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KUHN REVISITED: SCIENCE, EDUCATION AND VALUES *

Thomas Kuhn's theories about the nature of science have to some extent overshadowed previous controversies about the nature of scientific change which raged between historians, philosophers, sociologists and, at times, even professional scientists. In the 1920's and 30's the major intellectual force was that of logical positivism, a highly abstract doctrine of little interest to practising scientists. Then came Karl Popper with his continuous sequence of conjectures and refutations to be followed by still further conjectures. Popper's view of science was on the ascent when it was eclipsed by Thomas Kuhn and his *The Structure of Scientific Revolutions*¹ with its concepts of paradigm, normal and extraordinary science, crisis and revolution. Paradigms were much larger units than the theories (conjectures) of Popper and they had a sociological basis which previous notions of science had so singularly lacked. As Kuhn presents it, science is not an internalist, hermetically sealed enclave of pure ideas but is influenced by all kinds of non-cognitive factors which arise when we consider scientific activity as taking place within the context of a well defined community.

Kuhn's theory made its first appearance to a wider public at a History of Science symposium at Oxford in 1961. The emphasis then was not on "paradigm" but on "dogma" — and it was partly under the influence of criticism which he received in such liberal doses from the philosophers

* The author's previous contributions to the subject were: "Conceptual Models in Natural and Social Sciences and Their Implications for Educationists", in: R. J. W. Selleck (ed.), *Melbourne Studies in Education 1968-1969*, Melbourne University Press, Melbourne, 1969, pp. 71-113; "Paradigms and Models: A Comparison of Intellectual Frameworks in Natural Sciences and Sociology", *Australian and New Zealand Journal of Sociology*, vol. 6, 1970, pp. 100-119; "Amorphous Paradigms: A Critique of Sheldon Wolin's 'Paradigms and Political Theories'", *Politics: Australian Political Studies Association Journal*, vol. 6, 1971 pp. 178-87.

¹ The University of Chicago Press, Chicago and London, 1962.

and historians of science there assembled that the paradigm became the unmistakable pivot of the whole theoretical construct. At that symposium it was Polanyi (a physicist turned sociologist and psychologist of science) that proved Kuhn's only faithful ally — a pattern to be repeated at subsequent meetings of this sort.

The main points of Kuhn's thesis are too well known now to need any but the briefest mention (this in itself is significant — no other theory of science had such a wide circulation and appeal among scientists and the other academics outside the charmed circle dedicated to the study of the philosophy of science). According to Kuhn's original version of the theory (1962), mature science is a succession of normal periods and revolutions. Normal periods are very largely monistic with members of a mature scientific community trying to solve puzzles resulting from the attempt to see the world in terms of a *single* paradigm or of a closely related set of paradigms. Revolutions are pluralistic until a new paradigm emerges that gains sufficient support to serve as the basis for a new normal period. What is specially distinctive about science is its period of so called "normality" when consensus and unanimity of professional opinion reign supreme. As Kuhn himself put it, "a careful look at the scientific enterprise suggests that it is normal science, in which Sir Karl's (Popper's) sort of testing does not occur, rather than extraordinary science which most nearly distinguishes science from other enterprises."²

This view is echoed by Ziman when he writes that the "objective of science is not just to acquire information nor to utter all non-contradictory notions; its goal is a *consensus* of rational opinion over the widest possible field."³ Ziman, be it noted, is a distinguished physicist — and the convergence of his views on science with those of Kuhn is quite remarkable. Kuhn did not fare so well at the hands of the philosophers of science gathered at the International Colloquium in the Philosophy of Science held in London in 1965 where he was again subjected to some vigorous attacks of the philosophers, among them Popper (terrified that the mantle of leadership was slipping from his shoulders), Watkins, Toulmin and even Lakatos who, for all his attempts to introduce some kind of Popper-Kuhn synthesis, shows a clear dislike of many aspects of Kuhnian theory including its supposed "authoritarian and irrationalist overtones."⁴

At the Colloquium it was a scientist Margaret Masterman who almost alone rallied to Kuhn's defence and produced a most persuasive and

² T. Kuhn, "Logic of Discovery or Psychology of Research", in: I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge*, Cambridge University Press, Cambridge, 1970, p. 6.

³ J. Ziman, *Public Knowledge: The Social Dimensions of Science*, Cambridge University Press, Cambridge, 1968, p. 9.

