

*Abstract*

Theories of Science have been a significant source of inspiration for scholars concerned with educational theorizing. The current article deals with the rationalist influence on educational thought, in particular the appeal of Imre Lakatos to educational theorists of the last few years. It is argued that rationalist philosophy of science transcends political barriers, so that the paradox of marrying an idealist anti-Marxist philosophy of science, to an anti-idealist Marxist world view, is suppressed.

Anthony R. Welch\*

### **The Allure of Rationalism in Educational Thought: The Case of Imre Lakatos**

Although Karl Popper is one of the more prominent prophets of rationalism of this century, he is by no means the only one, nor necessarily the most influential, in the field of educational research and philosophy.<sup>1</sup> A second major figure of twentieth century rationalism has proved to be just as alluring, in the fields of philosophy of science and philosophy of education. His work exhibits much the same underlying assumptions, and may be subject to similar critiques.

Imre Lakatos' methodological arguments have proved very persuasive, and in this current essay on connections between rationalist theories of science and theories of education we are forced to widen significantly our concept of rationalist. "Warriors of objective truth"<sup>2</sup> are not always Cold War warriors at the same time, it appears, and in the case of Lakatos his appeal extends even to Marxist scholars in the philosophy of science, who have been sufficiently influenced to attempt a marriage of Lakatos and Marx.<sup>3</sup> In certain parts of the world this paradoxical philosophical blend has attained semi-paradigmatic status in particular fields of inquiry.<sup>4</sup> Indeed one could be forgiven for arguing that Althusser is alive and well and living in the Antipodes. Or at least that the project to unite a scientific Marxism with an equally positivistic philosophy of science, drawn from Lakatos, has proved particularly persuasive in select circles of Marxist philosophers of science, and especially philosophers of education of the last decade. This essay seeks to analyze and explain this paradoxical phenomenon.

Imre Lakatos, initially an enthusiastic Popperian, succeeded Popper in the philosophy of science<sup>5</sup> at the London School of Economics. One of Lakatos' major attempts, the force of which he felt was only partly understood and accepted by Popper, was to reformulate the concept, and use, of falsification. Rejecting dogmatic falsification (which firstly accepts only empirical evidence as arbiter for a theory, thereby implicitly accepting an absolutely firm bedrock of empirical facts which can be used to falsify theories; secondly uses falsifiability as a strict line of demarcation between science and metaphysics; thirdly accepts no equivocation in respect of a disproved theory; and lastly distinguishes sharply between theoretician on the one hand, and experimenter, who tests theories against stubborn and unyielding nature ('facts'), on the other), he characterizes it in the following manner, citing Braithwaite:

"man proposes a system of hypotheses: Nature disposes of its truth or falsity. Man invents a scientific system, and then discovers whether or not it accords with observed fact."<sup>6</sup>

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\*University of New England, Armidale, Australia.

The problem with this approach lies in its narrowness. It firstly assumes that there is a clear and natural demarcation line between theoretical propositions, and factual or observational statements. (This proposition is part of the naturalistic approach to scientific method which Popper critically outlined in his *Logik der Forschung* of 1934.) The second assumption is that if a proposition satisfies the criterion of being empirical, it is then true, proved from facts. Empiricism is further reflected in its demarcation criterion, where factual content distinguishes scientific from non-scientific theories. Only theories which specify an observable state of affairs, which can thus be falsified, are scientific. Scientific equals empirical. Clearly we are able to demonstrate the falsity of these claims. Firstly we must deny the phenomenal claim: there can be no pure, atheoretical, observation. What we see can only be explained and understood by reference to a theory of optics; unmediated sensory observation is untenable, and no natural demarcation between observational and theoretical statements exists. To the second claim, we must reply that facts cannot ever be proved (all knowledge is tentative and conjectural) nor can propositions be derived from facts (only from other propositions). Further, if facts (or factual propositions) are fallible then these cannot prove theories, or, just as importantly, cannot disprove them either:

“The demarcation between the soft unproven, ‘theories’ and the hard, proven ‘empirical basis’ is non-existent: all propositions of science are theoretical and incurably, fallible.”<sup>7</sup>

Lastly, theories, even the most venerated, are often unable to be disconfirmed. Respectable theories may not specify any observable state, the absence of which would be accepted as falsification of the theory. Auxiliary theories, functioning as *ceteris paribus* clauses can simply be adjusted or replaced, to cope with anomalies or contradictions; an anomaly, then, does not necessarily disprove the basic statement.

“And, as it happens, it is exactly the most important, ‘mature’ theories in the history of science which are *prima facie* undisprovable in this way.”<sup>8</sup>

(Similarly ‘all swans are white’ cannot be disproved by the discovery of a black swan: a *ceteris paribus* clause may explain this apparent anomaly). Clearly, this is inadequate as a method of demarcation of science from un-science. It leads us, if accepted, to the view that “scientific theories are not only equally unprovable, and equally improbable, but.. are also equally undisprovable.”<sup>9</sup>

Methodological falsification, a revolutionary form of conventionalism, is also ultimately naive. This view procedurally accepts certain spatio-temporally singular ‘basic’ statements, (or ‘observation statements’, or ‘test statements’), and only accepts a theory as being scientific if it can be made to conflict with such a basic statement (i.e. it is falsifiable). At the same time the theory must predict ‘novel’ facts for it to be acceptable. If, in the light of these conditions, (by which both unfalsifiable, and *ad hoc* hypotheses are illegitimate) the theory under consideration clashes with the accepted basic statement, then it ought to be rejected. According to this view the major external stimulus of scientific theories are myths:

“A critical attitude needs for its raw material . . . theories or beliefs which are held more or less dogmatically. Thus science must begin with myths, and the criticism of myths . . . and of magical techniques and practices.”<sup>10</sup>

The difference between the methodological falsificationist and the dogmatic is in the former’s separation of rejection (or falsification) and disproof. The dogmatic falsificationist must reject or eliminate a falsified theory; the methodological falsificationist recognizes it may still be true despite having been falsified. This distinction then leads to a new demarcation principle, accepting theories which are ‘observational’ (a set of coordinates may be derived from an existing theory of gravitation

but may not be able to be tested by any direct optical observation, only by experimental technique using existing theories of radio-optics uncritically, as 'touchstone theories' or 'unproblematic background theories') as well as directly observational.<sup>11</sup> Many more theories can thus be made 'falsifiable'; even probabilistic theories, given the specification of rules or rejection for certain kinds of (statistically interpreted) evidence. But in order to falsify theories unable to explain 'observable' phenomena without reference to a *ceteris paribus* clause, we must recognize that if "no finite number of 'observations' is enough to 'falsify' such a theory"<sup>12</sup> then we need take a further step in order to be able to discriminate between competing theories, or rigorously test an existing theory. If a test of the combined theory and *ceteris paribus* clause elicits a negative result, we must decide whether that anomaly should necessitate refutation of the specific theory. If the initial conditions withstand severe<sup>13</sup> testing, they may then be relegated temporarily to unproblematic background knowledge. If the *ceteris paribus* clause also withstands rigorous testing<sup>14</sup> it too may be held to be unproblematic, in which case it may act as a negation of the theory with which it was associated, "a potential falsifier of the theory itself".<sup>15</sup> It is, of course, possible that even this much may not make a theory empirically falsifiable, in which case it would be rational to reject such a theory if it clashed directly with another theory already judged to be scientific by the aforementioned criteria, and well corroborated, even unproblematic.

