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THE PROBLEM OF KNOWLEDGE IN HISTORICAL PERSPECTIVE

From Jerusalem to Delphi is a very long way. It was so two and a half millennia ago, is it nearer today? Seemingly yes - the Hebrew tradition turned into the Judæo-Christian world-view and this slowly combined with the Greek heritage and culminated in Western civilization, as it is understood in the twentieth century. This civilization is symbolized by science more than by any of its other intellectual products: the Western world can celebrate knowledge triumphant! And yet, we all know that something has gone wrong. Biting scepticism of the relevance of knowledge, of the reality of our long-cherished progress and of the integrity of our morals is causing us intellectuals, teachers, humanists many a sleepless hour. If Jerusalem symbolizes prophetic certainty, authority and deterministic binding dependence on the past and Delphi symbolizes free, unfettered speculation, uninhibited by myth or authority, then the two have not found their synthesis yet. The seeming synthesis of the Victorian version of Protestant ethic has failed us. Jerusalem is still very far from Delphi. Having come from Jerusalem to Delphi, I shall try tentatively to suggest a way out of our growing doubt by attempting to see our crisis in historical perspective and by drawing some lessons from history. I am not sure that I fulfill the rule 'nothing in excess' - at least culturally speaking I shall try to respect the «know thyself».

The crisis I was referring to is a natural outgrowth of development in knowledge in society in the last two decades. The distinction between the problems of relevance, progress and morals is artificial: they all interact. Yet, for the sake of clarity: by a crisis in 'progress' we mean that for the first time in the last three hundred years we are not morally certain that our achievements indeed constitute progress. This doubt is not the traditional pessimism of the intellectual elite in all times, but an integral part of a growing anti-scientific and indeed anti-rationalistic trend which worries thousands of politicians and occupies hundreds of sociologists. The doubts about progress are linked to the moral doubts: it is not so clear any more that knowledge is an objective intellectual product of disembodied minds while human beings are free to choose what to do with it: right or wrong. It is not even so clear any more that the production of that quasi-objective knowledge is an unquestioned good. The Puritan ethic which was typical of 19th century and even 20th century science until a short while ago is crumbling with only bewilderment to replace it. But the gravest problem of all is, as I see it, the problem of relevance — not in the fad sense of the word — but in the following sense: we have reached a point where the problem-situations facing mankind, in its various cultural settings in all their richness and differentiæ are totally disconnected from themes occupying our accepted knowledge-producing institutions. It is not only true on the socio-political level but even on the deepest intellectual level that the existing scientific disciplines do not overlap with the existing problem-situations in science. On the socio-political level one could argue that in the universities only objective facts are being collected - objective facts on society, economics, education, phychology, etc. and the decision-maker should use this data to the best of his intellectual ability or his moral integrity. But this is 'objectivist fallacy' which is not rigidly adhered to anymore. On the other level mentioned, the situation is new: twenty years ago the major scientific problems still neatly fell into the old compartments of physics, chemistry and biology or the newer biochemistry, biophysics or evolutionary cosmology. This is not the case any more: the problem of cancer might be one of the last problems where we know where to look for the answer. The other great problems: how the brain works, the genesis of life and of the physical universe, the environmental crisis, the information explosion, the future of the underdeveloped countries, all look for new disciplines to deal with them. This is lack of relevance in the deepest intellectual sense. The crux of the problem is our traditional separation between the social and the natural sciences - a dichotomy which goes back to Socrates but perhaps has outlived its usefulness. We must look for new disciplines with sharp analytic tools which will combine biology and anthropology, sociology and the life sciences not by simply putting different experts in the same room, but by starting from problem-situations and not from the answers of the existing disciplines. This is the problem of knowledge. I suggest to explore a model of how knowledge grows and then paint in broad lines how, in the conceptual framework of that model, modern science developed from the

16th century to the end of the nineteenth. But let me first argue that no historical understanding is possible without having an articulated theory of the growth of knowledge.

