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REVOLUTIONS IN SCIENCE, OCCASIONAL OR PERMANENT?

I

There are three views in the literature on science concerning the nature of scientific revolutions, and a few variants and combinations of them. The most important view is the radicalist view which was expressed by Sir Francis Bacon very forcefully, which was traditional since the foundation of the Royal Society and until the Einsteinian revolution in physics, and which is still believed by many philosophers and historians of science, as well as by many scientists, natural and social. It claims that science is born out of a revolution against prejudice and superstition but that within science itself every part is so securely founded that it cannot be shaken. The most important alternative to Bacon's view is Pierre Duhem's theory of continuity; it was born out of the crisis in physics, and it is becoming increasingly popular amongst the sophisticated. It claims that every achievement of science is capable of modification, but not of overthrow. For instance, we may believe in determinism and then overthrow it, thereby exhibiting its unscientific (metaphysical) character; but we may only modify, not overthrow, Maxwell's theory — say, by viewing its equations not as precise to the last point but as mere averages. The third view was developed by Sir Karl Popper after the Einsteinian revolution and under its impact; though Einstein and a few others have accepted it, at least in part, those who have heard of it usually consider it rather eccentric. It claims that unless a theory can be overthrown by empirical evidence it is unscientific, and *vice versa*. For example, determinism cannot be overthrown, but a scientific theory which may be deterministic or indeterministic such as Newton's theory and Heisenberg's theory respectively, can be overthrown. Bacon's theory is of one revolution, Duhem's theory is of no revolution but merely reforms, and Popper's theory is of revolution in permanence.

Let us now introduce a totally different point — the fear of losing touch with one's colleagues. This point is very closely related to the above theories of scientific revolutions in the following way. These theories share a progressivist attitude towards science. And one unpleasant consequence of progressivism for the individual is that he might one day find himself left behind.

There is little or no literature on this subject, and so one has to refer to some results of field-work, however partial and cursory (not to say impressionistic). Such results indicate that the fear is very widespread, and is based on the widespread theory that with the ever-growing growth-rate of science it is very easy to lose touch with the forefront of science. One can lose touch through neglect (even a temporary and fully justifiable one, such as brief illness), through losing one's intellectual sharpness or one's creative powers (as Freud constantly feared), or through developing a mentally rigid allegiance to theories and methods which were indeed significant in one's early days but which quickly became outdated (*i.e.*, much less significant than they used to be). One can become old-fashioned in one's beliefs simply from being ignorant of, or even unable to understand any longer, the newest methods, ideas, and experiments in the field. In such a case it is futile to feign agreement with the more up-to-date researchers, because one cannot really believe what one cannot understand. It is clear that most old-fashioned thinkers are out of touch, since if they were in touch with the latest developments they would in all likelihood see as clearly as any up-to-date person does, what the facts of the situation are. (This is not entirely universal: a person may become so dogmatic in his attachment to fashions accepted in his youth that he would not agree with his colleagues even when he knows all the facts at hand and understands all the newest ideas).

That such fears are deeply related to progressivism is almost too obvious to note. It may even be empirically illustrated by pointing at the similar fears which progressive artists harbor, and at some non-progressive cultures which show no trace of it. (The only way for a traditional Rabbi, for instance, to lose touch, is to become literally senile). Now, being progressivist, the three views about science and revolutions both leave room for the fear of losing touch, and provide prescriptions as to how to avoid losing touch and becoming old-fashioned. The three prescriptions diverge, and discussing their relative merit is one mode of critically assessing the relative merit of the views which give rise to them. We shall examine views of scientific revolutions, about loss of touch, and the connection between them. If we provide clear-cut prescriptions we shall thereby design some crucial tests between the various theories.

