Normal Science and its Dangers

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Professor Kuhn's criticism of my views about science is the most interesting one I have so far come across. There are, admittedly, some points, more or less important, where he misunderstands me or misinterprets me. For example, Kuhn quotes with disapproval a passage from the beginning of the first chapter of my book, *The Logic of Scientific Discovery*. Now I should like to quote a passage overlooked by Kuhn, from the Preface to the First Edition. (In the first edition the passage stood immediately before the passage quoted by Kuhn; later I inserted the Preface to the English Edition between these two passages.) While the brief passage quoted by Kuhn may, out of context, sound as if I had been quite unaware of the fact, stressed by Kuhn, that scientists necessarily develop their ideas within a definite theoretical framework, its immediate predecessor of 1934 almost sounds like an anticipation of this central point of Kuhn's.

After two mottos taken from Schlick and from Kant, my book begins with the following words: 'A scientist engaged in a piece of research, say in physics, can attack his problem straight away. He can go at once to the heart of the matter: that is, to the heart of an organized structure. For a structure of scientific doctrines is already in existence; and with it, a generally accepted problem-situation. This is why he may leave it to others to fit his contribution into the framework of scientific knowledge.' I then go on to say that the philosopher finds himself in a different position.

Now it seems pretty clear that the passage quoted describes the 'normal' situation of a scientist in a way very similar to Kuhn: there is an edifice, an organized structure of science which provides the scientist with a generally accepted problem-situation into which his own work can be fitted. This seems very similar to one of Kuhn's main points: that 'normal' science, as he calls it, or the 'normal' work of a scientist, presupposes an organized structure of assumptions, or a theory, or a research programme, needed by the community of scientists in order to discuss their work rationally.

The fact that Kuhn overlooked this point of agreement and that he fastened on what came immediately after, and what he thought was a point of disagreement, seems to me significant. It shows that one never reads or understands a book except with definite expectations in one's mind. This indeed may be regarded as one of the consequences of my thesis that *we approach everything in the light of a preconceived theory*. So also a book. As a consequence one is liable to pick out these things which one either likes or dislikes or which one wants for other reasons to find in the book; and so did Kuhn when reading my book.

Yet in spite of such minor points, Kuhn understands me very well—better, I think, than most critics of mine I know of; and his main criticism is very important.

This main criticism is, briefly, that I have completely overlooked what Kuhn calls 'normal' science, and that I have been exclusively engaged in describing what Kuhn calls 'extraordinary research', or 'extraordinary science'.

I think that the distinction between these two kinds of enterprise is perhaps not quite as sharp as Kuhn makes it; yet I am very ready to admit that I have at best been only dimly aware of this distinction; and further, that the distinction points out something that is of great importance.

This being so it is a minor matter, comparatively, whether or not Kuhn's terms 'normal' science and 'extraordinary science' are somewhat question begging, and (in Kuhn's sense) 'ideological'. I think that they are all this; but this does not diminish my feelings of indebtedness to Kuhn for pointing out the distinction, and for thus opening my eyes to a host of problems which previously I had not seen quite clearly.

'Normal' science, in Kuhn's sense, exists. It is the activity of the non-revolutionary, or more precisely, the not-too-critical professional: of the science student who accepts the ruling dogma of the day; who does not wish to challenge it; and 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. To resist a new fashion needs perhaps as much courage as was needed to bring it about.

You may say, perhaps, that in so describing Kuhn's 'normal' science, I am implicitly and surreptitiously criticizing him. I shall therefore state again that what Kuhn has described does exist, and that it must be taken into account by historians of science. That it is a phenomenon which I dislike (because I regard it as a danger to science) while he apparently does not dislike it (because he regards it as 'normal') is another question; admittedly, a very important one.