The importance of a theory of the growth of knowledge

Growth of knowledge is a topic on which everybody has a theory. It is not always conscious, it is not always verbalized, and for those cases when it is conscious and it is verbalized, it is not always fully developed and coherent. But it is there. Whatever statement we make on science or scientists, on validity of a theory or on criteria of truth, we carry with that statement the implied burden of a whole bunch of other statements on the genesis of knowledge. If we reread carefully the two lists of statements which I wrote down above, picturing them at random, it will be inevitably clear that they imply that knowledge grows by accumulation. According to most of them knowledge consists of clearly established facts (positive contributions to knowledge) which are independent of any theoretical frameworks and free of unwarranted presuppositions. This growing mass of data creates the severe problem of information explosion and therefore makes strict professionalization inevitable. While, with growing knowledge, the frontiers of knowledge shift wide areas become clarified and left behind all in good order with their problems solved. It is also clear that if a scientist wishes to contribute anything original he has first of all to read up the subject and reach the frontiers of the field. Then, and only then, can he see further and develop the field. It is presupposed here, although not so stated, that at least the facts so far established (even if the theories are changeable) are eternal and therefore the only correct basis for further discoveries.

It follows here that since knowledge advances all the time, and one has to reach the frontiers, though in a more and more narrowly delimited field, the analytical tools become more and more sophisticated and therefore the initial investment in the quality of the tools (be it mathematical or experimental) becomes larger. It also follows that since this is so, the emotional investment in the importance of those tools is very large and thus the readiness to go down to fundamentals from time to time, an activity which needs an ability of oversimplification and thinking away the tools and in addition does not promise any certain success meets more and more emotional opposition, in addition to total absence of institutionalized encouragement. The organized scepticism of scientists relates only to the quality of the tools, not the basic theory. Let us recall three fairly recent developments:

(i) The Ph. D. dissertation which used to be considered an 'original contribution to human knowledge' has changed its meaning: in today's scientific community it is considered immoral on the supervisor's part to allow a student to choose a problem to which the supervisor does not at least vaguely see a solution; the reason for this is that if he fails to solve the problem, and only succeeds in analysing it or showing in what ways not to approach it (I am not referring to a *positive* experiment showing that something does not work: this is acceptable) then his career is ruined. This is not only a heavy burden on the supervisor's conscience but also constitutes a waste of society's investment in each Ph. D. in science, which is huge. All this has brought a redefinition of what is an original contribution. Another corollary of this situation is that only rarely does a student choose a topic — he is generally *given* one: this is then generally in the field of immediate interest of the supervisor — a rather enforced form of teamwork ensues. My main criticism is that at an age when scientists are admittedly at their best, they are given problems and discouraged from original or speculative probings. That in this situation the independent mind has to clash with his peer is clear. If he fails, it will not be recorded that he was a daring genius. If he succeeds, we all admire him for having gone against the trend and succeeded.

(ii) Referees of scientific journals (and especially in biochemistry) tend to reject papers if their experiments have not been executed on the most recent equipment; this irrespective of whether the theory described in any way depends on the precision of the results.

(iii) Experimental and sometimes even theoretical scientists, when asked on what they are working, will not respond by specifying a problem, but rather by giving the name of the technique or of the machinery they are using. This naturally may not mean more than just a way of expression, but very often it reflects the situation that having invested so many years in study, he is now most efficiently employed in concentrating on the technique and applying his knowledge to problems whose solution demands such a technique. If the problem is interesting and chosen by a good scientist then the whole question whether they are scientists or research technicians is a question of labels. If however these research technicians are wielding vast financial power and are monarchs of their kingdoms, they will desperately look for their own problems with the technique they want to use as their only criterion for locating the problems. The result is a huge percentage of totally unnecessary results and shoddy papers. When pointing to this state of affairs in discussion with open-eyed leading scientists, the answer generally is that the high percentage of meaningless scientific work is a necessary by-product of the scientific enterprise («it has always been so») and thus this is the price that has to be paid.