II

The commonest modern view about science is that in science we can prove what we believe. This view is less common amongst men of science since the Einsteinian revolution, but not much; amongst historians of science, vulgarizers of science, and educated laymen, it is still as popular as ever. One instance may deserve mention here. The Italian philosopher of science Ludovico Geymonat published in 1957 a book by the title *Galileo Galilei* and the sub-title "A biography and inquiry into his philosophy of science". The book was translated by Stillman Drake and published in 1965 with a foreword by Giorgio de Santillana and notes by the translator. Professor Geymonat's thesis is that Galileo is so very important a figure in the history of science and of philosophy because he made the discovery of a great truth about scientific method, perhaps the great truth about it: it is not enough to show some thesis to be very highly probable; as long as a thesis is not utterly and completely proven, it should be viewed as unscientific. One corollary from this great truth is that one should rely only on the first hand testimony of one's own senses, not on witnesses.

This theory about the requirement of absolute demonstration in science makes a scientist "guilty of... (an) unpardonable mistake", to use words of Sir John Herschel, when he permits himself to say something which might be a mistake. As a scientist, better say nothing at all than say something which may turn out to be an error. This doctrine renders the life of a scientist a nightmare. After he has done his best to find the truth, and committed himself to a view after he was as satisfied with its demonstration as was possible, he may find the slightest need for readjustment the greatest burden; for the need for readjustment shows faults in one's scientific past, in one's scientific education, in one's teachers of science. The slightest need for a modification thus becomes a matter of the highest principle. The slightest criticism thus becomes identical with the most sweeping condemnation.

So Sir Francis Bacon understood the situation. If proof in science is so easy, asked penetrating Bacon, why did we live in the dark ages for so long? Because, he answered, people would rather distort every fact they observe than admit that they had erred. Hence, if you want to be a scientist make a clean slate and proceed cautiously. If you make a guess, as Copernicus did, chances are you are building a new dark age. Bacon's theory is the theory of one and only one revolution in science: the revolution of science against error. Hence, science begins with the last revolution. Physics, say Bacon's followers, begins with the seventeenth century, chemistry with late eighteenth century, and optics with the early nineteenth century. The radicalist must view the latest revolution as the starting point, as has been stressed so beautifully and forcefully by Michael Oakshott in his *Rationalism in Politics*. Indeed,

as Lakatos has pointed out, when Russell was a radicalist in mathematics, he vacillated between viewing George Boole or his own self as the father of mathematics proper. Holding to the view that there can be no revolution in science, but only of science against error and prejudice, Lavoisier and his followers concluded that all pre-Lavoisierian chemistry had been superstitious, and Madame Lavoisier burnt ceremonially the books of Stahl — her husband's most distinguished predecessor. This act reflects the sentiments of many historians of science, including even such a liberal historian as Herbert Butterfield. Though Butterfield rejects the view that a scientific theory needs no modification, since he endorses the view that there was only one revolution of science against prejudice, he cannot but view the revolution in chemistry a latecomer to the scientific revolution.

Lavoisier's view is not fully endorsed by Butterfield and other historians and philosophers of science. Lavoisier followed Bacon (and Newton) in thinking that there are no revolutions in science because science proves its theories beyond doubt. Nowadays we all view Newton's theory as scientific, although we do not believe in it, rejecting the invariance of mass and action-at-a-distance. Nowadays, likewise, we do not burn Lavoisier's books even though we do not share his chemical views (to say nothing of his physics). He divided chemicals into active and inert; the active ones he divided into acids, which contain oxygen (= "acid maker") and alkalies which are without oxygen; the inert chemicals he viewed as salts combined of acids and alkalines. We think nowadays of this theory as exceptionally naive, just as we think of his view that all processes of combustion, fermentation, calcination, and acidulation, are nothing more than the combination of chemicals with oxygen (which was previously combined with caloric). Nowadays, in brief, we are somewhat more tolerant towards our predecessors, and do not call them prejudiced and superstitious as soon as we discover errors in their teachings. Should we not, likewise, cease viewing as superstitious and prejudiced Lavoisier's phlogitonist predecessors, and even perhaps Galileo's mediaeval predecessors? This is the question which engages many contemporary historians of science.