But perhaps the precepts and procedures of methodological falsificationism are too daring, and even arbitrary; in any event how can one explain the actual process of theoretical development and replacement, such as the long lapse of time (eighty-five years) between the *discovery* of the anomalous perihelion of Mercury, and its eventual *acceptance* as falsification of Newton's theory. Ultimately both dogmatic and methodological falsification contain, according to Lakatos, two elements at variance with the actual history of science: 1) Tests are, or ought to be, contests between a theory and experiment only. 2) The only useful results of such a conflict is (final) falsification: science proceeds through negation and elimination. The historical development of science demonstrates, in opposition to these two elements, that tests are, at the least, triangular contests between rival theories and crucial experiment, and secondly that some of the more important experiments tend, *prima facie*, towards confirmation rather than falsification. There are, as a result, two courses open to us: to abandon this form of rational explanation<sup>16</sup> or to attempt its resurrection in modified form.

In adopting the latter, basically Popperian, course Lakatos outlines a 'sophisticated' form of falsification which contains different demarcation criteria from its more 'naive' form. To the former, a theory is only acceptable if it contains excess corroborated empirical content over its rival/predecessor. Both the excess content, and the corroboration of that content are important elements of its acceptability. For the former also, falsification can only be said to occur (for example of a theory  $T_1$ ) when a competing theory ( $T_2$ ) has greater empirical content, or predictive potential for discovering novel facts (those not anticipated or proscribed by  $T_1$ ); when the new theory explains all the former successes of  $T_1$  (its unrefuted content); and when some of the novel content of the newer theory is corroborated.

But acknowledging, as we now must, that no experimental result can ever vanquish a theory (it can always be rescued via an auxiliary hypothesis)<sup>17</sup> the problem becomes one of discrimination between the rational and thus progressive adjustments of this kind, and the irrational and degenerating variety, which Popper terms ad-hoc hypotheses.

"But then any scientific theory has to be appraised together with its auxiliary hypotheses, initial conditions etc. and, especially, together with its predecessors so that we may see by what sort of change it was brought

about. Then, of course, what we appraise is a series of theories rather than isolated theories.’<sup>18</sup>

Thus falsification must be seen in the context of the appraisal of a developmental programme of theories; it therefore acquires a ‘historical character’. Single theories are unable to be judged scientific or unscientific<sup>19</sup> and we may only judge a series to be theoretically progressive if each new theory has excess empirical content over that which it succeeds (if it predicts unexpected facts) and if some of this new empirical content is corroborated (if it leads to the actual discovery of some new fact). If a problem shift fails to conform to these methodological mandates (if it solves the anomaly or contradiction by recourse to semantics), it must be seen as unscientific. The empirical character of a theory and its growth are hereby united. Falsification, we now see, can only be seen to occur with the elaboration of a more successful rival theoretical candidate — ‘There is no falsification before the emergence of a better theory’<sup>20</sup> — and is therefore comprehensible not in the light of a dialogue between theory and the empirical base, but a dialectical interplay between competing theories, the original empirical base, and empirical developments which occur as a result of competition and interplay. Counter evidence, from this perspective, can only exist within the framework of a progressive, superseding explanatory theory. And we cannot know what counts as *crucial* counter-evidence, except with hindsight, that is when a superseding explanatory mode has emerged: *Exemplum docet, exempla obscurant*. Indeed refutation, according to this view, is not indispensable for scientific development to occur. New theories may emerge in such rapid succession that experimental support of one theory ( $T_2$ ) may only later be seen to refute another ( $T_1$ ). We must always struggle to replace existing theories by better, fuller ones; we cannot wait for refutation of existing theories. Sophisticated falsification is then, experiential or empirical, activist, and conventionalist. Above all, excess corroborated content is the key to the superiority of one theory over another. Einstein’s theory was not superior to Newton’s because the former was unrefuted (indeed both were refuted)<sup>21</sup> but because it explained more than Newton’s theory (it explained some of the anomalies in that theory) and predicted certain empirical results which were proscribed by other theories: further, some of this excess content was subsequently given experimental support. In the case of mutually inconsistent theories one or other, or both, may be rejected by a process of attempting to replace one, then another, leading gradually to a new form with the greatest possible increase in corroborated content, and prediction of novel facts susceptible to experimental test. This solution, then, ‘provides the most progressive problem shift’.<sup>22</sup> Nevertheless, the decision about which basic statements to accept is residual, and although experience is still in some sense a final arbiter, the central question of conventionalism (i.e. where do we draw the line dividing problematic from non-problematic knowledge?) remains with us.

The appraisal of the growth of knowledge must be seen, however, in terms of the continuity of what Lakatos terms scientific research programmes, and the methodological rules for their evaluation. Such programmes each have their negative heuristic (research paths to avoid) and their positive heuristic (paths to pursue or explore). Each also has a ‘hard core’,<sup>23</sup> which is deemed by proponents of the theory to be the unassailable kernel of the programme, and a ‘protective belt’ of auxiliary — hypotheses, initial conditions, ‘observational’ hypotheses, all of which may be modified, or replaced, given anomalous results.<sup>24</sup> For the research programme to be developmental rather than degenerating, each step must increase content; and at times this increased content must be supported by empirical test, even though in the interim several apparent refutations may well have necessitated dogmatic defence of the programme. On the other hand, if the programme ceases to predict novel facts, its hard core may have to be rejected. The positive heuristic, decided by proponents of the programme, sets out a long term research policy which often anticipates certain

refutations, and may suggest means of reformulation of the protective belt:

“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 for predicting (producing) and digesting them.”<sup>25</sup>

The negative heuristic, again as a matter of decision, articulates the incorrigible hard-core.

A new, ‘budding’ theory ought to be temporarily sheltered from its more institutionalized rivals, while the newcomer still has progressive potential. At some point however it must exhibit its heuristic power by discovering new facts; if not, that tolerance will be withdrawn.<sup>26</sup> But how then may we discriminate successfully between rival theories? Within a research programme, a minor crucial experiment is often enough to decide between different versions: the successful candidate will not only contradict its rival, but more importantly will have greater corroborated empirical content. If, however, the observational theories form part of a larger matrix, the rival research programmes themselves clash and the need is for a ‘major crucial experiment’. One contender may be defeated in battle, but several such battles must be decided before the spoils are awarded to the victor, or the scientists agree to bestow ‘the honorific title of refutation’ upon a long-existent anomaly.<sup>27</sup> Unfortunately, according to Lakatos, “we need hindsight to evaluate experiments”.<sup>28</sup> Indeed we may say that there are no such things as crucial experiments (which immediately refute a research programme), only experiments which over the course of time are elevated to this status, and are henceforth so considered in historical treatments or studies.