⁴ I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes", in: Lakatos and Musgrave, *op. cit.*, p. 92.

coherent critique and extension of Kuhn's views.⁵ Kuhn had indeed given so many definitions of a paradigm as to make the concept more obscure than it need be. (Masterman claims that Kuhn uses the term in no less than 21 different senses and "possibly more, not less".) Analysis of those different usages reveals however that the paradigm has *three* distinct facets. We thus have *metaphysical* paradigms (which refer to a set of beliefs, a myth, an organising principle governing perception itself, or a map) and this is the kind of theoretical species which Kuhn's critics principally refer to. (Indeed, Rupert Hall in his 1961 critique of the concept made it clear that to him the term "paradigm" stands simply for an "intellectual framework".⁶) Another facet of the concept is that of a universally recognized scientific achievement — which gives rise to a definition of paradigm as a *sociological* entity. Finally Kuhn defines paradigm as something very concrete — an actual instrumentation or problem solution, as an entity supplying tools, a tacit knowledge not derived from any symbolic generalization but actually learned on the job. In Kuhn's words, it involves „learning which is not acquired by exclusively verbal means. Rather it comes as one is given words together with concrete examples of how they function in use; nature and words are learned together.”⁷ Masterman refers to this third facet of the original Kuhnian concept as a *concrete* paradigm and her definition of the concepts of metaphysical, sociological and concrete paradigms comes close to Kuhn's subsequent usage of the collective term *disciplinary matrix* made up of (a) beliefs in particular models, (b) shared values, (c) symbolic generalizations, and (d) exemplars (which I take to be equivalent to Masterman's concrete paradigms).

After all these modifications what we have left is a powerful and most persuasive theory to explain the nature and development of science, a theory which (unlike its predecessors) has an immediate appeal to most practising scientists. What is more, it has repercussions outside of the research laboratory, for it illuminates much that at first sight may be puzzling about the nature of scientific instruction, of the way in which novices are initiated and socialized into the profession. It also throws light on the relationship of science to the other branches of learning by stressing the unique characteristics of science. This in turn touches materially upon a much wider debate on the distinct features of various disciplines and the current fragmentation of knowledge, its purposes and justification. Kuhn's works thus form an indispensable reading for all intellectuals, be they scientists, philosophers, educationists or historians of art, science or philosophy.

⁵ "The Nature of a Paradigm", in: Lakatos and Musgrave, *op. cit.*, pp. 59-90.

⁶ R. Hall, "Commentary on 'Problems in the Sociology of Science'", in: A. C. Crombie (ed.), *Scientific Change*, Heinemann, London, 1963.

⁷ T. Kuhn, *The Structure of Scientific Revolutions* (2nd ed), 1969, p. 191.

In the field of education commitment to the reigning paradigm produces a consensus of opinion on what the young aspirants should be taught — hence a great reliance on textbook instruction among scientists. Fundamentals recorded in textbooks must first be mastered before a student's imagination can be let loose on the problems which are currently preoccupying the attention of the profession; and these problems too will be more in the nature of puzzles within the accepted framework rather than revolutionary hypotheses designed to shatter such a framework. Such description of the situation refers, of course, to the periods of normal science when the scientists have confidence in their paradigm and in the existence of solutions to the puzzles which it generates.

It is this view of normal science as the essence of science and of the consequent methods of instruction of the young which provoked such fury and outcry among Kuhn's numerous opponents and detractors. Their views range from the assertion that normal science as described by Kuhn does not exist and is merely a figment of his imagination to a grudging admission that, even if such activities do go on, they occur on the periphery of the big, creative science and that they attract the feeble minded, the conformist and least gifted among the scientists. It is further asserted that the scientific education to which they give rise is a perversion of what should be an initiation into a creative, free ranging and imaginative activity in which no theory, however dominant and successful, no paradigm, is ever sacred and untouchable.