In my view the 'normal' scientist, as Kuhn describes him, is a person one ought to be sorry for. (According to Kuhn's views about the history of science, many great scientists must have been 'normal'; yet since I do not feel sorry for them, I do not think that Kuhn's views can be quite right.) The 'normal' scientist, in my view, has been taught badly. I

believe, and so do many others, that all teaching on the University level (and if possible below) should be training and encouragement in critical thinking. The 'normal' scientist, as described by Kuhn, has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination. He has learned a technique which can be applied without asking for the reason why (especially in quantum mechanics). As a consequence, he has become what may be called an *applied scientist*, in contradistinction to what I should call a *pure scientist*. He is, as Kuhn puts it, content to solve 'puzzles'.[1] The choice of this term seems to indicate that Kuhn wishes to stress that it is not a really fundamental problem which the 'normal' scientist is prepared to tackle: it is, rather, a routine problem, a problem of applying what one has learned: Kuhn describes it as a problem in which a dominant theory (which he calls a 'paradigm') is applied. The success of the 'normal' scientist consists, entirely, in showing that the ruling theory can be properly and satisfactorily applied in order to reach a solution of the puzzle in question.

Kuhn's description of the 'normal' scientist vividly reminds me of a conversation I had with my late friend, Philipp Frank, in 1933 or thereabouts. Frank at that time bitterly complained about the uncritical approach to science of the majority of his Engineering students. They merely wanted to 'know the facts'. Theories or hypotheses which were not 'generally accepted' but problematic, were unwanted: they made the students uneasy. These students wanted to know only those things, those facts, which they might apply with a good conscience, and without heart-searching.

I admit that this kind of attitude exists; and it exists not only among engineers, but among people trained as scientists. I can only say that I see a very great danger in it and in the possibility of its becoming normal (just as I see a great danger in the increase of specialization, which also is an undeniable historical fact): a danger to science and, indeed, to our civilization. And this shows why I regard Kuhn's emphasis on the existence of this kind of science as so important.

I believe, however, that Kuhn is mistaken when he suggests that what he calls 'normal' science is normal.

Of course, I should not dream of quarrelling about a term. But I wish to suggest that few, if any, scientists who are recorded by the history

[1] I do not know whether Kuhn's use of the term 'puzzle' has anything to do with Wittgenstein's use. Wittgenstein, of course, used it in connection with his thesis that there are *no genuine problems* in philosophy—only puzzles, that is to say, pseudo-problems connected with the improper use of language. However this may be, the use of the term 'puzzle' instead of 'problem' is certainly indicative of a wish to show that the problems so described are not very serious or very deep.

of science were 'normal' scientists in Kuhn's sense. In other words, I disagree with Kuhn both about some historical facts, and about what is characteristic for science.

Take as an example Charles Darwin *before* the publication of *The Origin of Species*. Even after this publication he was what might be described as a 'reluctant revolutionary', to use Professor Pearce Williams's beautiful description of Max Planck; before it he was hardly a revolutionary at all. There is nothing like a conscious revolutionary attitude in his description of *The Voyage of the Beagle*. But it is brim full of problems; of genuine, new and fundamental problems, and of ingenious conjectures—conjectures which often compete with each other—about possible solutions.

There can be hardly a less revolutionary science than descriptive botany. Yet the descriptive botanist is constantly faced with genuine and interesting problems: problems of distribution, problems of characteristic locations, problems of species or sub-species differentiation, problems like those of symbiosis, characteristic enemies, characteristic diseases, resistant strains, more or less fertile strains, and so on. Many of these descriptive problems force upon the botanist an experimental approach; and this leads on to plant physiology and thus to a theoretical and experimental (rather than purely 'descriptive') science. The various stages of these transitions merge almost imperceptibly, and genuine problems rather than 'puzzles' arise at every stage.

But perhaps Kuhn calls a 'puzzle' what I should call a 'problem'; and surely, we do not want to quarrel about words. So let me say something more general about Kuhn's typology of scientists.

Between Kuhn's 'normal scientist' and his 'extraordinary scientist' there are, I assert, many gradations; and there must be. Take Boltzmann: there are few greater scientists. But his greatness can hardly be said to consist in his having staged a major revolution for he was, to a considerable extent, a follower of Maxwell. But he was as far from a 'normal scientist' as anybody could be: he was a valiant fighter who resisted the ruling fashion of his day—a fashion which, incidentally, ruled only on the continent and had few adherents, at that time, in England.