Science, then, according to Lakatos must exhibit continuous growth, in anticipating novel facts and auxiliary hypotheses. Dogmatism on the part of scientists, however, must still be seen as rationally defensible despite Popper’s insistence on the ‘critical attitude’. In sum, Lakatos’ work served to further refine Popper’s concept of falsification, or at least by rejecting some of Popper’s simpler versions, to insist on a more sophisticated understanding, consistent with Popper’s early *Logik der Forschung*.<sup>29</sup> If Popper is accepted as having provided an Occam’s Razor in the field of philosophy of scientific discovery, then Lakatos’ contribution may be seen as further tuition in applying it deftly and with care, lest we excise too much. As Feyerabend puts it in a critical appreciation, Lakatos

“has provided a method for judging not just theories, but also standards, and he has shown in detail how the method can be employed. He has considerably improved the critical inventory of critical rationalism.

Despite having further refined the rationalist research programme, Lakatos enthusiastically embraced its basic ideologies, and was subjected to similar critiques to those of his mentor, Popper. As with Popper, many of these critiques relate to Lakatos’ devout scientism: the belief that science is a metaphor for rationality, and that since science is the most perfect form of rationality, all fields of inquiry can, indeed ought to, be scientized or rationalized.<sup>31</sup> It is for these and allied beliefs that Lakatos, as Popper, is considered by critics to be within the historical traditions of positivism.<sup>32</sup>

The major criticisms levelled at rationalist theories of science, and at Lakatos in particular, are explicable by reference to developments in the philosophy of science of the past twenty years or so. Lakatos, of course, was an important protagonist in the debate on theories of science which culminated in the seminal book *Criticism and the Growth of Knowledge*,<sup>33</sup> but almost a decade prior to that publication, the safe and secure world of philosophers of science had begun to be turned “upside down”.<sup>34</sup> That discomfiting process of revolution has increased greatly in strength and vigour since the early nineteen-sixties.

Perhaps one of the most disconcerting developments was Kuhn's influential demonstration that scientific communities worked according to particular, puzzle-solving paradigms.<sup>35</sup> This being the case, comforting versions of a correspondence theory of truth have now often been replaced with a consensus theory.<sup>36</sup> The thesis of the incommensurability of paradigms, although held to different degrees by Kuhn and Feyerabend, has been said to demonstrate the impossibility of couching statements in the language of preceding or alternate paradigms.<sup>37</sup> On the contrary, learning a theory is, as Wittgenstein argued, like learning a language, and translation of key terms which gain their meaning from their embeddedness in a particular theoretical frame, cannot be easily or unambiguously performed.<sup>38</sup> According to Feyerabend, and to the early Kuhn, "certain statements (about Greek gods, about phlogiston) were once true, but have now ceased to be so".<sup>39</sup>

If natural scientific theories are paradigmatic in structure, then it may well be that natural scientific communities are much more like those in the social sciences than Lakatos and Popper would have us believe. In which case the much-vaunted rationalist argument for unity of method may be more true than may have been suspected, but for all the wrong reasons. A degree of empathic hermeneutics<sup>40</sup> may be a feature of entering into any language-game, whether in the social or natural sciences. It may well be characteristic of the process of inquiry itself, in defiance of objectivist claims about the possibility of logically demonstrating the superiority of one theory over another.

But a commitment to logic is itself expressive of a value position, a point which Feyerabend in the natural sciences, and Habermas in the social realms have used to highlight aspects of the ideology of scientism.<sup>41</sup> In the case of rationalism, this commitment to logic has at least two effects, both of which have been attacked by scholars in the social and natural sciences. Firstly the commitment to an unrestrained logic, by Lakatos and others, has led to attempts to sanitize the history of science, by performing rational reconstructions, which are implausible without the added factor of hindsight.<sup>42</sup> Such reconstructions abstract theoretical developments from any real history, whether of prevailing ideologies, social constraints, life situations or lack of knowledge at the time. What emerges from rationalist obsessions with abstract canons of logic is the conventional 'great man' histories of science offered to apprentice scientists, and roundly condemned by Barnes, Kuhn, and Feyerabend, *inter alia*, as no more than idealist Whig histories.<sup>43</sup>

The second consequence which arises from the irrational adoption and proselytizing of scientific methodology by Lakatos, and other rationalists, is the covert pursuit of a technocratic value system embracing technical efficiency and economy, while downgrading other, potentially more humanitarian, values.<sup>44</sup> The employment of an unrestrained technologic which is clearly implied in the programme of Lakatos, and Popper, is highly intolerant of alternative values, or traditions which attempt to control science by subjecting it to traditional evaluative criteria such a capacity to contribute to individual enlightenment or emancipation.<sup>45</sup> And although *l'esprit scientifique* is itself expressive of a value position, it is conventionally defended on the basis of being value free; a position which has been exposed by Marcuse,<sup>46</sup> Adorno,<sup>47</sup> Feyerabend<sup>48</sup> and Habermas:

"... the concept of rationality which the critique of ideology seeks to make prevail in its commitment ultimately implies an entire organization of society: one in which a technology become autonomous, dictates a value system — namely its own — to the domain of praxis it has usurped — and all in the name of value-freedom."<sup>49</sup>

We are left overall with a more concrete, historical, social and political conception of science, as a net result of developments in the philosophy of science of the last two decades. Even the hallowed

objectivism of Popper and Lakatos has been attacked as idealist,<sup>50</sup> while in physics the following has now become reasonable as a result of modern studies of particles:

“. . . our epistemic activities may have a decisive influence even upon the most solid piece of cosmological furniture — they may make gods disappear and replace them by heaps of atoms in empty space.”<sup>51</sup>

Our understanding of science and the way in which scientific change and progress occurs has changed fundamentally over the last twenty years, and we are now far less likely to see practitioners and theoreticians of science as philosopher-kings in the way scientific rationalists have implied. The central and residual questions raised by the Popper/Lakatos approach, however, revolve around the concept of rationality, and its place in epistemological development. Is intellectual progress fully comprehensible within the restrictive rationalist tradition to which Popper and Lakatos claim allegiance? Or, is “the world of a theory . . . a social world, . . . built up by *scientists who have to decide* what to keep and what to eliminate”;<sup>52</sup> can its contents be entirely divorced from their material originators, and dispassionately considered in Popper’s World Three?<sup>53</sup> To put the question in another form, can rationality be considered entirely as a set of methodological procedures, as apart from *human* sensibilities, decisions and ethics. It is not so much, as Lakatos claims, whether Kuhn “excludes any possibility of a rational construction of the growth of science”,<sup>54</sup> but whether one’s concept of rationality must, necessarily, comprehend these judgements and choices. It has been convincingly argued that even the fuller and more consistent articulation of the rationalist position, as developed by Lakatos, remains stubbornly “social-psychological in its repeated reliance on decisions governed not by logical rules, but by the mature sensibility of the trained scientist”.<sup>55</sup> Recourse to transcendentalism is unacceptable: Kant . . . (and Popper and Lakatos) assume “that the human mind does not change”.<sup>56</sup>