Thus Watkins, after first dismissing "normal science" as hack work and an exercise fit only for plodders, later questions its very existence for, in the form in which it is described by Kuhn, it is so conservative and makes scientific community such a closed society that it could never give rise to extraordinary (revolutionary) science.⁸ Popper, on the other hand, claims that "normal" science does *exist* — as an activity of "the not-too-critical professional (and of the) science student who accepts the ruling dogma of the day ... (one) who accepts a new revolutionary theory only if almost everybody else is ready to accept it — if it becomes fashionable by a kind of bandwagon effect". In fact Popper openly admits to intensely disliking the phenomenon but shows rather more magnanimity to the conformist involved in such base activities, for he merely feels "sorry" for him. It is not his fault. He has "been taught badly ... he is a victim of indoctrination". But the phenomenon itself, which Kuhn assesses as "normal", Popper regards with more than mere derision, for he sees "a very great danger in it and in the possibility of it becoming normal: a danger to science and, indeed, to our civilization."⁹ The danger

⁸ J. Watkins, "Against 'Normal Science'", in: Lakatos and Musgrave, *op. cit.*, p. 27.

⁹ K. Popper, "Normal Science and Its Dangers" in: Lakatos and Musgrave, *op. cit.*, *passim*, pp. 52-3.

lies in assuming the domination of dogma over considerable periods and in rejection of the view that the normal method of science is that of bold conjecture and criticism.

At this stage in the discussion it would be of interest to look at the opinion of scientists themselves, especially those with considerable experience of research and teaching. In this connection the views of the Cavendish Professor of Physics, Professor Brian Pippard would carry special weight. In his inaugural lecture at Cambridge University he gave us a rather clear picture of the current education of a physicist.¹⁰ This reads almost as if it were written in collaboration with Professor Kuhn — yet at the time it was delivered Pippard has not read *The Structure of Scientific Revolutions*. In brief, what universities attempt is to turn out professional researchers whose ideas are close to those of their mentors: a succession of technically accomplished performers well groomed in the current theories of physics but ignorant of society and its needs (a state in which they are likely to remain throughout their professional lives unless they are able to supplement for themselves the diet provided them at the Cavendish Laboratory). According to Pippard this type of education is based on the “curiously limited view... about what constitutes the essence of scientific thought”. The source of this limitation “lies in the extraordinary perfection of one side of physics, the formal structure of the Laws of Nature which is the foundation of our science and which in itself we manage to teach rather well”. This has led to a misapprehension that these laws are the foundation upon which the understanding of Nature is built.

We therefore proceed to teach these laws and this task becomes almost an end in itself. The methods which we adopt to inculcate them into our pupils involve not only the theory, not only the verbal instruction, but also a “fine collection of standard problems to which the laws can be applied to give the right answer”. And however artificial these problems may sound to the uninitiated, “the physics student who has been exposed to that sort of thing for three or four years fails to notice how artificial they are.”¹¹ To put it another way, he has now been socialized into the profession and looks at these things through the eyes of a community whose gestalt he has thoroughly assimilated. Here Pippard, who is himself the author of such a book of problems and (as he himself admits) hardly in a position to deny their value, is referring to that component of Kuhn’s paradigm to which the label of “exemplars” has now been given and which Masterman has called the concrete paradigm. Kuhn defines the exemplars as the “concrete problem-solutions that students encounter

¹⁰ *Reconciling Physics with Reality*, Cambridge University Press, Cambridge, 1972.

¹¹ *Ibid.*, pp. 5-6.

from the start of their scientific education, whether in laboratories, on examinations, or at the end of chapters in science texts." In Kuhn's view such problems are not simply there to provide them with practice in the application of what they already know. "After the student has done many problems, he may gain only added facility by solving more. But at the start and for some time after, doing problems is learning consequential things about nature. In the absence of such exemplars, the laws and theories he has previously learned would have little empirical content". The main skill that a student acquires from doing such exemplary problems is a way of seeing a variety of situations as like each other, as subjects for the same symbolic generalization, such as $f=ma$. "After he has completed a certain number ... he views the situations that confront him as a scientist in the same gestalt as other members of his specialists' group."¹²

Pippard emphasizes that such problem exercises are in some way related to five-finger exercises at the piano, i.e. although valuable and in fact essential they are not an end in themselves. It is of interest that Kuhn too makes a similar analogy and that he also refers to such a process of professional initiation as a "narrow and rigid education, probably more so than any other except perhaps in orthodox theology."¹³ In his view, however, it may be narrow but nonetheless it is indispensable for there is no other way of making a student a fully fledged member of a particular scientific community. And Pippard's description of what actually goes on in science laboratories and lecture rooms (an evidence which could be corroborated by countless other scientists) would seem to provide supporting evidence for Kuhn's theory.