The philosopher who introduced the idea that mediaeval science is not superstitious did so by arguing that all science is always alterable. He was Pierre Duhem, a strange amalgam of a most daring and revolutionary philosopher with a most reactionary one. How revolutionary it was at his time to consider Newtonianism alterable is hard to imagine. The greatest skeptical philosopher of modern times, David Hume, believed Newtonianism would go down to the end of the ages utterly unaltered. And since the days of Hume, more and more impressive evidence in favor of Newtonianism kept coming forth. Faraday thought Newton's theory must be modified to eliminate action at a distance,

but this fact had been forgotten, and rediscovered only recently. In the latest biographies of Faraday this fact is still overlooked. Poincaré considered the possibility of having to modify Newtonianism, and argued that it would be always preferable to adhere to it even at the cost of altering the meanings of its terms so as to keep it in accord with the facts. Pierre Duhem attacked this argument and declared that Newtonianism too is not sacrosanct. But for the fact that Einstein outdid Duhem the very same time (by actually offering an alternative to Newtonianism), his position in the history of thought might have become most prominent. Duhem, however, was a reactionary; his chief purpose was to argue that mediaeval science is qualitatively no different than modern science, and that those who preached otherwise, notably Galileo, suffered from a touch of megalomania or of incredibly naive optimism about what science (and they as scientists) can achieve. Butterfield has combined the radicalist view of science as anti-mediaevalist with the reformist view of science as open to modification at any time. He has achieved this partly by failing to refer to Duhem, partly by writing so very clearly and beautifully. He simply asserted that in the Middle Ages not even the slightest modifications were allowed, whereas in the Renaissance and later, modifications were welcome. This thesis he has not discussed rationally; he has not refuted, for example, Duhem's stories of modifications in mediaeval physics. His view, however, fell on fertile ground and became very popular because it is simply so very seductively quietist. After Einstein it is ridiculous to claim that anything in science is the last word; yet this is made to sound so much less disquieting than it first sounds, after you declare that though not the last word, present day theories are not going to be radically different from tomorrow's. This gives a desired and comfortable continuity to the history of science. The continuity becomes less comfortable when it extends to the distant past, beyond Newton and Galileo to Galileo's opponents and their predecessors. So Butterfield simply cuts the line when it becomes uncomfortable.

Thomas Kuhn's philosophy is a further variant on Duhem's. He endorses a continuity theory of Duhem, and with Butterfield he rejects Duhem's view of the Middle Ages as scientific. But he has a modification of Duhem's view which justifies his deviation from Duhem about the Middle Ages. Though science constantly alters, says Kuhn, it has discrete levels recognizable as the discrete standard text-books of the different periods. The continuity is provided both in the formation and in the dissolution of each text-book. The Middle Ages, however, had no science text-book to speak of. The astronomy text-book was ancient, and it had been dissolved to a sufficient extent before Copernicus came; the astronomy text-book, in other words, was much too dated. Other fields, chemistry for one, had no textbook at all.

The Duhem-Butterfield-Kuhn continuity theory of the history of science which views all modifications in science as small, is *prima facie* in conflict with the facts of the recent revolutions in science, whether in genetics, in relativity, or in quantum theory. Indeed, Duhem viewed the revolution in physics as utterly unscientific. However, there is some progress from the nineteenth-century theory of science according to which a genuinely scientific theory is in need of no modification whatsoever, to the continuity theory of science, which allows at least minor modifications.

One way to test a theory, it has been argued above, is to see how its applications look. Let us see how we may apply the view that genuinely scientific theories are modifiable.