### *On Education*

As we have seen, Popper is by no means the only representative of the rationalist tradition in philosophy of science. In education, those who interest themselves in philosophy of science, and to some extent those who wish to borrow the mantle of legitimacy and prestige which the methods of the hard sciences are held to bestow upon its followers, have flirted with Lakatos’ methodology also. Here again however, when we look at what is perhaps the major attempt to use Lakatos as a basis for understanding education, we find that it is written by a Marxist, Michael Matthews, an occurrence of which neither Popper nor Lakatos would have approved.<sup>57</sup> But given a more critical view of science, as outlined in the natural sciences by Feyerabend, and in social inquiry by Habermas, there is much strategic sense in such a synthesis. If the object of the exercise is to defend the objectivity of educational judgements, then Lakatos and Althusser, although curious bedfellows from one point of view, provide mutual support. If Marxism is viewed as an axiomatic science of society, akin to physics in the natural realm, then its most effective ally would be found in a positivistic philosophy of natural science, which views science in the same way.

It is true to say that not all rationalist ideologies are embraced by the following Marxist philosophers of education. Rational reconstructions are unacceptable, for example, but the commitment of scientism may be more powerful than the paradox of allying Marxism to a theory of science and epistemology which contradicts Marxism at several crucial points.<sup>58</sup>

Matthews’ argument is that consciousness is the product of processes of intellectual production, a view which he allows has considerable affinity with Popper’s World Three.<sup>59</sup> Further, Lakatos’ *Methodology of Scientific Research Programmes* (MSRP) is viewed as the most effective method for

appraising 'bodies of opinion' or 'theoretical discourses', in particular "liberal and Marxist theories of schooling".<sup>60</sup> At a more general level, Lakatos is used as a means to "intellectually . . . appropriate the world", while Marx is used as the basis "to act intelligently for its transformation".<sup>61</sup> Rationality, according to this view, is rooted in social praxis and social relations; but apparently scientific methodology transcends such connections. What vision of the good society is embodied in forms of scientific rationality, and what effect the uncritical adoption of such modes of inquiry might have, seem not to be important questions for Matthews, who appears to be more interested in making education yet one more natural science

The objectivity of scientific knowledge in accepted for example, without much acknowledgement of the import of the critiques of Kuhn,<sup>62</sup> Feyerabend,<sup>63</sup> Habermas,<sup>64</sup> Young,<sup>65</sup> Barnes,<sup>66</sup> *inter alia*, or indeed the findings of modern particle physics,<sup>67</sup> Why is it that chairs, and theories in the social sciences, both of which can likewise be characterized as objects, are recognized as having been fashioned by men and women but not theories of science? Why is the materialist insistence on this relation not fully comprehended, even by Marxist philosophers?<sup>68</sup> Above all, why is the dialectical view of Marx, so central to his work, eschewed in favour of an objectivism which however, as Matthews argues any epistemology must, contains "basic metaphysical and ontological commitments",<sup>69</sup> and which is therefore forced to ally itself with an objectivist philosophy of science which avers social praxis, and overtly opposes Marxism.<sup>70</sup> From our perspective, the attempt to correct the errors of one objectivist philosophy with the metaphysics of another involves a refutation of the concept of objectivity. How can one marry two objective theories of knowledge, one which is idealist and anti-Marxist, and the other which is Marxist and anti-idealist? From the point of view developed here, this represents not a correction but the heaping of one error upon another.

Other recent Marxist critics in educational philosophy reveal further interesting ideological baggage which they carry over, enthusiastically, from the rationalist tradition in science. In the course of reviewing, and defining the use of Lakatos' more sophisticated form of falsification as a guarantor of progress in the social sciences, Harris argues that

"criticising our total body of non-scientific theory, rendering such theory internally consistent and solving theoretical problems of a non-scientific sort is really a matter of applying the same general principles that underlie the specific process of scientific criticism . . . in which falsification is possible".<sup>71</sup>

Harris' embrace of methodological monism, however, rests on two assumptions, both of which are problematic. Firstly, that the model of scientific evaluation he wishes to apply is sound; and secondly, that differences between natural and social science are of degree rather than of kind. To take the former, it has been strongly suggested by philosophers and sociologists of science alike, that no single set of methodological rules, such as those proposed by Lakatos, is adequate to explain all scientific change. Both Barry Barnes as sociologist of science, and Feyerabend and Kuhn as philosophers and historians of science, have convincingly shown that the history of science is replete with episodes and examples which violate any particular set of methodological rules which supposedly guarantee progress, truth or warranted knowledge. One such quote, from the philosopher of science Mary Hesse is sufficient to show the force of the argument:

". . . every set of metaphysical or regulative principles that has been suggested as necessary for science in the past has either been violated by subsequent acceptable science, or the principles concerned are such that we can see that plausible developments in our science would in fact violate them in the future. As for principles of simplicity, there are so many different versions of these that their formulation has to be tailored to actual theories rather than theories to them; and as for the categorial principles, almost every traditional principle of space, time, matter and causality has been violated in modern physics."<sup>72</sup>

If, then, there are no singular sets of rules sufficient to guarantee progress in the natural sciences, then clearly no mandate exists for the supposed reformation of the social sciences by these same rules. It may well be that

“the distinction between scientific and other ‘types’ of knowledge and theory can be seen to be insignificant and irrelevant. . . .”<sup>73</sup>

but not for the reason that Harris supposes. On the contrary, modern philosophical and sociological explanations of scientific inquiry, at least since Kuhn’s influential demonstration of the paradigmatic way in which scientific knowledge proceeds and is structured within scientific communities, now argue that a degree of emphatic hermeneutics may be necessary to move from one paradigm to another within the natural sciences. The force of such accounts has been to replace the former rationalized, sanitized models of science with one which demonstrates a clear analogy with social scientific communities and inquiry. Consider the following contemporary characterization of scientific knowledge:

“context dependent, inherently fluid and imprecise, . . . lacking a clear fact theory distinction . . . and communally sustained.”<sup>74</sup>

To be fair, Harris is aware of some of the weaknesses of Lakatos’ scheme of theory preference, notably the problems of hindsight and idealism,<sup>75</sup> but does not accord the problems of conventionalism, a sufficiently central place. He does, however, acknowledge the social production of knowledge, and thus that particular historical circumstances can inhibit or prevent the growth of certain kinds of knowledge; “theoretical practice can be dominated or distorted by social practice”. Here again, however, Harris gives as the consequence of such domination the production of “not the real object but rather knowledge that serves social interest and practices”,<sup>76</sup> and his account of the ‘real object’ depends on a structural, scientific, Althusserian ‘problematic’, in which the individual human is seen not so much as a knowing, feeling active subject, but merely the “agent of theoretical production”, a conception which Harris allows “greatly diminish(es) the place of the knower”.<sup>77</sup> This devaluation of human being and human action, in which “teachers and students are presented as simply value receivers, . . . passive . . . cultural dupes”<sup>78</sup> is, as we have seen also present in rationalist ‘social science’ accounts of education and educational research by scholars of liberal political persuasion,<sup>79</sup> in the post war period.