The difference between Kuhn and Pippard, however, is that while the close fit between his theory and the actual experiences of scientists gives Kuhn grounds for satisfaction and he rejoices in the "immense effectiveness" of this type of education,¹⁴ Pippard deplores many of its aspects, for in his view "too much emphasis on problem solving where the answer is provided at the end of the book obscures certain important aspects of real physics, which we fail to teach as competently as those already mentioned." For example, because many of the physical problems are mathematically intractable "we resort to guessing and insight ... far more than one would infer from looking at the syllabus of a physics course ... (yet) we have never seriously tried to devise techniques for teaching people how to make reasonable guesses." There are other ways in which our current teaching is deficient. Thus in "real physics", we frequently encounter problems which clearly have an answer but where one does not have any indication how to start working them out. We may then have to rely on qualitative observations on the "intuitive feeling for

¹² T. Kuhn, *The Structure of Scientific Revolutions*, passim, pp. 187-9.

¹³ *Ibid.*, p. 165.

¹⁴ *Ibid.*, p. 164.

what can and cannot happen". In Pippard's opinion it is this type of intuition which is the mark of the sound scientist and it is also a quality which is "not developed by concentration on the laws and their exact application."¹⁵

Indeed, there is a stark contrast between Kuhn's implied approval of the present system on the grounds that although our "scientific training is not well designed to produce the man who will easily discover a fresh approach" it will nevertheless turn out a scientist who for the purposes of "normal-scientific work ... is almost perfectly equipped",¹⁶ and Pippard's plea for the need to develop a "fascination for ... all the marvelously complicated things that can happen, that are worth looking at and speculating about even though one knows an exact analysis is not practicable." Pippard's conclusion is that it is this side of a scientist's life that is the "spring of his imaginative originality" and that by neglecting to develop it "we are losing a great educational opportunity".¹⁷

Pippard's almost poetical stress on "intuition", "feeling", "marvel" and the "fascination" with what is still not fully explicable and analysable shows how a most distinguished scientist *would like* his students to be educated; it is a far cry from the current reality and a far cry from Kuhn's description of that reality. But if Pippard is correct, if our present scientific education is sadly deficient in stressing those very qualities which distinguish a highly imaginative and gifted scientist from a hack one — then Kuhn's theory which accurately describes such highly unsatisfactory reality carries within it seeds of danger. As Kuhn himself points out, "the consequences (of his theory) are not exhausted by the observations upon which it rested from the start." Once formulated it becomes a "useful tool for the exploration of scientific behaviour and development."¹⁸ It can also act as a means of legitimising certain current practices in scientific education and thereby help to perpetrate a narrow and one sided training to which Pippard objects. Whether Pippard is correct in his assumption that qualities such as "intuition" and "fascination" can be successfully taught at undergraduate level and, even if they can, whether this can be achieved with any but the most gifted students and very brilliant instructors is another matter. It would seem that a Cavendish Laboratory at Cambridge would be a most appropriate place to investigate the matter. However, we should listen with respect to the views of such a distinguished and experienced scientist as Professor Pippard. Analysis of this type is also valuable because it draws our attention to the *proscriptive* elements of Kuhn's theory and its actual influence on science and education.

¹⁵ B. Pippard, *op. cit.*, passim, pp. 7-12.

¹⁶ T. Kuhn, *op. cit.*, p. 165.

¹⁷ B. Pippard, *op. cit.*, p. 12.

¹⁸ T. Kuhn, *op. cit.*, p. 208.

This is the significance of Kuhn's work which is particularly stressed by Feyerabend¹⁹ who almost alone of Kuhn's philosophical critics discusses its implications not only from the internalist-scientific point of view but also for the effect which it is likely to have on the social sciences, on the social relations of science, and on education not only of the science specialists but also of students who had no intention of becoming research scientists.

Feyerabend sees the social and educational effects of Kuhn's thesis as almost wholly negative. Nor is he satisfied with looking at the theory itself, but also examines the *ideology* which has prompted Kuhn to propound it. History and philosophy of science is not, of course, itself a natural science and as a humanity and/or social science it is much more subject to the influence of values. Indeed, such influence is almost unavoidable and can be positively beneficial. The role of values in the choice of subjects for research and their subsequent role as selectors of evidence must, however, be always carefully scrutinised. Otherwise, values might influence one's work without one being aware of it and hence without control. Alternatively, the author may be well aware of the role which values play in his work but may keep this knowledge from his readers.