III

Jonathan Swift once wrote a note to remind himself when old, what old men are prone to do which young men do not particularly enjoy, so as to prevent himself from being an old pest. It is hard to say whether we can address ourselves when old: we may by then change our minds far enough and think that we know better when old than when young, thus rejecting the advice of our young selves. Sometimes, quite correctly. For example, when we get older we may tend to become less ambitious and thus acquire a better sense of proportion, not to say become more clear-sighted. For instance, in time we may learn to feel somewhat indifferent to the question, do young people like us or not. Also, we may erroneously find it more important to improve their conduct or abilities even if they are ungrateful. In some instances it is obvious that older people deteriorate. For example, when we get older we may desperately hold to our achievements, feeling too old to have never ones and fearing that if our past achievements are all insignificant we shall face empty lives with no ability to do anything to improve matters.

This grim possibility is the one which Max Planck saw as the common situation. Though he was one of the most distinguished scientists of the century when he wrote his scientific autobiography, this work is candidly bitter and full of a sense of disappointment at his fellow scientists. Surely, here we have a striking fact. Someone has explained this fact by reference to Planck's bitter life as a German nationalist, as a German citizen, and as the father of a victim of the Nazis. Planck's life was indeed far from enviable, yet to view his bitterness against the world of science as the mere reflection of his bitter life and thus to dismiss his complaint is, again, mere quietism.

What Planck narrates is that his teachers, Kirchhof and Helmholtz, were unappreciative of his work. Everyone was so unappreciative of

him to begin with, that he got his first academic position through family connections. Even later, when he became known, none of his ideas was accepted on his own arguments, for the reasons that he had initially advanced it. He says, in a most striking and well-known passage, that science progresses not because its old leaders change their minds but because they die, leaving the field to the young newcomers who look at the situation afresh merely because they can do nothing else. That Planck's picture is misleading is beyond doubt; even though his facts are largely true, he omits the facts which do not fit his grim view of the world of learning and of his own place in it. He probably did get his first job because of the help of a family friend, but doubtlessly he became the secretary of the Prussian physical society for different reasons. As he fails to mention the fact in his scientific autobiography one cannot know what his view of it was. He likewise fails to mention that Lord Rayleigh referred to his radiation law as soon as was possible, that his papers to the Prussian society were regularly reported, for instance, in the news column of the "Journal de Science Pure et Appliquée", that his *Treatise* was translated into English early in the century whereas similar continental works are still untranslated. He mentions that all the leading scientists he met before he was a celebrity ignored him, mostly dogmatically, and in the case of Boltzmann, even somewhat viciously. Boltzmann did later become friendly but, according to Planck, only after Planck had endorsed some of his views. Planck glosses over the details of his rise to fame; all he has to say is, his ideas were accepted for reasons other than his own. Why were his ideas accepted for different reasons, and why should this have spoiled his fun? He is reticent on these points. Obviously, the points he is reticent about are such that might not gain his readers' sympathies: his readers, too, might accept his ideas for reasons other than his. But why should this be so unpleasant? Perhaps this is a symptom of a serious ambivalence which Planck suffered from when writing his own scientific autobiography: he had effected a revolution which he did not like at all: he was rejected by his elders as a rebel, and by his own followers as an old conservative. He could not accuse himself of selfish conservatism because his own ideas were accepted and for selfish reasons he would have to join the younger generation rather than keep aloof from them. He was an unselfish conservative and so he felt he was right. Which comes to show how many ways there are to be mistaken.

IV

What makes a scientist conservative? Planck's answer, the overestimate of one's own contribution to science, does not apply to Planck himself, yet we judge him a conservative. The theory of selfishness

which Planck implicitly proposes is thus not universally true. Priestley already refers to this theory and shows his own behaviour to be a refutation of it. Richard Kirwan's fame, says Priestley, was increased, not diminished, by his conversion from phlogistonism to antiphlogistonism. Hence, selfish motives should entice Priestley to convert as well. But, says Priestley, he cannot honestly endorse views so revolutionary and so poorly based on evidence, and he cannot see the complete overthrow rather than the mere modification of a view which only a generation earlier was considered by all scientists as the best established and the greatest achievement since Newton's.