Methodologies influenced by rationalism, whether of a Popperian or Lakatosian mode, exhibit at least three central problems. All of these problems are explicable in terms of a failure to sufficiently pursue the implications of the connection between knowledge and interests, or the social production of knowledge thesis, in the area of methodology of science itself. The lack of recognition of the fundamental nature of problems of conventionalism, and the elevation of ‘facts’ (‘empirical reality’) and ‘logical’ and ‘empirical’ tests<sup>80</sup> to apodictic status, are each of result of failing to conceive scientific methodology as itself contingent, man-made, and susceptible to many differing interpretations. The overt or covert emphasis on a science of education, renders the subject passive in the face of overwhelmingly powerful, often deterministic forces, laws or rules. If, however, no particular set of methodical precepts will warrant progress, then perhaps we ought to look beyond rules of method for criteria for adequacy, in both the natural and social sciences. Feyerabend in the natural sciences, and Habermas in the social sciences, have refused to be bound by the “infernal machine”<sup>81</sup> of scientized logic in developing evaluative criteria, and both reject the incursion of this same logic into our everyday life. Both tend to argue that the generally positivistic conquest of the social sciences proposed by both rationalist philosophers of science and scientific Marxists, is little more than an

historical confidence trick. Not only, might it be argued, are the social sciences wrong in attempting to scientize themselves, in an attempt to wrap themselves in the mantle of academic respectability, but the natural sciences are in fact more like the social sciences than they have led us to believe. Thus aping the natural sciences represents little more than the desire to put a philosophically shaky cart before a reasonably healthy horse.

Perhaps one of the most serious and insistent critiques Marxists have levelled at 'traditional', analytic philosophy of education is that it fails to make its assumptions and values explicit. Yet in uncritically adopting the perspective of scientific Marxism<sup>82</sup> with its view of Marxism as a science of society not unlike physics in the natural realm, Matthews, and Harris, fail to consider the alternative tradition of Marxist scholarship which views Marxism as critique, and firmly resists any movement to make Marxism into an axiomatic science. The test of truth for this latter tradition must be, as Wellmer argues, *vérité à faire*, rather than conformity to isolated standards of scientific methodology. Positivism is as much a danger in Marx as elsewhere,<sup>83</sup> and its constraints and demands will continue to distort theoretical development in the social sciences wherever scientific Marxism is allied to positivistic philosophy of science. Alternatives to scientism in education which are already developing<sup>84</sup> serve to highlight the unseeing commitment to an aberrant positivism in both philosophy of science, and Marxism, on the part of those who would wish to make education into a Marxian science.

We have seen, then, that philosophers of any particular political persuasion are equally susceptible to the ideology of modern rationalism: the belief in a neat, rationally ordered, objective world,<sup>85</sup> and a set of transcendental methodological rules which are violated in practice. We may explain such an ideological commitment, but it is harder to accept it.

#### Résumé

Les philosophies de la science ont été des sources importantes d'inspiration pour l'élaboration de théories en éducation. L'article traite de l'influence rationaliste sur la pensée éducative et, en particulier, de l'attrait qu'exerce Imre Lakatos sur les théoriciens en éducation depuis quelques années. L'auteur soutient que la philosophie rationaliste de la science transcende les barrières politiques de telle sorte qu'il n'y a plus de paradoxe à proposer le mariage d'une philosophie de la science idéaliste et anti-marxiste à une vision marxiste et anti-idéaliste du monde.

#### NOTES

<sup>1</sup> The philosophy of Karl Popper and its use by educational theorists in the post-war period, has been analyzed in Welch, A.R., *Theories of Science and Theories of Education: The Allure of Rationalism*. Part One: Karl Popper.

<sup>2</sup> Abel, P., 'The Nature of Scientific Change' (Review of Barnes, B., *T.S. Kuhn and Social Science*) *Times Higher Education Supplement*, 18/6/82.

<sup>3</sup> See for example Chalmers, A., *What is This Thing Called Science?* St. Lucia, University of Queensland Press, 1976.

<sup>4</sup> The paradox of blending two opposing philosophies, one anti-Marxist and idealist, and the other Marxist and anti-idealist has been historically associated with what might be called the 'Sydney' school of Althusserian

Marxism. Chalmers, *What is . . .* represents one example in the philosophy of science, while two other figures from the same department, J. Curthoys and W. Suchting, recently provoked an acerbic admonition from Feyerabend, in response to their critical review of his earlier book. See Feyerabend, P., 'Marxist Fairytales from Australia' *Science in a Free Society*, New Left Books, 1978. For the influence of these views on philosophers of education, see the prefaces to both Harris, K., *Education and Knowledge*, Routledge and Kegan Paul, 1979, and Matthews, M., *The Marxist Theory of Schooling*, Harvester, 1980.

<sup>5</sup> Although Lakatos preferred to refer to himself as a philosopher of mathematics. His work in this field is best represented in Volume two of his philosophical papers. Lakatos I (Ed. Worrall, J., and Currie, G.) *Philosophical Papers*, Volume II, Cambridge University Press, 1978.

<sup>6</sup> Lakatos, I., *Falsification and the Methodology of Scientific Research Programmes*, in Lakatos, and Musgrave, *Criticism . . .*, p. 97.

<sup>7</sup> *Ibid.*, p. 100.

<sup>8</sup> *Ibid.*, p. 102.

<sup>9</sup> *Ibid.*, p. 103.

<sup>10</sup> Popper, *Conjectures . . .*, p. 50. See on the fertility and suggestibility of myths and their importance, *Ibid.*, p. 38.

<sup>11</sup> As Lakatos points out, even calling the results of human eye-activity observational, itself only serves to indicate that we rely on some vague physiological theory of human vision. Lakatos and Musgrave, *Criticism . . .*, p. 107.

<sup>12</sup> *Ibid.*, p. 110.