I shall return to this remark later: let me only run ahead with the historian's remark that all this resulted from the 20th century image of knowledge and it has *not* always been so. In the 17th, 18th and even 19th century a much higher percentage of results was meaningful and *not* because there was more to be discovered then [1]. As it is today, at no stage in the process of education, apprenticeship, or of high level consultation is a scientist encouraged to question critically the very foundations, nor are intellectual exercises done checking which eternal facts would become irrelevant (if not wrong) had the theoretical framework changed. On the other hand, it is widely admitted that nobody can influence the Einsteins of this world and they would go their way in any case. The presupposition here is that science's accumulative course suddenly changes by big leaps produced by individual geniuses who change the course of development, and, almost always, as if by a law of development, they are in radical opposition to their peers [2]. To read it once more: all these are views which together constitute a theory of the growth of knowledge.

According to the present image of knowledge, it is widely accepted that theories are accepted or abandoned on experimental evidence, irrespective of the price one has to pay for abandoning a theory or accepting a new one. We all know that very often ad hoc hypotheses are invented to save theories only to be tested on their own experimentally. This is more or less true. The problem is that here we must act differently than scientists, let us say in the 18th century, and if we succeed inventing an ad hoc hypothesis so formulated that it can

1. Though dealing here with the problems of progress in science, one should not forget that irrespective whether this wasteful system furthers (as usually considered) or hinders (as I hold) that progress, it seems that social pressures will not allow it for very long. With spreading popular scientific education more and more *enlightened* criticism of the scientific profession is to be expected.

2. Here I am only describing the presently current view that knowledge grows by accumulation. Below I shall show that the details of this theory are well explained by the presently accepted image of knowledge and do not relate to history at all. It will be also explained why the Kuhnian picture is so well-received by scientists.

be tested experimentally, our experimentation is vindicated and the scientific community rewards us. Since this process of experimentally testable ad hoc hypotheses is a practically infinite one, an ingenious scientist can spend a lifetime on that, the image of knowledge, and consequently the social pressure of the consensus-minded scientific community does not push us first to investigate the meaning of an undesirable experimental result with respect to fundamentals, nor with respect to other scientific theories. In other words, under the ruling image of knowledge, we think in terms of single theories, not networks of theories. To this I shall return when dealing with Scientific Research Programmes.

One last aspect of our current social image of knowledge: there is a widespread view that great discoveries are often made simultaneously in different places. Since science is universal (we believe) and since results are communicated immediately, this is not surprising. While admitting that due to good and speedy communication simultaneous discoveries are much more frequent in the 20th century than in previous centuries, I claim that this is only a seeming phenomenon because we do not concentrate on problems but our results can see developments by hindsight. Now hindsight if seen in historical context can be very illuminating, but when used in a conceptual framework according to which science is an accumulation of eternally true, theory-independent data, then hindsight is very misleading. What is actually happening in most cases is that different problems are posed, then different answers given, these different answers supplement each other and become one theory which is then read back into both results.

For a recent case of alleged simultaneity, take the case of Poincaré and Einstein. It is often stated that since Poincaré and Einstein simultaneously and independently (the unworthy and unproved claim of some [1] writers that there might have been conscious copying on Einstein's part I leave here as both uninteresting and false) developed identical transformation equations and thus Poincaré is a codiscoverer of at least part of the theory of relativity. Generally historians then ask why did Poincaré not go the whole way and why did he not discover the whole of the theory? It is pointed out that there was no *information* available to Einstein which was not available to Poincaré! But this point is

1. G. H. Keswani, Origin and Concept of Relativity I. Brit. J. Phil. Sci. XV (1965), 286-306 and II Brit. J. Phil. Sci. XVI (1965), 19-32.

true not only for Poincaré — as far as information goes the theory could have been discovered at least twenty years earlier. Goldberg in two papers showed that actually Poincaré and Einstein were working on two different problems:

> «The fact that Lorentz and Einstein had arrived at the same transformation equations does not mean that their theories are the same. Nor is it significant that Poincaré realized that the Lorentz equations form a group or that the equations imply that the velocity of light is the ultimate velocity. Poincaré (and Lorentz) and Einstein were doing different things, working on different theories from different points of view [1].