It is clear that Feyerabend suspects Kuhn of a conservative bias and of consciously wishing to exploit his theory's "propagandistic potentialities" while hiding behind the facade of objective description of "facts". The ideology which he uncovers in Kuhn's work could "only give comfort to the most narrow minded and the most conceited kind of specialism. It would tend to inhibit the advancement of knowledge. And it is bound to increase the anti-humanitarian tendencies which are such a disquieting feature of much of post-Newtonian science."²⁰ Thus Feyerabend's examination of Kuhn takes place at *two levels*. There is the „internalist" critique of the adequacy of the theory's explanation of scientific change and, interspersed with it, an attack on its effect on the world outside science.

In terms of the internalist philosophico-methodological discourse Feyerabend is surprisingly close to many of Kuhn's views, for they both reject as absurd Popper's idea that theories are "blameless for decades and even centuries until a big refutation turns up and knocks them out."²¹ Such myth, they claim, is believed by the existence of anomalies at any point in the history of a paradigm. This leads Feyerabend to master a whole battery of arguments in favour of what he terms a "principle of tenacity", i.e. the ability of scientists to select from

¹⁹ P. Feyerabend, "Consolations for the Specialist", in: Lakatos and Musgrave, *op. cit.*

²⁰ *Ibid.*, p. 198.

²¹ *Ibid.*, p. 207.

a number of theories one that promises to lead to most fruitful results and to stick to this one theory even if the recalcitrant facts which it encounters "should happen to be as plain and straightforward as daylight itself."²²

Where Kuhn's and Feyerabend's ways part is in relation to what Feyerabend describes as Kuhn's "monomaniac concern with only a single point of view" and his insistence that during periods of normal science each puzzle-solving tradition is normally guided by its own single paradigm. What particularly goads Feyerabend, however, is the fact that Kuhn defends „the rejection by a mature science of the uninhibited battle between alternatives ... not only as a *historical fact*, but also as a *reasonable move*."²³

One suspects that Feyerabend's emotional rejection of Kuhn's monism and of the associated concept of normal science and his peons of praise in favour of proliferation of competing points of view *throughout* the course of scientific development (and not simply during the times of crisis and revolution) are motivated by more than internal demands of a philosophical argument. (In fact the stance which he here adopts requires him to execute a number of logistic contortions in an attempt to reconcile the principle of tenacity with proliferation in permanence.) He passionately rejects what he regards as the anti-humanitarian element in Kuhn's theory: the conformism, stereotypeness and blind uniformity associated with subservience to the dictates of a single paradigm. Science *should not* be like this — therefore it *cannot* be so!

But whence comes the fury and passion of the rejection of this particular description of science? Out of an abstract contemplation of how terrible science would be if scientists actually did behave as Kuhn claims they do? It seems not. Feyerabend obviously detects such disturbing symptoms *in the current practice*, symptoms whose "anti-humanitarian" tendencies appear with increasing frequency in mature science. In fact he admits as much when, in a more uninhibited aside, he remarks that subjects such as physiology and parts of psychology are "far ahead of contemporary physics in that they manage the discussion of fundamentals as an essential part of even the most specific piece of research." In these more "advanced" subjects concepts are never completely stabilized or subjected to a dominance of a single theory and yet progress is in no way hampered by such "more philosophical" procedure. "Quite the contrary, we find a greater awareness of the limits of knowledge, of its connections with human nature [presumably one would expect that in subjects such as psychology and physiology!], we find also a greater familiarity with the history of the subject and the abil-

²² *Ibid.*, p. 205.

²³ *Ibid.*, pp. 201-2.

ity ... to *actively* use past ideas for the advancement of contemporary problems.”²⁴ How unlike the “constipated style of a ‘normal’ science”!