Here is a very strong argument for the conservative cause against a revolution, which everyone will recognize and accept unless he is a hopeless opportunist: a revolution against science is one which we all have to oppose. But what is a revolution against science? Even the most anti-scientific revolution in modern history was not declared as anti-scientific but rather anti-Jewish. Lenard, a scientist respected before and after the holocaust, was engaged at the time writing a book against Jewish science (Einstein), and for true science, namely for German science. Now, if the German hooligans did not say openly that their revolution was against science then no other anti-scientific revolution has to; yet we must find out whether the revolution is not anti-scientific, so as to oppose it, if need be. Planck was doubtlessly anti-Nazi, yet being a historicist and a German nationalist he deceived himself that what later proved to be a catastrophe of the first order was a mere aberration, a passing phase. Priestley, to take the opposite extreme, saw with horror Lavoisier's book-burning — which is surely anti-scientific — and he consequently opposed much too strongly everything related to the Lavoisierian revolution in chemistry. Similarly, Planck and Einstein exaggerated the irrational element of the revolution in quantum theory, namely the subjectivism and positivism of Heisenberg, as well as the obscurity and shiftiness of Niels Bohr. These events and a little reflection may show us that it is not so easy to avoid being a conservative: we all want to conserve something, at least our progressive philosophy *etc.*, and whether giving up this or that and swimming with the current hither or thither is progressive or opportunistic who knows.

We are all told with horror about the way the Mozarts and the Schuberts of the past were let to die in loneliness and misery; this makes us willing to be as appreciative of and generous towards all innovators; but, in the midst of all the tolerance and willingness to appreciate, even the last generation has ignored some of the great artists of its day according to present day judgment. This is obviously much less the case today than yesterday, and much less current in science than in the arts. This may be explained by the existence of

better standards of excellence in science than in the arts which permit a wider range of toleration and a clearer view of what is impossible. But the standards are neither perfect nor absolutely universal, and this accounts for the errors of serious men of science concerning their attitudes towards scientific or allegedly scientific innovation.

There is little doubt that the standards of science cannot be perfect: the disagreements concerning them and changes of them through the ages are sufficient testimony even for those who would not accept the general view of the imperfection of man. Yet somehow we fail to see that such standards may lead to conservatism on the one hand and to opportunism on the other. So many people, especially historians of science, accept as scientific and thus as ever-lasting, any idea on which the scientists are agreed. Even philosophers of science often say so almost explicitly. Herbert Feigl, in his essay in honor of Karl Popper, says Popper must agree that the law of conservation of energy is well founded since for over a century no scientist has doubted it — with the single and very ephemeral exception of the famous paper by Bohr, Kramers, and Slater. This remark is particularly amazing since Poincaré has shown in *Science and Hypothesis* that the law of conservation of energy cannot be supported by experience just as no conceivable evidence can lead to its rejection. Now, if Feigl can today commit such an error after it has been shown to be an error, why could not men of science have committed the same error long ago?

There is more to it. Whatever the canons of science are, it has always been agreed since Galileo, Bacon, and Boyle has insisted on it and since it became the standard of the Royal Society three centuries ago, that clarity is the hallmark of science. Obscurity is condemned as one of the greatest violations of the canons of science. That Bohr was obscure, however, no one ever denied, least of all Bohr himself. Yet, whereas Bohr was merely worried about his obscurity and merely tried to do his desperate best to clarify his view, certain physicists reacted much more radically than Bohr. Paul Ehrenfest was doubtlessly much disturbed by the problem whether his opposition to Bohr was not as old-fashioned as the run-of-the-mill opposition to Einstein's relativity was. Niels Bohr, in his classical report on his debates with Einstein, refers to Ehrenfest's remarks to this effect as teasings of friends; Einstein, in his (much earlier) obituary notice on Ehrenfest, describes him as a depressive self-doubter who could commit suicide because of such a doubt. Einstein says clearly that indeed the primary cause for Ehrenfest's suicide was his doubt whether his opposition to Bohr was not old-fashioned. The variance between Bohr's story and Einstein's is formidable. This should make us all see how serious and how involved the problem is: even the problem whether the problem at hand is relevant to the suicide of Ehrenfest is too difficult to solve without,

at least, much study and deliberation, including the study of the testimonies of Einstein and Bohr.