A different process which is more recent (again due to speedy communication) that by an iteration-like process one result is taken up, somewhat differently read than written (see my discussion on Einstein and the monkey below) which leads to a new result somewhat different in direction until finally the two individuals or the two teams reach the same results by two different processes heavily relying on each other. (A similar process is the Crick-Watson story) [2].

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1. Stanley Goldberg, «Poincaré's Silence and Einstein's Relativity: the Role of Theory and Experiment in Poincaré's Physics». Brit. J. in the Hist. Sci. V (1970), p. 82 and his previous: «Henri Poincaré and Einstein's Theory of Relativity» Amer. J. Phys. XXXV (1967), 934 - 944.

2. For the simultaneity argument see: W. F. Ogburn and D. S. Thomas, «Are inventions inevitable»? Political Science Quarterly, 37 (1922), 83. Here the authors collected some 150 cases of what they considered cases of independent discovery, or, as Merton calls them: «multiples». R. Merton, «Priorities in Scientific Discovery», B. Barber and W. Hirsch (eds.), The Sociology of Science, New York, 1962, pp. 447 - 485. There is another important essay by Merton on essentially the same problem: «Singletons and multiples in scientific discovery: a chapter in the sociology of science», Proc. of the Amer. Phil. Soc., 105 (October 1961), pp. 420 - 486. Also by Merton, «Resistance to the systematic study of multiple discoveries in science», European Journal of Sociology 4 (1963), pp. 237 - 282. T. Kuhn, «Energy Conservation as an Example of Simultaneous Discovery». In M. Claqett (ed.), Critical Problems in the History of Science, Madison 1958, pp. 321 - 356. Also in Barber and Hirsch (see above note), pp. 486 - 515. Y. Elkana: «The Conservation of Energy: a Case of Simultaneous Discovery?» in Archives Internationales d'Histoire des Sciences. Numero 90 - 91, Janvier - Juin 1970. For the genetic code see Watson's book «The Double Helix» and Gunther S. Stent: «DNA» and Robert Olby:

To round up the present discussion with a positive remark: we must create an image of knowledge according to which scientists are encouraged to think fundamentals and to differ from each other (theoretical pluralism): now discoveries are being made by those who have intellectual gifts plus daring to dissent. If the present image changes then the intellectual gifts will be sufficient and the group will not be delimited by the additional demand of daring.

What I described above was a randomly illustrated theory of growth of knowledge. Since my claim is that everybody has such a theory (albeit inarticulated) I claim that it was important to articulate the one which most scientists share and to juxtapose to it other theories and one which I hold to be true. In describing the present theory I relied on the concept of the image of knowledge which is an integral part of the theory of the growth of knowledge which I shall endorse below [1].

One of the two most fruitful models of the growth of knowledge is Lakatos' theory of Scientific Research Programmes [2] which views the growth of knowledge as a continuous critical dialogue between different research programmes at the core of which lie different scientific metaphysics. According to the other, Mertonian view, there is a somewhat random motion between socially determined strategic research sites, one replacing the other. I think these two models must be synthesized by describing the growth of knowledge as a result of three interacting factors:

i) problems in the body of knowledge which emerge from the scientific ideas themselves and which point to possible directions of change. Such problems can both originate in hypotheses, as well as create testable and untestable hypotheses. This world can be viewed as one of the disembodied ideas: the potentiality of directions of development. Here are all the possible avenues of research all with different scientific metaphysics at their core. However, only few of these avenues are actually explored. The actualization is determined by

ii) the image of science in a given culture at a given time: what-

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«Francis Crick, DNA, and the Central Dogma» both in Dædalus, Fall 1970: The Making of Modern Science: Biographical Studies.