This latter unguarded remark reveals that, *volens volens*, normal science *does exist*, at least in some of the “less advanced” subjects such as physics. In fact Feyerabend’s treatment of normal science shows to what length one can go in denying the existence of some phenomenon which one dislikes and how grudging and incomplete one’s admission of its existence can be when forced to do so by the logic of one’s own argument or when in a moment of uncontrolled fury one cannot resist an outburst against its current practice. Thus we proceed from a “suspicion” that “normal or ‘mature’ science, as described by Kuhn, is not even a historical fact” to the “hope” that “normal periods, if they ever existed, cannot have lasted very long and cannot have extended over large fields either.” A few examples from the nineteenth century physics would “seem” to confirm this hope, although “of course” not everyone participated in the philosophical discourse between competing theories and “the great majority (*sic!*) (of scientists) may well have continued attending to their ‘tiny puzzles’.”²⁵

Feyerabend is openly contemptuous of that “unimaginative” majority, for in his view they were certainly not the people to generate material which might later serve as a revolutionary fuel. The incontrovertible proof is readily at hand: “the Presocratics progressed without paying the slightest attention to puzzles” (!) We then learn that where transition to mature science occurs, the “uninhibited proliferation and the universal criticism” of the pre-science era are supplemented by the “puzzle-solving tradition of normal science” or rather (such objective terminology irks Feyerabend) by the “more practical and less humanitarian tradition (which is) best exemplified by the attitude of the members of a closed society towards their basic myth.” Kuhn is therefore eventually given credit for having „discovered” normal science but is berated for singling out “the most boring and most pedestrian part of this scientific enterprise” for special prominence while missing entirely the “philosophical or critical component” of mature science.²⁶

Kuhn’s myopic-monic type of science is therefore never said to exist entirely on its own. Feyerabend’s brand of synthesis of Kuhn’s and Popper’s views leads him to assert that proliferation (associated by Kuhn with revolutionary science) and tenacity (associated with normal science) do not belong to successive periods in the history of science (as publicity hungry journalists would apparently make us believe!!) but are ever-present components of mature sciences. Not *all* the scientists are wearing professional blinkers — not even for a briefest period in history. An

²⁴ *Ibid.*, p. 199.

²⁵ *Ibid.*, *passim*, pp. 207-9.

²⁶ *Ibid.*, p. 212.

active and imaginative minority (which at one stage in the argument shrinks to the size of one scientist, albeit of a genius type, for "even a single man can revolutionize an epoch") is engaged in philosophical pluralistic argument which is most conducive of change and revolution and which most scientists would regard as lying outside science proper. The majority of scientists could support the latter view by pointing to their own lack of philosophical acumen but apparently it does not matter what these more hide-bound scientists think, for the "fundamental improvement" in science is not due to them but to those who further "active interaction" of the normal and philosophical components through the criticism of the "entrenched and unphilosophical by the peripheral and philosophical." Scientific education must therefore be based on the assumption that proliferation is "good for science." "Everyone may follow his inclinations and science, conceived as a critical enterprise, will profit from such an activity." ²⁷

Feyerabend's critique of Kuhn thus represents a good example of a case where a theoretical argument is made to serve an external and non-scientific cause. What we must reject here is the *method* adopted by Feyerabend to express his views on the social and educational effects of normal science — not necessarily the views themselves. Normal mature science is not what Feyerabend would wish it to be and it is wrong of him to pretend otherwise — but it may still be true that it would have been better for us all *if it were* such as Feyerabend would like to make it (and which might still exist in peripheral and "underdeveloped" sciences such as some parts of biology and the social sciences).

Feyerabend has a habit of asking rather unconventional questions in a paper devoted to the philosophy of science. He asks, for example, whether the picture of science which emerges from his analysis is an "attractive" one; whether it makes the pursuit of science "worthwhile"; and whether the presence among us of such a discipline as science is "beneficial to us or perhaps liable to corrupt our understanding and diminish our pleasure." ²⁸ Kuhn, who attempts to describe science *as it is* and is hardly concerned with whether it is beautiful or harmful, is duly shocked by the unpardonable bad taste in asking such "irrelevant" questions. As he claims with innocence, there is, after all, "nothing in (his) argument that sets the value of science itself." Although he therefore denies Feyerabend's claim that his description is liable to diminish human happiness and freedom or that it is, as Popper claims, "a danger... to our civilization", ²⁹ he can yet with full equanimity and unruffled complacency explain that there is nothing in his argument which

²⁷ *Ibid.*, passim, pp. 210-13.

²⁸ *Ibid.*, p. 209.