Yet, hard as the problem is, it is obvious that certain past solutions to it were mistaken, and the mistakes need not be repeated. For instance, the idea is erroneous that Einstein was against science because he proposed to modify Newtonianism though Newtonianism had been so strongly verified by experience. Even the most strongly supported view may be in need of improvement. Joseph Priestley, we saw, was willing to consider a well-verified theory modified one way or another, and he himself studied a number of modifications, some of which were of his own invention, before he settled for Cavendish's modification. But he could not settle for an overthrow of an established theory. Those who agree with Priestley in principle, must deny either that Phlogistonism had been well-established or that Lavoisier's theory was a break from phlogistonism. Indeed, already H el ene Metzger, Duhem's chief disciple, opted for the second alternative. James B. Conant, another disciple of Duhem, and Kuhn's teacher, settles for a compromise between the two. Some of phlogistonism looks to him not too scientific, some of it looks to him a close approximation to Lavoisier's theory.

The case of the Einsteinian revolution as seen by the continuity theorists is not different. Duhem allowed modifications of Newtonianism, but not as drastic as those Einstein proposed. He dismissed Einstein as anti-scientific. Whittaker, on the other hand, invested much effort in presenting relativity as a natural development in small steps from some nineteenth-century studies.

Exercises like these are very legitimate and partly even interesting, yet the cost of taking them seriously is the readiness to give up hope of rendering the continuity theory applicable to practical problems such as Priestley's, let alone Ehrenfest's. Though the continuity theory (in all its versions) applies against Einstein's opponents who forbade any modification of Newtonianism, the continuity theory is not applicable to the opposition to some modification, but not to all. For, if we cannot know from sheer appearances whether a doctrine was scientific to begin with, and whether a modification to a scientific theory is small enough to be acceptable, then we may just give up hope of providing workable criteria. Popper's theory, conversely, does not oblige us to defend any theory against any modification, no matter how well-supported the theory was or how radical the modification proposed. Is this approach not too radicalist?

V

It is no doubt the case that whether one is progressive or old-fashioned much depends on one's beliefs; yet though most people think so, it is a mistake to identify being old-fashioned with believing out-

dated theories or being progressive with believing every day the theories of that day (or the next). This popular error is particularly hard to eradicate because it leads to distorted history, and distorted history provides ample evidence in its favor. Thus, when someone was progressive but held old-fashioned beliefs our historians gloss over his beliefs, etc.

Newtonian physics ousted Cartesian physics, and those who advocated Cartesian physics after the publication of Newton's *Principia* are condemned in many history of science text-books as old-fashioned; naturally, you will not expect these text-books to contain the information that Newton himself was a Cartesian, as Euler was, and that even Laplace had a strong Cartesian tendency; yet this information is true. To say that Euler was not progressive because he held old-fashioned beliefs is preposterous.

Equally preposterous it is to praise scientists who jumped on the band-wagon of a new school without understanding it sufficiently to have left the old school or even while consciously trying to compromise between the two.

Helmholtz is praised for having held the theory of conservation of energy. Actually, he first advocated the view of the conservation of force, and not as a pioneer but as a compromiser between the old and the new. He said that Newton's third law assures us that the sum of all forces at any time is zero so that the law of conservation of force is quite legitimate. When he realized that this idea led to the construction of fields of force in empty space he first rejected it as mad and then accepted it either on a model of the ether or as a pure mathematical construction devoid of all physical meaning. It is clear that Helmholtz was old-fashioned in physics (though not in physiology and psychology) yet he joined the right band-wagon and even made contributions to the field.