1. It is very rarely that one can read moderate criticism of the scientific enterprise written by insiders. The one recent source is J. Ravetz's fascinating book: «Scientific Knowledge and Its Social Problems». Oxford U.P. 1971.

2. I. Lakatos, «History of Science and its Rational Reconstructions», Boston Studies in the Phil. Sci. VIII, pp. 91 - 136.

ever people in general and scientists in particular *think* of science, its role, its ethos etc. This influences heavily the choice of problems from among the enormous range of open problems as provided by the body of knowledge itself (i.e. by factor i). The image of science shapes the formulation of selected problems, determines what are called the 'frontiers of science' and determines the *reasons* for scientists' promotions. It is sometimes so influential, that it not only emphasizes some of the open problems but even totally obscures others. It decides what is legitimate science and what is pseudo-science, it shapes the demarcation criteria between science and metaphysics, it determines the criteria for a satisfactory explanation.

iii) Social and political factors which interfere directly with the lives of the scientist and the scientific institutions and thus influence the development of science through the scientist [1].

The problems arising out of the body of knowledge can be understood in terms of pure history of disembodied ideas and especially as results of the continuous critical dialogue between the competing research programmes. However, which problems are chosen out of the multitude of possibilities, that is, which problems will become strategic research sites, is generally determined by the prevailing image of science, i.e., it is socially determined. Factor iii), namely institutional influences on a scientist's physical and social environmental or physical manipulation of his person, is the typical «external» influence which both Lakatos[2] and Kuhn[3] (from different points of view) would happily demarcate from the internal factors of i).

Such a model of growth or change is a prerequisite before we can do proper comparative historical sociology of scientific knowledge.

When we look at the history of culture, the distinction of three factors seems as artificial as the internal-external dichotomy which is advocated by Lakatos or Kuhn[4] for it is clear that histori-

1. The external factor can be understood on the following level: if we ask why there was no Mendelian genetics in Soviet Russia, the answer is: because all the geneticists were executed during the Stalinist purges. This is truly and trivially «external».

2. Ibid.

3. T. Kuhn, «The Structure of Scientific Revolutions», Chicago U.P. Second Ed., 1969.

4. Lakatos' paper is a printed one followed by a critical analysis in my: «The Debates around Boltzmann's scientific research programme and the alternatives to it» in: Y. Elkana (ed.). Chapters in the interaction between science and philosophy», in press, Humanities Press.

cally all three interact very complexly. The distinction becomes useful, however, if we look at a given moment at a society as if we took a photographic picture of an intellectual-social situation. Then it becomes very clear that whatever people think about the world belongs to the body of knowledge, whatever they *think* (only conscious thought-processes are relevant here) about science and its task is the social image of knowledge, and their ideologies, politics, whishes, needs etc., are the manipulative factors which make people act in a given way. In other words, their intellectual choices are factor ii), the decisions are factor iii).

Could we apply to these two kinds of tradition, the Nietzschean distinction between Apollonian and Dionysian? As N i e t z c h e sees it:

«Apollo, als der Gott aller bildnerischen Kräfte, ist zugleich der seiner Wurzel nach der «Scheinende», die Lichtgottheit ist, beherrscht auch dem schönen Schein der inneren Phantasie-Welt. Die höhere Wahrheit, die Volkommenheit dieser Zustände im Gegensatz zu der lückenhaft verständlichen Tageswirklichkeit, sodann das tiefe Bewustsein von der in Schlaf und Traum heilenden und helfenden Natur ist zugleich das symbolische Analogon der wahrsagenden Fähigkeit und überhaupt der Künste, durch die das Leben möglich und lebenswert gemacht wird.

Das Wesen des *Dionysischen* wird uns am nächsten noch durch die Analogie des Rausches gebracht. Entweder durch den Einfluß des narkotischen Getränkes, von dem alle ursprünglichen Menschen und Völker in Hymnen sprechen, oder bei dem gewaltigen, die ganze Natur lustvoll durchdringenden Nahen des Frühlings erwachen jene dionysischen Regungen, in deren Steigerung das Subjektive zu völliger Selbstvergessenheit hinschwindet».