<sup>13</sup> See Lakatos, I., 'Changes in the Problem of Inductive Logic', Lakatos, I. (ed.) *The Problem of Inductive Logic*, Amsterdam, North Holland Publishing Company 1968, pp. 397 ff., where this aspect is more fully explained.

<sup>14</sup> "How does one test a *ceteris paribus* clause severely? By assuming that there *are* other influencing factors, by specifying such factors, and by testing these specific assumptions. If many of them are refuted, the *ceteris paribus* clause will be regarded as well corroborated", Lakatos, I., *The Methodology of Scientific Research Programmes* (Philosophical Papers, Volume One), Cambridge University Press, 1978, p. 26.

<sup>15</sup> *Ibid.*, p. 26.

<sup>16</sup> Note Lakatos' interesting and provocative comments on the 'scientific sceptics dilemma', as applied to Hume (if science is mere belief, then what of Hume's theory of learning itself?) and Kuhn (and his attempt to develop a social psychology of paradigm change), *Ibid.*, p. 31.

<sup>17</sup> The problem of the role of auxiliary hypotheses had been raised before, by Pierre Duhem, in his famous *The Aim and Structure of Physical Theory*, of 1904: "if the predicted phenomenon is not produced, not only is the proposition questioned at fault, but so is the whole theoretical scaffolding used by the physicist". See Duhem, P., 'Physical Theory and Experiment', Harding, S.G., *Can Theories Be Refuted? Essays on the Duhem-Quine Thesis*. Dordrecht, Reidel 1976, p. 6. See also the radicalization of this view by Quine: "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system", Quine, W.V., "Two Dogmas of Empiricism", Harding, *Can . . .* Pp. 60; the critical response by A. Grunbaum to Duhem: 'The Duhemian Argument' Harding, *Can . . .*, Pp. 116-31, and Laudan's retort that Grunbaum overstated Duhem's argument: Laudan, L., "Grunbaum on 'The Duhemian Argument' ", Harding, S., *Can . . .*, Pp. 156-61.

<sup>18</sup> *Ibid.*, p. 33. But note Barnes' warning: "As research proceeds, qualifications, secondary hypotheses and arbitrary additions surround the initial theme; the line between explanation and rationalization becomes more and more difficult to draw. It becomes increasingly difficult to think of something which could count as a test of the existing system of knowledge." Barnes, B., *Scientific Knowledge and Sociological Theory*, Routledge and Kegan Paul, 1974, p. 47.

<sup>19</sup> Lakatos here argues that Popper fails to distinguish clearly enough between theories, as singular events, and series of theories. An individual theory, according to Lakatos, can only be judged to be scientific in the context of the historical evolution of the programme of which it is a part.

<sup>20</sup> Lakatos and Musgrave, *Criticism* . . . , p. 119.

<sup>21</sup> Newton's *Principia* was unable to adequately account for the motion of the moon; Einstein's relativity theory was 'refuted' by Kaufmann, himself distinguished, in the first year of its appearance. See Feyerabend, *Against*, p. 56 and n., 202 and n.

<sup>22</sup> Lakatos, I., *The Methodology* . . . , p. 45. See also *Ibid.*, 44, 45, and n, where this method of trying to eliminate inconsistencies between interpretive theories (which provide facts) and explanatory theories (which explain them) can take the form of the theoretician questioning, and perhaps replacing, the experimentalists' facts, an example of which is cited in respect to the debate between Newton and Flamsteed, the first Astronomer Royal. Note here also Lakatos' admission of a form of induction (a 'synthetic inductive principle') which he claims is necessary within the Popperian research programme, to avoid lapsing into scepticism or apriorism. See Lakatos, I., 'Popper on Demarcation and Induction', *The Philosophy of Karl Popper*, Schilpp, P.A. (Ed.), The Open Court Publishing Company, Illinois, 1974, I, pp.256-270.

<sup>23</sup> Which, however, is not to say that this hard core is apparent or fully articulated at the beginning of the programme. On the contrary it evolves slowly, with the development of the programme. The negative heuristic directs the *modus tollens* away from the hard core.

<sup>24</sup> Lakatos contrives to give a brief sketch of Newtonian gravitational theory ("the classical example of a successful research programme") and argues that despite the number of anomalies that were present with the original theory, successive Newtonians ingeniously managed to overcome and explain these by modification to the 'protective belt'. In turn, they produced, and resolved further anomalies. Meanwhile, it had been decided that Newton's three laws of dynamics, and his law of gravitation were to form the 'hard core' of the programme. The development of solutions to the anomalies raised, in turn added to the corroborated empirical content of the theory, thereby representing a progressive problem-shift. See Lakatos 'The Methodology . . .' pp. 48-52 (*passim*), 16-17.

<sup>25</sup> *Ibid.*, p. 51. Or to put it another way, "Our considerations show that the positive heuristic forges ahead with almost complete disregard of 'refutations': it may seem that it is the verifications which provide the contact points with reality." *Ibid.*, pp. 51-2. But consistency must ultimately be the aim. It is not possible to permanently hold to two inconsistent findings or theories, if no model which successfully incorporates both can be articulated. Temporary, or apparent, contradictions may be tolerated; indeed, with hindsight, important or necessary. See Lakatos' explanation of attempts by Bohr to reconcile his theory of quantum action with classical mechanics and electro-dynamics, an effort which had positive and negative results according to Lakatos, *Ibid.*, pp. 55-68 but esp. 58 and n. For different potential reactions to such a possible dilemma, see *Ibid.*, 59.

<sup>26</sup> At this point Lakatos points to the conflict he sees between Popperian criticism and fallibility as being both the mainspring and the central problem of the Popperian model of epistemology. I can only take this to mean, in this context, that the recognition of the fallibility of all knowledge (even relatively well-established knowledge) forces us to suspend, to a degree, our naturally rigorously critical attitude towards theoretical newcomers, even if their empirical verisimilitude is as yet unsubstantial. See *Ibid.*, p. 71 n.

<sup>27</sup> Note at this crucial juncture, the Kuhn-like quality of the metaphor which is invoked. In employing terms like 'battle', 'war', etc. as descriptors, Lakatos gives implicit support to the Kuhnian position: Kuhn's phrase in this context would be something like 'battle for dominance'. The behaviour of scientists (fallible and human) in preventing anomalies from being accepted as legitimate refutations is also given Kuhnian overtones (although finally described as being 'unscientific') as too the forms of resolution of anomalies or 'puzzles'. *Ibid.*, pp. 71, 72, and n, 73.

<sup>28</sup> *Ibid.*, p. 77. This again lends weight to actual decisions of real scientists, rather than abstract rules of procedure. This is supported by Feyerabend's critique of the inadequacy of Lakatos' rules on evaluation of programmes. Feyerabend, P., "On the Critique of Scientific Reason", Howson, C., *Method and Appraisal in the Physical Sciences*, Cambridge University Press, 1976, p. 322, *et seq.* See also on the importance of hindsight, Turner, D., 'Some Considerations on the Hypothetico-Deductive Method', Unpublished MS., University of London Institute of Education, 1981.