I believe that Kuhn's idea of a typology of scientists and of scientific periods is important, but that it needs qualification. His schema of 'normal' periods, dominated by *one* ruling theory (a 'paradigm' in Kuhn's terminology) and followed by exceptional revolutions, seems to fit astronomy fairly well. But it does not fit, for example, the evolution of the theory of matter; or of the biological sciences since, say, Darwin and Pasteur. In connection with the problem of matter, more especially, we have had at least three dominant theories competing since antiquity: the continuity theories, the atomic theories, and those theories which tried to combine the two. In addition, we had for a time Mach's version of Berkeley—the theory that 'matter' was a metaphysical rather than a scientific concept: that there was no such thing as a physical theory of the structure of matter; and that the phenomenological theory of heat should become *the one paradigm* of all physical theories. (I am using here the word 'paradigm' in a sense slightly different from Kuhn's usage: to indicate not a *dominant theory*, but rather a *research programme*—a mode of explanation which is considered so satisfactory by some scientists that they demand its general acceptance.)

Although I find Kuhn's discovery of what he calls 'normal' science most important, I do not agree that the history of science supports his doctrine (essential for his theory of rational communication) that 'normally' we have *one* dominant theory—a 'paradigm'—in each scientific domain, and that the history of a science consists in a sequence of dominant theories, with intervening revolutionary periods of 'extraordinary' science; periods which he describes as if communication between scientists had broken down, owing to the absence of a dominant theory.

This picture of the history of science clashes with the facts as I see them. For there was, ever since antiquity, constant and fruitful discussion between the competing dominant theories of matter.

Now in his present paper, Kuhn seems to propose the thesis that the logic of science has little interest and no explanatory power for the historian of science.

It seems to me that coming from Kuhn this thesis is almost as paradoxical as the thesis 'I do not use hypotheses' was when it was pronounced in Newton's *Optics*. For as Newton used hypotheses, so Kuhn uses logic—not merely in order to argue, but precisely in the same sense in which I speak of the *Logic of Discovery*. He uses, however, a logic of discovery which in some points differs radically from mine: Kuhn's logic is the logic of *historical relativism*.

Let me first mention some points of agreement. I believe that science is essentially critical; that it consists of bold conjectures, controlled by criticism, and that it may, therefore, be described as revolutionary. But I have always stressed the need for some dogmatism: the dogmatic scientist has an important role to play. If we give in to criticism too easily, we shall never find out where the real power of our theories lies.

But this kind of dogmatism is not what Kuhn wants. He believes in the domination of a ruling dogma over considerable periods; and he does not believe that the method of science is, normally, that of bold conjectures and criticism.

What are his main arguments? They are not psychological or historical—they are logical: Kuhn suggests that the rationality of science presupposes the acceptance of a common framework. He suggests that rationality *depends* upon something like a common language and a common set of assumptions. He suggests that rational discussion, and rational criticism, is only possible if we have agreed on fundamentals.

This is a widely accepted and indeed a fashionable thesis: the thesis of *relativism*. And it is a *logical* thesis.

I regard the thesis as mistaken. I admit, of course, that it is much easier to discuss puzzles within an accepted common framework, and to be swept along by the tide of a new ruling fashion into a new framework, than to discuss fundamentals—that is, the very framework of our assumptions. But the relativistic thesis that the framework *cannot* be critically discussed is a thesis which *can* be critically discussed and which does not stand up to criticism.

I have dubbed this thesis *The Myth of the Framework*, and I have discussed it on various occasions. I regard it as a logical and philosophical mistake. (I remember that Kuhn does not like my usage of the word 'mistake'; but this dislike is merely part of his relativism.)

I should like just to indicate briefly why I am not a relativist:[1] I do believe in 'absolute' or 'objective' truth, in Tarski's sense (although I am, of course, not an 'absolutist' in the sense of thinking that I, or anybody else, has the truth in his pocket). I do not doubt that this is one of the points on which we are most deeply divided; and it is a logical point.

