis, and about the so-called 'second scientific revolution' of the later eighteenth and earlier nineteenth centuries.

[2] M. D. King, 'Reason, tradition and the progressiveness of science', *History and theory*, 1971, *10*, 3–32.

[3] To be sure the essay contains mention of a type of straightforward 'proto-normal' research, e.g. fitting new elements into the periodic table. This is contrasted not with grand revolutionary science per se; but, as we have seen, with any research which has a significant feed-back effect on practice, from the subliminal to the catastrophically revolutionary.

[4] A version of this sort of revision of Kuhn's 'normal science' can be found in the opening portions of J. R. Ravetz, *Scientific knowledge and its social problems*, Oxford, 1971.

[5] I do not mean to suggest that the views of any of these figures precisely mirror the picture presented here, only that there are significant 'family resemblances' amid real differences of interest and approach. Nor do I suggest that they reached their positions in the way outlined. For one thing they were all influenced by Kuhn's own evolution along the other path.

[6] This may help explain some of the ambivalences of those deeply influenced by Kuhn and yet unwilling to accept very many or even any of the doctrines of *SSR*. Kuhn's own historical writing often owes much to views related to the first alternative, and this may partly explain the 'lack of application' of *SSR* to his concrete studies.

[7] A detailed intellectual biographical sketch of Kuhn which stresses the shifting institutional contexts and opportunities of his earlier career has been written by R. K. Merton, and appears in R. K. Merton and J. Gaston (eds.), *The sociology of science in Europe*, London & Amsterdam, 1977, pp. 71–113.

[8] The proceedings of this symposium were later published as Imre Lakatos and Alan Musgrave (eds.), *Criticism and the growth of knowledge*, Cambridge, 1970.

[9] For example, the image of the scientist making bold conjectures and vigorously attempting to falsify them may fit aspects of revolutionary episodes, but misconceives the character of normal tradition-bound research. Conversely, the methodological slogan that the falsificationist 'learns by his mistakes' has plausible reference to the conditions of normal research but mistakes the character of revolutions. Normal science, moreover, rather than falsifiability, should serve to demarcate science from non-science. Astrology, for example, does not lack falsifiable predictions; yet it is no science, for it lacks a puzzle-producing and puzzle-solving paradigmatic core. It is more like traditional medicine—a not very effective practical art posturing under a pretentious theoretical cover which bore little relation to practice.

[10] *SSR* and 'Logic of discovery . . .' hinted at themes Kuhn develops here.

[11] Progress itself can be seen as a Kuhnian value, for there are differing senses of progress and differing interpretations of how to achieve them. On such varying ideals and interpretations of progress see T. Kulka, 'Some problems concerning rational reconstruction: comments on Elkana and Lakatos', *The British journal for the philosophy of science*, 1977, *28*, 325–33 (340–1).

[12] At the end of the paper Kuhn turns to the problem of the 'conversion' to a new theory by immersion in its conceptual language. He seems to hold that all the talk about value judgements merely helps to explain how initial persuasion is possible. Effective work in the new theory requires conversion in which one effectively chooses, though 'no process quite like choice has occurred'. Though Kuhn appears to think otherwise, I would suggest that for historians the most important matters to elucidate are value judgement and persuasion. We need a non-Lakatosian theory of (practical) rationality; the fact of subsequent full conversion via immersion can take care of itself.

[13] Piaget's young subjects split their original concept of 'faster' into 'something like the adult's notion of "faster" and a separate concept of "reaching-goal-first" ' (p. 245). Galileo's thought experiment teaches his reader not to conflate, as Aristotelians did, the concepts of instantaneous and average speed (p. 251; cf. the note on Carnot p. 259, n. 30).

[14] I shall not be discussing the final metahistorical study, 'Comment on the relations of science and art'.

[15] The initiatives included the formulation of special cases of energy conservation; a general but qualitative conservation principle; or, with differing exemplars, formulation, and quantification of a conservation principle.

[16] The 'paradigms' in question are, to repeat, not to be identified with the first law of thermodynamics as it appears in early systematizations of that science, e.g. those of Kelvin, Clausius, and Helmholtz, but rather they are the various 'initiatives' mentioned above.

[17] All but one have appeared previously, sometimes in several forms, as the editorial notes make clear. The previously unpublished paper, 'Newton's effect on scientific standards' is partly symptomatic of Lakatos's 'Popper-centric' view of the recent history of Western culture. In seeing Popper (and Einstein) as the improvers of standards of scientific rationality, triumphing over the mistakes of the Newtonian ideology of science, Lakatos actually manages to write history better than his usual standard. But he still conflates real standards of practice, grand methodological pronouncements, and vague scientistic ideology, while always giving the impression that he knows better than to do this. This paper, and 'Popper on demarcation and induction', may well be viewed by non-Lakatosians as curious scholastic pieces reflecting, respectively, the public ideology of the sect, and the content and quality of its private dialogue with Popper.

[18] See, for example, pp. 119, 48, 51.