<sup>29</sup> The English translation of this work was published as *The Logic of Scientific Discovery*, Hutchinson, 1959 and contains important statements on, *inter alia*, conventionalism. See p. 53. The question remains what one makes of this point.

<sup>30</sup> Feyerabend, P., 'Popper's Objective Knowledge' *Inquiry* 17, 1975, p. 497. But as Musgrave points out this is still insufficient. Even worse, Musgrave's solution is no better: Musgrave, A., 'Method or Madness? Can the Methodology of Research Programmes be rescued from Epistemological Anarchism?' *Boston Studies in the Philosophy of Science*, Vol. 39, *Essays in Memory of Imre Lakatos*. Cohen, R.S., Wartofsky, M.W. & Feyerabend, P.K., (eds) Dordrecht, Reidel, 1976.

<sup>31</sup> Rationalise can have more than one meaning, as Habermas has made clear, including the Freudian one of the concealment of unconfessed motives. See Habermas, J., *Theory and Practice*, Heinemann, 1974, pp. 270ff, and Welch, A.R., 'Ideology, Sociology and Education. Some Developments and Relationships', *Australian and New Zealand Journal of Sociology*, 16, 2, 1980 pp. 74-5, 80n. Habermas' work on rationality, including that sense given above, seems most appropriate to the rationalist assumption of the substitutability of science and rationality.

<sup>32</sup> See Feyerabend 'On the Critique of Scientific Reason' *Essays in Memory of Imre Lakatos* (Boston Studies in the Philosophy of Science Vol. XXXIX, D.Reidel, Dordrecht, 1976 and Barnes' outline of a generally positivistic account of science in Barnes, *T.S. Kuhn and Social Science*, Macmillan, 1982, p. 61-3. See also Adorno's 'Introduction' to Adorno, T.W. *et. al.*, *The Positivist Dispute in German Sociology*, Heinemann, 1976.

<sup>33</sup> Lakatos, I., and Musgrave, A., *Criticism and the Growth of Knowledge*, Cambridge U.P. 1970.

<sup>34</sup> Papineau, D., 'Thinking Up Reality', *Times Literary Supplement*, 29/10/1982, p. 1198.

<sup>35</sup> Kuhn, *The Structure of Scientific Revolutions*, Chicago, University of Chicago Press, 1970. See Feyerabend's general acceptance of this position in *Science in a Free Society*, New Left Books, 1978, pp. 66-70.

<sup>36</sup> See Kuhn's discussion of truth relative to paradigms, where he argues tests are seldom 'apodictic', Kuhn, T., 'Logic of Discovery or Psychology of Research', Lakatos and Musgrave *Criticism*. . . , p. 10. See also Barnes' treatment of truth in histories of science, Barnes, B., *Scientific Knowledge and Sociological Theory*, Routledge & Kegan Paul, 1974, p. 7, and *T.S. Kuhn*, p. 4. Feyerabend too accepts that incommensurability poses insuperable difficulties to arguments based on increasing verisimilitude, 'Consolations for the Specialist', Lakatos and Musgrave *Criticism*. . . , p. 220 *et. seq.* and *Against Method*, New Left Books, 1975, pp. 163-4, 193. Mary Hesse also sees verisimilitude as 'misleading', *Revolutions and Reconstructions in the Philosophy of Science*, Harvester, Sussex, 1980, p. 217.

<sup>37</sup> See Feyerabend's early 'Wittgenstein's Philosophical Investigations', *Philosophical Review* LXIV, 1955, and Feyerabend, P., *Against Method*, New Left Books, 1975, pp. 275-6.

<sup>38</sup> See Kuhn's and Feyerabend's excellent discussion of the linguistic allusion in Lakatos and Musgrave *Criticism*. . . , respectively pp. 267-71, and 225. Kuhn's and Feyerabend's ideas both derive, to an extent, from Wittgenstein, and serve to highlight the dependence on a universal observation language by Lakatos, and Popper.

<sup>39</sup> Papineau, D., 'Thinking. . .', p. 1198.

<sup>40</sup> This would seem to be the implication of Kuhn's work and Feyerabend's use of Wittgenstein. See also Barnes' analysis of Habermas, Barnes, B., *Interests and the Growth of Knowledge*, Routledge and Kegan Paul, 1977, esp. pp. 18ff.

<sup>41</sup> See Feyerabend, 'On the Critique of Scientific Reason', Howson, C., *Method and Appraisal in the Physical Sciences*, Cambridge University Press, 1976, *passim*, and on Lakatos' standards p. 327. See also Feyerabend, *Science*. . . , pp. 74-5 and Habermas, J., 'Technology and Science as Ideology' *Toward a Rational Society*, Heinemann, 1971.

<sup>42</sup> See, for example, Lakatos, I. and Zahar, E., 'Why Did Copernicus's System Supersede Ptolemy?', Lakatos, *The Methodology*. . . , which fails to answer its own question and would be more properly titled, "Why

ought Copernicus's System to Have Superseded Ptolemy's?' Compare this with Kuhn's fuller, more concrete, historical examination of the same content, Kuhn, T. *The Copernican Revolution: Planetary Astronomy and the Development of Western Thought*, New York, Random House, 1959. See also Kuhn's reply to Lakatos's attempt to distinguish between normative, and social-psychological histories of science, Kuhn, T., 'Reflections on My Critics', Lakatos and Musgrave, *Criticism*. . . , pp. 233, 237-41.

<sup>43</sup> Feyerabend, *Science*. . . , p. 25. See also Barnes, T.S. *Kuhn*. . . , pp. 61-3, and Feyerabend *Against*... , p. 19. "Scientific education . . . simplifies education by simplifying its participants."

<sup>44</sup> These criteria often operated in the past: 'Man is the measure of all things', originally voiced by Protagoras, was an important catch-cry of the Renaissance. The rise of science, and theories of science from the seventeenth century, signalled the demise of such traditional forms of evaluation, and heralded their replacement with a powerful new value: scientific methodology. See on this Feyerabend *Science*. . . , p. 178 and Habermas 'Technology. . . '

<sup>45</sup> See Feyerabend, 'Consolations. . . ', p. 210.

<sup>46</sup> See Marcuse, H., *One Dimensional Man*. Sphere, 1968.

<sup>47</sup> See Adorno, 'Introduction' *passim*.

<sup>48</sup> ". . . the scientific approach to reality respects only efficiency and theoretical adequacy, no matter what damage this does to the spirit of man. . ." p. 178.

<sup>49</sup> Habermas, J., *Theory and Practice*, Heinemann 1974, p. 270.