²⁹ K. Popper, *op. cit.*, p. 53.

depends on such a surmise being wrong. "To explain why an enterprise works is not to approve or disapprove it." ³⁰

Kuhn is here committing himself to a view within the field of the history and philosophy of science which is also held by the majority of scientists as applied to their own type of professional activity, namely that it is value-free. Feyerabend, who himself so openly violates this assumption, is not of course the only scholar to cast doubts on such claims. Gunnar Myrdal, for example, has recently reasserted his view that "No social research can be neutral and in that sense simply 'factual' and 'objective'. Valuations determine not only our policy conclusions but all our endeavours to establish the facts, from the approaches chosen to the presentation of our results. We can keep unaware of the valuations that *volens volens* are implicit, and this is unfortunately still regular practice in the social sciences. But by not in a rational manner selecting and making explicit the value premises that steer our research, we so provide a space of indeterminateness where biases can enter the analysis." ³¹

The extent to which Kuhn has been affected by such ideological biases is impossible to determine with any degree of certainty. Kuhn himself gives us no hint about the nature or origins of his values, but although never mentioned explicitly in his works, their influence cannot be underestimated either on the construction of the theory itself or on the way in which, *via* the theory, they affect the reader. Amsterdamski ³² in his comparative critique of the views of Kuhn and Lakatos draws a distinction between those models of scientific development which are purely descriptive and try to give a most accurate factual account of how science actually develops and the normative models which advocate a certain definite type of approach to the study of nature as the one most likely to ensure that development. Kuhn's theory (as that constructed by a historian of science) is placed in the first category while that of Lakatos, the methodologist, is in the second. That type of distinction, however, does not appear to be a very convincing one. In examining the effects of Kuhn's theory on education of scientists and on the development of social sciences Feyerabend certainly does not acknowledge the existence of any such distinctions but merely wonders to what

³⁰ T. Kuhn, "Reflections on My Critics", in: Lakatos and Musgrave, *op. cit.*, p. 237.

³¹ The Need for a Sociology and Psychology of Social Science and Scientists, opening address to British Sociological Association, York (England), 1972. See also Myrdal's paper "The Social Sciences and Their Impact on Society", in H. D. Stein (ed.), *Social Theory and Social Intervention*, The Press of Case Western Reserve University, Cleveland, 1968, pp. 145-62.

³² S. Amsterdamski, „Spór o koncepcję postępu w rozwoju nauki” (Controversy Concerning the Progress in the Development of Science), *Kwartalnik Historii Nauki i Techniki*, vol. 15, 1970, p. 487.

extent the objective consequences of the theory have been intended and/or foreseen by its author.

Such fuzziness of the boundary between the descriptive and normative aspects of models and theories in social sciences is being increasingly recognized. Suchodolski, for example, commented on the failure to make a sharp distinction in education between so called "educational sciences" (which were once thought to be concerned solely with description of educational realities) and "pedagogy" (which was thought to be concerned with proscription and particularly with formulation of goals and ideals which were to be followed in the education of the young). Suchodolski pointed out how in a complex cultural milieu in which educational processes took place at present "educational science" could only furnish useful information if material to be studied was to be subjected to a careful and purposeful selection which, in turn, was dependent on one's valuations and determination of the goals of the educational process. Thus it was found that "not only was it impossible to separate and transfer to another discipline the value-normative functions [of education], but that in their absence it was impossible to assure the appropriate performance of the descriptive function."³³

Nor is such special, almost symbiotic, relationship of the normative and descriptive functions a unique feature of education, although in this field such relationship is undoubtedly a very noticeable one. An Australian historian Hugh Stretton, for example, goes so far as to claim that valuations form an integral part of the work of *all* social scientists. In such a situation "it would be silly to criticise values for being present; instead it would be good to analyse their qualities and the manner in which they performed their indispensable work."³⁴ In other words, values can exert beneficial or injurious effect depending on the nature of the values themselves. Stretton would therefore argue that all models, whatever the intention of their constructors and whether formally descriptive or normative, are socially involved and that failure to admit this may lead one to overlook that the model one follows may purvey values which are highly questionable or even socially dangerous. Indeed, one must beware of the models which designed for description or discovery soon start giving social advice. "The modern model has its own method of turning into a programme — having failed in its task of modelling predictable regularities, it turns into a selector instead."³⁵

It is not suggested here, of course, that Kuhn's model of scientific development is in any sense a "failed" model. On the contrary, it does seem to fit the scientific enterprise as it exists today more closely than

³³ B. Suchodolski, *Trzy Pedagogiki* (The Three Pedagogies), Nasza Księgarnia, Warszawa, 1970, pp. 7-9.