19 Similarly, historical evidence forces Lakatos to recognize occasional very radical alterations in the prevailing 'preconceived plan' or positive heuristic. The legitimacy of such moves can only be established in MSRP by the progressiveness of future work in the programme. Now, from the actor's perspective, any bids radically to alter the positive heuristic must be acted upon at the moment, in the light of skilled interpretation and judgement of the current and likely future state of play, something different from simply making 'bold conjectures' and waiting for the verdict of reason in history. The history of science is punctuated with such moves; yet for Lakatos they are either insufficiently rational, hopelessly *ad hoc*, or, as noted, awaiting 'the long run'. Hence, Lakatos cannot theorize about a crucial type of history-making practical judgement exercised by actors.

For a nominally Lakatosian study which illustrates the fluidity of hard cores, see H. Frankel, 'The career of continental drift theory: an application of Imre Lakatos' analysis of scientific growth to the rise of continental drift', *Studies in history and philosophy of science*, 1979, *10*, 21–66. Ironically, precisely because of the granted fluidity of the hard core, this excellent paper reads better as a neo-Kuhnian than as a Lakatosian study.

20 P. 36.

21 Pp. 90–1, 135–6.

22 There is no need to believe that a consensus about novelty must emerge in the long run; that is simply a reflection of Lakatos's belief that the field is linear. And even if such consensus ever occurred, it would be just *that*: a consensus, a social process, explicable in terms of Kuhn's minor themes. There would not be widespread recognition of a cosmic truth that '*x*' *is* a novel fact.

23 Pp. 69–72. On the complex relations between criteria of progress and stagnation, and the heuristic rules, see P. Quinn, 'Methodological appraisal and heuristic advice', *Studies in history and philosophy of science*, 1972, *3*, 135–49.

24 'One may rationally stick to a degenerating programme until it is overtaken by a rival *and even after*. What one must not do is to deny its poor public record' (p. 117). 'The scores of the rival sides, however, must be recorded and publicly displayed at all times (p. 113; cf. Lakatos' n. 5 on the same page about Feyerabend's apparent denial that the scores can be recorded).

25 A. Chalmers, 'Towards an objectivist account of theory change', *The British journal for the philosophy of science*, 1979, *30*, 227–33, tries to salvage Lakatos by turning 'fruitfulness' into an objective (third-world) property of theories. See his natural analogy on p. 231: he considers two identical gardens, suitably populated by birds. One garden contains nesting boxes; the other does not. That many more birds will nest in the first is 'adequately explained' by the objective nesting opportunities. One need not refer to the decisions of the birds. But what if birds also waged wars, and wrote theology, or what if they had the option of practising birth control? Would we then need to study their decisions about the interpretation of 'fruitfulness' and the weighting it should be given in the aviary 'life world'?

26 'We are no longer interested in the thousands of trivial verifying instances nor in the hundreds of readily available anomalies: the few crucial *excess-verifying instances* are decisive' (p. 36).

27 The points in the last three sentences are established in an important and perceptive paper by G. Doppelt, 'Kuhn's epistemological relativism: an interpretation and defense', *Inquiry*, 1979, *21*, 33–86. The paper as a whole deserves an important place in any subsequent debate over Kuhn and his critics and competitors.

28 D. Bloor, 'Popper's mystification of objective knowledge', *Science studies*, 1974, *4*, 65–76. P. K. Feyerabend, 'Popper's *Objective knowledge*', *Inquiry*, 1975, *17*, 475–507.

29 Pp. 169–78, 10–31. Another Lakatosian straw man is sociology, though in this case it is not clear whether his lack of critical discrimination is a ploy or a confession. There is in any case no whisper of a suggestion that Mertonian and post-Kuhnian sociology of science might differ in consequential ways. *A fortiori* one could not expect Lakatos to consider phenomenological sociology in the Schutz-Luckmann tradition. It just might happen that this tradition will prove fruitful in facilitating the nuanced study and reconstruction of scientists' contexts of judgement and action, as it has in the study of the ordinary 'life world'. Its promise lies in its potential for showing the essentially and unavoidably historical and constructed character of knowledge, and its entanglement with socially created schemas of relevance and interest which can be studied from macro-sociological and biographical-psychological perspectives. Kuhn's views on value judgment pass near its gravitational field. And I would suggest that those historians producing the most sophisticated 'internal' history often have an intuitive grasp of some of its themes. Both fields would benefit from a dialogue on this relation.

30 See, for example, p. 117 n. 4, for one of these falsely-aware concessions to the real complexity of human rational judgement.

31 Lakatos then claims that when Kepler and Galileo noted the stagnation of Copernicus's programme in dealing with calculational details, they each boldly opted for a new positive heuristic, loosely taken as the project of the new dynamics, celestial and terrestial respectively.

In MSRP no attention is given, nor need be given, to where this bold departure came from, or why or how in more detailed terms it was worked out by the protagonists. Future progress retrospectively justifies the bold gambit, and that is all we can say as critical historians.

32 See R. S. Westman, 'Three responses to the Copernican theory: Johannes Praetorius, Tycho Brahe, and Michael Maestlin' in R. S. Westman (ed.), *The Copernican achievement*, London, 1975, 285–345, (305–29). Note also Westman, 'The Melanchthon circle, Rheticus and the Wittenberg interpretation of the Copernican theory', *Isis*, 1975, *66*, 165–93.

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