<sup>50</sup> See Feyerabend, P., 'On the Critique. . . ' pp. 322 *et. seq.* and on Popper, Feyerabend, P., 'Popper's Objective Knowledge', *Inquiry* 17, 1975.

<sup>51</sup> Feyerabend, *Science*. . . , p. 70.

<sup>52</sup> Feyerabend, 'Popper's. . . ' p. 496. See also Kuhn's argument on the limits of logic (but not its absence!), 'Reflections on my Critics', Lakatos & Musgrave, "*Criticism*. . . , pp. 260-6.

<sup>53</sup> To repeat, Popper's articulation of his Three Worlds gives as the 'inhabitants' of World Three the contents of theories, beliefs, books, problem situations etc. By defining his ontology in this way, Popper is making clear claims for the relative autonomy of theory, but see Feyerabend's comments 'Popper's. . . ' pp. 479 *et. seq.*, and Habermas' critique, Habermas, J., 'An Approach to the Rationality Problematic Through the Problem of Action', (Tr. McCarthy, T.) 1981 (?), pp. 68-72. See also Grunfeld, J., *Science and Values*, B.R. Gruner, Publishing Company, Amsterdam, 1973, introduction.

<sup>54</sup> Lakatos, *The Methodology*. . . , p. 90.

<sup>55</sup> Kuhn, T., 'Reflections. . . ', p. 233. Conventionalist decisions (about where to draw the line) may be seen in this light. See also n. 26. above, and Popper, *The Logic*. . . , pp. 106-11, esp. 111.

<sup>56</sup> Feyerabend, P., 'Popper's. . . ', p. 495. See also p. 493 for further criticism of Popper's Platonism.

<sup>57</sup> Matthews, M., *The Marxist*. . . Lakatos' relationship with the Marxist tradition was less dogmatic than Popper's. See Lakatos I., *Philosophical Papers, Vol. II*.

<sup>58</sup> This has been pointed out, in different ways, by Haynes, and Aspin, in the reviews mentioned below.

<sup>59</sup> *Ibid.*, p. 4., Matthews cites Marx, K., *The Economic and Philosophic Manuscripts of 1844*, New York, International Publishers, 1964, pp. 113-139 to show consciousness as a social product.

<sup>60</sup> Matthews, *The Marxist*. . . , p. 5.

<sup>61</sup> *Ibid.*, p. 6. The contradictions inherent in using two opposing research traditions in science and epistemology have been pointed out already, but see also Aspin, D., ' "Revolutionary Practice" or 'Philosophy of Education', *Comparative Education* , 16, 2, 1980.

<sup>62</sup> See Kuhn's sophisticated conventionalism in *The Structure*. . .

<sup>63</sup> Feyerabend's critique of objectivism and embrace of a degree of relativism, are mainly contained in *Against . . . and Science. . .*

<sup>64</sup> Mainly in 'Technology. . .' but see also Mary Hesse's analysis of the Habermasian critique of scientific conceptions of objectivity 'Science and Objectivity', Thompson, J.B. and Held, D., *Habermas, Critical Debates*, Macmillan, 1982, esp. pp. 102 *et. seq.*

<sup>65</sup> Young, R.M., 'Science is Social Relations', *Radical Science Journal*, 5, 1977.

<sup>66</sup> Barnes, B., *Scientific. . . , Interests. . . , and T.S. Kuhn. . .*

<sup>67</sup> A curious omission for a work studded with references to scientific research findings and their implications. (For the argument on objectivism in Matthews, see *The Marxist. . .*, pp. 54-5 *et passim*). A useful non-specialist discussion of the radical implications of modern particle physics is in Davies, P., *Other Worlds*, Abacus, 1982.

<sup>68</sup> Matthews discusses the necessities of drawing the line between 'internal' and 'external' histories of science, admits Lakatos' deficiencies in this respect, allows the importance of social relations and forces of production in affecting the history of science, but does not pursue the question within the realm of methodology. His review of the I.Q. controversy, however, does adequately include external history, and ideology, within its account.

<sup>69</sup> See Haynes, F., Review of Matthews, M., *The Marxist. . . , Discourse 2*, 2, 1982, p. 64.

<sup>70</sup> Marx and Popper are curious bedfellows. Our argument here as to the irony of this uneasy alliance (an irony unacknowledged by Matthews) is supported by Haynes' review. See *ibid.*, p. 65. To repeat the lesson which Matthew ignores, science like other forms of discourse, is ideological and cannot be separated from social relations.

<sup>71</sup> Harris, K., *Education and Knowledge; The Structured Misrepresentation of Reality*, Routledge and Kegan Paul, 1979, pp. 44-5.

<sup>72</sup> Hesse, K., *Revolutions and Reconstructions in the Philosophy of Science*, Harvester Press, Sussex, 1980, p. x.

<sup>73</sup> Harris, *Knowledge. . .*, p. 45.

<sup>74</sup> Barnes, B., *Interests. . .*, p. 18.

<sup>75</sup> See Harris, *Knowledge. . .*, pp. 46-7 and p. 61 where the idealism of Popper and Lakatos is described and criticized. But, as with Matthews, the irony of using an anti-Marxist idealist metaphysics to buttress an overtly materialist and Marxist account of education is suppressed by Harris. See here the review by Aspin, ' "Revolutionary. . ." ', p. 177, where Harris is criticized for having his Kuhn and eating it too.

<sup>76</sup> *Ibid.*, p. 59.

<sup>77</sup> *Ibid.*, p. 57.

<sup>78</sup> Giroux, *Ideology, Culture and the Process of Schooling*, Falmer Press, 1981, pp. 14-15.

<sup>79</sup> See the examples in Welch, 'Theories. . .' (Part One).

<sup>80</sup> See Young, R.E., 'Progressive and Degenerating Education Policy Theories, Young, R., Pusey, M., and Bates, R., (Eds.), *Australian Educational Policy Issues and Critique*. Deakin University, 1982, p. 16. See also Young's acknowledgement of the suggestibility of Harris' work, p. 15.

<sup>81</sup> Adorno, 'Introduction. . .', p. 2.

<sup>82</sup> See Larrain, J., *The Concept of Ideology*. Hutchinson 1979, esp. Ch. 6. and Gouldner, A., *The Two Marxisms*, Macmillan 1980, esp. Part One, Chapters 2 and 3.

<sup>83</sup> See the work of Wellmer, A., *Critical Theory of Society*, New York: Herder and Herder, 1971; and Habermas, J., *Knowledge and Human Interest*, Heinemann, 1978.

<sup>84</sup> Notably Giroux, *Ideology*. . . See pp. 14-19 and passim for the limitations of Althusser in educational analysis.

<sup>85</sup> Feyerabend dismisses (rationalist) philosophers of science who are committed to such a view, as embodying a "craving for orderliness (which) easily exceeds that of the most systematic, scientist, and approaches that of a catatonic". Feyerabend, "On the Critique. . ." p. 113.