³⁴ *The Political Sciences*, Routledge and Kegan Paul, London, 1969, p. 141.

³⁵ *Ibid.*, p. 255.

any other theory in the history and philosophy of science. It does not mean, however, that it can exert no detrimental effects or that Kuhn can be absolved from all responsibility for the social and educational consequences of his work on the grounds that his aim, as that of historian of science, was purely a scholarly descriptive one. As Kuhn himself now admits, his generalizations about science are much more than an objective description of reality: its prescriptive qualities justify a certain type of scientific education and certain type of approach to the training of research workers which, as was pointed out by Pippard, are defective in their high degree of scientific introversion and disregard of the world outside and in an over-emphasis on conformist adherence to rules and puzzle-solving according to a model solution.

A characteristic feature of this type of education is the heavy reliance on textbooks rather than on samples of research chosen from different sources according to a particular teacher's conception of his discipline. Kuhn explains away this state of affairs as a natural outcome of scientists' commitment to their paradigm: during periods of normal science scientists generally agree as to what every student in the field should know, or at least they agree to a very much greater extent than the sociologists, historians, or students of literature. Such high degree of professional unanimity on fundamentals of their discipline has undoubtedly many valuable functions such as an increase in efficiency which, for example, ensures that every young aspirant is properly trained before being allowed to engage in research on his own account. This type of educational practice, however, also perpetuates those aspects of science which are most authoritarian and which, according to Lakatos, are so much emphasised in Kuhn's work.³⁶ What is more, there are important features of real scientific life which our textbook conventions almost invariably miss. For example, as was pointed out by Pippard, most real problems of physics defy mathematical analysis. Thus "even when the relevant laws and boundary conditions can be written in elegant form... the necessary manipulations defeat us. (And) it is not that the problem is necessarily complicated in a physical sense; it is just mathematically intractable, a very different matter."³⁷ The techniques which are then generally employed in the laboratory involve the use of imaginative insight, a kind of structured leap into the unknown. In this respect the natural reaction of a gifted scientist is not so different from that of an arts man, yet those more *qualitative* aspects of the work of a scientist are largely ignored by the textbook writers who present their material in as deductive and mathematically rigorous form as possible.

One explanation for this particular feature of scientific education

³⁶ *Ibid.*, pp. 92-3.

³⁷ *Ibid.*, p. 6.

is our formal acceptance of Galileo's dictum that the book of the universe is written in the language of mathematics „withouth which it is humanly impossible to understand a single word of it." It has become the official philosophy of the scientific world that whatever exists in nature must be quantifiable and expressed in mathematical notation and although individual gifted scientists do not always obey such rules in their everyday work such notions have become institutionalized and frozen into scientific syllabuses and texts. It is the aim of scientists such as Pippard to unfreeze this rigid educational structure so that students can learn how to make use of their imagination and intuition. For example, there are always some apparently intractable but fascinating problems in science about which students should know how to speculate and dream, letting their unconscious mind roam freely so that when confronted later with the rational criticism of the consciousness they can select those ideas that seem most sensible, engage in some experimentation, read and observe... How far removed it all seems from Kuhn's world of normal science and the rigorous methods of initiation to the paradigm-prescribed rules and techniques which we now so uniformly adopt. Indeed, the only flight of intuitive feeling that Kuhn seems to allow to his normal scientist is the intuitive appreciation of what is possible (or rather permissible) within the confines of a paradigm.

The training which science students receive within current educational context is thus very much of the type which Kuhn envisages as the best method for transmission of paradigms to the next generation of scientists. Students which are subjected to this type of education, however, are at the same time inspired by their teachers to regard themselves as the free explorers of the uncharted territory of nature, abiding no dictates save those of nature itself. The result is an in-built conflict between the actual practice of science on the one hand and the rather vague scientific ideology on the other. This conflict is never fully resolved either in the lecture room or in the theory which Kuhn propounds. Thus although Kuhn gives us a much more accurate picture of current educational practice than does Pippard with his vision of the scientific world populated by highly gifted and imaginative students and teachers, his theory reflects a truncated and hence a *defective reality* which it thereby sanctifies and perpetuates.