Who cares much about the fact that A. H. Lorentz could never believe in relativity? He was one of the best relativists of his day, his own beliefs notwithstanding. Conversely, who cares that Kelvin joined the thermodynamicist school in the nick of time, just before it won? His contributions to the field until then bore little or no significance to the dispute.

All this comes to show that the problem, whom should we believe, is a misplaced problem or a misstatement of a genuine problem. Let us go to the arts again. The problem there is not of truth but of beauty. Now beauty has to be enjoyed, and so the problem who is today's Mozart, or Schubert, can be translated into, whose work should I enjoy? But the real question is not as subjective; it is, whom should I appreciate? Appreciation is both more objective than enjoyment and of a wider compass: we can explain our appreciation and discuss it criti-

cally, we can appreciate without enjoyment, we can appreciate even without seeing beauty: think of all the influential artists — painters, composers, and authors — who were in their own days artists' artists and then sank into oblivion; think of the geniuses who influenced posterity and whose works are devoid of all beauty, such as Wagner; think of dadaism, whose immense impact did not save it from oblivion as it has nothing interesting to offer us any more; not a single exciting poem, not a single interesting canvas. And now back to science.

The analogue is clear: it does not matter what one believes is true but what one considers important or interesting — what one appreciates. Make the following experiment: look for an old-fashioned thinker who gets along well with the young, and look for the old fogey who only follows the young. You will easily observe that usually, the old-fashioned person whom the young appreciate is one who understands them, rather than agrees with them; who can expound their interests. The old fogey tries hard to agree with the young, yet they view him as merely ridiculous. This paragraph contains enough material for a few suggestions of experiments which the interested may perform.

VI

The idea suggested here is that we avoid being old-fashioned, no matter what we believe, by being able to understand the interests of the young; but to make it fit the phenomena closely or not at all so as to make it applicable we must specify who is familiar with the interests of the young and how such a familiarity can be acquired.

To this the answer offered here is this. He who is familiar with your problems, and can to some extent explain their significance to you, can claim that he knows what your interests are. There are some striking instances of older people who were able to understand the problems which beset the younger generation and thus be active in the progress of learning even though their own major preoccupations lay elsewhere. The case of Niels Bohr is perhaps a famous contemporary case. Another case, more impressive but virtually unknown, is that of Joseph Priestley, the arch-conservative in the whole history of modern science. The facility with which he could move from one theoretical system to another, compare and contrast them, and examine their limitations, is a source of immense pleasure to all his readers (few as these are). He understood the problems of his opponents all too well, even though he was a bit too dogmatic in his conviction that these were insurmountable. Because of his religious and political heresies, the mob of Birmingham was provoked into burning his house. He fled to London, but because of his philosophical heresies, he found no friend there. He went to Pennsylvania and died there in almost total desolation. Almost; for

he made friends with Humphrey Davy, a daring young upstart who rose to relative fame from a rather humble walk of life. Priestley understood Davy very well, encouraged him and advised him, helped him in preparing the overthrow of Lavoisier's doctrines. In his *Elements of Chemical Philosophy* Davy speaks of Priestley with exceptional warmth, commending him particularly for his openmindedness and readiness to alter his view at a drop of a test-tube.

Davy was a revolutionary scientist, a rebel, an aspirant. When he refuted the doctrine of Lavoisier by extracting oxygen from alkalis, his success in finding a publisher for his discoveries led to threats (by Poisson, no less!) to call the police. Even on his triumphant tour to the Continent of Europe he continued to destroy accepted views, including his own! (He thought that only oxygen and chlorine can oxydize, and so suspected iodine of being a chlorine-compound but soon destroyed his own suspicion). He never accepted Dalton's views, but this did not in the least disturb his researches: he understood Dalton well enough to be able to use his ideas and he even improved upon Dalton's experiments of weighing gases. Yet his unwillingness to believe Dalton was a source of vexation to his and Dalton's mutual friends, who therefore decided to have it out with him. The story is told by Thomas Thomson and it is very human and very funny, but it has little or nothing to do with the problem of atomism which these people studied.