I do admit that at any moment we are prisoners caught in the framework of our theories; our expectations; our past experiences; our language. But we are prisoners in a Pickwickian sense: if we try, we can break out of our framework at any time. Admittedly, we shall find ourselves again in a framework, but it will be a better and roomier one; and we can at any moment break out of it again.

The central point is that a critical discussion and a comparison of the various frameworks is always possible. It is just a dogma—a dangerous dogma—that the different frameworks are like mutually untranslatable languages. The fact is that even totally different languages (like English and Hopi, or Chinese) are not untranslatable, and that there are many Hopis or Chinese who have learnt to master English very well.

The Myth of the Framework is, in our time, the central bulwark of irrationalism. My counter-thesis is that it simply exaggerates a difficulty

[1] See, for example, Chapter 10 of my *Conjectures and Refutations*, and the first *Addendum* to the 4th (1962) and later editions of volume ii of my *Open Society*.

into an impossibility. The difficulty of discussion between people brought up in different frameworks is to be admitted. But nothing is more fruitful than such a discussion; than the culture clash which has stimulated some of the greatest intellectual revolutions.

I admit that an intellectual revolution often looks like a religious conversion. A new insight may strike us like a flash of lightning. But this does not mean that we cannot evaluate, critically and rationally, our former views, in the light of new ones.

It would thus be simply false to say that the transition from Newton's theory of gravity to Einstein's is an irrational leap, and that the two are not rationally comparable. On the contrary, there are many points of contact (such as the role of Poisson's equation) and points of comparison: it follows from Einstein's theory that Newton's theory is an excellent approximation (except for planets or comets moving on elliptic orbits with considerable eccentricities).

Thus in science, as distinct from theology, a critical comparison of the competing theories, of the competing frameworks, is always possible. And the denial of this possibility is a mistake. In science (and only in science) can we say that we have made genuine progress: that we know more than we did before.

Thus the difference between Kuhn and myself goes back, fundamentally, to logic. And so does Kuhn's whole theory. To his proposal: 'Psychology rather than Logic of Discovery' we can answer: all your own arguments go back to the thesis that the scientist is *logically forced* to accept a framework, since no rational discussion is possible between frameworks. This is a logical thesis—even though it is mistaken.

Indeed, as I have explained elsewhere, 'scientific knowledge' may be regarded as subjectless.[1] It may be regarded as a system of theories on which we work as do masons on a cathedral. The aim is to find theories which, in the light of critical discussion, get nearer to the truth. Thus the aim is the increase of the truth-content of our theories (which, as I have shown,[2] can be achieved only by increasing their content).

I cannot conclude without pointing out that to me the idea of turning for enlightenment concerning the aims of science, and its possible progress, to sociology or to psychology (or, as Pearce Williams recommends, to the history of science) is surprising and disappointing.

In fact, compared with physics, sociology and psychology are riddled

[1] See now my lecture 'Epistemology Without a Knowing Subject' in *Proceedings of the Third International Congress for Logic, Methodology and Philosophy of Science*, Amsterdam, 1968.

[2] See my paper 'A Theorem on Truth-Content' in the Feigl Festschrift *Mind, Matter, and Method*, edited by P. K. Feyerabend and Grover Maxwell, 1966.

with fashions, and with uncontrolled dogmas. The suggestion that we can find anything here like 'objective, pure description' is clearly mistaken. Besides, how can the regress to these often spurious sciences help us in this particular difficulty? Is it not sociological (or psychological, or historical) *science* to which you want to appeal in order to decide what amounts to the question 'What is *science*?' or 'What is, in fact, normal in science?' For clearly you do not want to appeal to the sociological (or psychological or historical) lunatic fringe? And whom do you want to consult: the 'normal' sociologist (or psychologist, or historian) or the 'extraordinary' one?

This is why I regard the idea of turning to sociology or psychology as surprising. I regard it as disappointing because it shows that all I have said before against sociologistic and psychologistic tendencies and ways, especially in history, was in vain.

No, this is not the way, as mere logic can show; and thus the answer to Kuhn's question 'Logic of Discovery or Psychology of Research?' is that while the Logic of Discovery has little to learn from the Psychology of Research, the latter has much to learn from the former.

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