Davy had no difficulty in understanding Faraday's opposition to Dalton: in this respect Faraday was a close follower of Davy. But Davy could not understand Faraday's interest in Oersted's circular forces, and soon he lost touch with his closest friend and disciple. He opposed his candidacy to the Royal Society allegedly on personal grounds (Faraday was suspected of plagiarism), but really from a loss of touch. Faraday's problems meant nothing to him from 1821 to his death in 1829 because in that period Faraday was struggling with new problems which most scientists could not share with him.

VII

It may be doubted that the view offered here is specific enough. Suppose it happens that he who shares the problems of the young avoids being a reactionary regardless of his own beliefs. Can we say that anyway he who shares the young one's interests also shares their beliefs, so that finally the view offered here amounts pretty much to the received opinion?

Without much disquisition, one can push the difference by discussing a further stage in the practical problem: suppose you do not know how to make yourself believe what you don't believe, nor how to be interested in what you find so utterly uninteresting. To declare that

you do agree with the young ones, or that you find their work so very interesting, merely in order to be on the right side, is opportunism and folly — quite apart from the fact that even all the young scientists together may be barking up the wrong tree. What you can do, is try to find out why the young ones are interested in whatever it is. It may turn out that they do bark up the wrong tree, or that they do have a genuinely important interest which they somehow fail to state clearly and correctly! If this kind of a discovery will be of value, then surely this will show the superiority of interest over belief.

But how does one go about interests? Interests are presentable in terms of problems and of the assessment of their relative significance. We have to explain this and provide an instance.

When all the scientists around begin to show concern with models of the ether, one may ignore this interest in an old-fashioned way or in a hyper-modern way; how do we know which is which? The answer is simple: there is a problem behind the interest in the ether; those who ignore the interest in the ether and the problem as well, may be losing touch; not so those who can address the problem while declaring that the ether does not exist — as Faraday did. Nobody can call Faraday old-fashioned because he did not join the new fashion of looking for models of the ether, because he knew the reason for the search and found an alternative way of taking account of it.

This story shows that the chief aspect of the current interest need not connect at all with current beliefs but may connect with problems.

Hence, according to the present proposal, if one concerns oneself with current problems one does not lose touch even when one is very old-fashioned. For another example we may take Priestley who was well aware of the problems of his opponents and thus was always in the frontiers of science, quoted by the best students of chemistry until his very death.

But what if the problems of the young ones look to you so very trite and uninteresting? The answer to this may be, try to solve the problem, why do all the young members of my profession concern themselves with a dull problem? Doing this you may either find out where your mistake lay and thus save your skin, or where the error of your profession lies and save your profession. These things are not too likely but they do happen on occasion, and interests of very few individuals do sometimes turn out to become the interests of the whole profession in the matter of one generation or less.

To conclude, Popper's theory of science as a critical debate with empirical criticisms enables us to offer clear-cut recommendations for keeping abreast and so it can be further examined by observations and experiments. The continuity theories of science as always permitting reforms but never revolutions, either offer no clear-cut recommenda-

tions, or clearcut recommendations which ought clearly to be rejected. The radicalist theory of science as the utter overthrow of all that is unstable and thus becoming utterly stable, offers a clear-cut recommendation which evidently ought to be rejected. As to the problem itself, as to the desire to keep abreast, it concerns so many scientists possibly because their view of science and its progress is rather nebulous, and their anxiety is merely an expression of their bewilderment. Because it concerns many, it has been discussed here; whether it should concern anybody is a different matter altogether. Perhaps it is preferable to concern oneself with interesting scientific problems than with one's own place in science. As long as one is interested in problems and is intrigued by them, one need not bother so much about the judgment of posterity. But perhaps this is merely an alternative formulation of the above proposal to keep abreast by studying contemporary problems: if we bother about an interesting problem, either it is a current problem, or we may render it a current problem by our studies. It was Faraday already who considered, amongst other kinds of contributions to science, the announcement of problems to be solved.