N i e t z c h e saw the two in terms of art: Apollo symbolizing the «Kunst des Bildners» while Dionysus the «unbildliche Kunst der Musik». The Attic tragedy is born out of the synthesis of the two. This distinction has been repeatedly mentioned in the context of classifying two kinds of genius in general and scientific genius in particular. O s t w a l d's distinction between the classical and the romantic scientist is well-known, so is M a t t h e w A r n o l d's. Recently the noted biologist, A. S z e n t - G y ö r g y i remarked on it in a readers letter to «Science» where he pleads for allocating to intuition (the Dionysian element according to him) its rightful place in science [1]. But this dicho-

1. The N i e t z s c h e quotation is from «Die Geburt der Tragödie» in G.

tomy is much too simple-minded. The growth of knowledge cannot be viewed simply through the fierce battle between the rational and irrational or between the clearly defined and vague. Neither can it be seen on the social level as a fierce battle between elitist and populist («Apollo always moved in the best society whereas Dionysus was much more the god of common man»)[1]. The critical dialogue is much more subtle than all these distinctions: it is between those who, in order to save one metaphysical view of the world, are ready to tolerate clear-cut discrepancies, well-formulated theories, and securely established experimental results, and others who cannot tolerate such a discrepancy and prefer to delimit themselves to one single theory (and not a network of theories) and thus to ignore the broad metaphysical world-view. The scientific elite among both groups at all times formulated their theories as clearly as the times in which they lived dictated to them, and performed their experiments as accurately as it was expected of them. Their methods were rational, i.e. purposeful to a consciously acknowledged aim, and their sticking to a metaphysical world-view or to logical coherence of a partial world-view can be seen as irrational if we wish to see it so. If we are referring to the image of knowledge we can detect a critical dialogue between those who wanted to explain all phenomena by reducing them to essences (essentialists) and others who thought that science's task is to qualify all phenomena (the mathematical physicists): or we can see a critical dialogue between the realists who claim that science must make us understand the world as it is and the instrumentalists who claim that the task of science is merely to predict; then again there is a critical dialogue between those who say: nature is not infinitely comprehensible, for nature the '*ignorabimus*' holds and these therefore concentrate on the visible, solvable problems, while the others to

Stenzel (ed.), Nietzsches Werke, Salzburg 1952, Vol. I, pp. 605 - 606. The Szent-Györgyi reference is from a letter to «Science» 176 (1972) 966. Ostwald expounded his theory in «Große Männer» Leipzig 1909. M. Arnold in his «Pagan and Christian Religious Sentiment» and in his juxtaposition of 'Hellenism' to 'Hebraism'.

1. Quoted by Antony Andrews in «Great Society» Pelican 1971, p. 252. The tension between the Apollonian and Dionysian penetrates Greek culture in all its manifestations. Homer tried to exclude the irrational. Even when he (or some other Homeric poet) writes a hymn to Demeter, the goddess becomes almost sober and not accompanied by the usual wild music and ritual. On the other hand, Andrews says of Euripides 'Bacchæ: «Whatever else the play may contain, it has to be read as a sermon on the danger of trying to suppress the irrational altogether, a danger to which Greeks were liable» (p. 258).

whom nature is comprehensible to the last and therefore to most problems apply at most the '*ignoramus*', attack all problems not yet solved. At all ages there are representatives of both sides in each dialogue, yet most ages are characterized by one side while a dissenting majority represents the other. This we shall see below.

Was the Scientific Revolution a Revolution?

If it is true, as I think it is, that knowledge grows by a continuous critical dialogue between competing Scientific Research Programmes, and especially between competing scientific metaphysics at the core of these programmes, and by a continuous critical dialogue between competing social images of knowledge, then there are no revolutions [1]. However, hindsight is inescapable: the knowledge considered true and scientific in the 19th and 20th century, the conceptions prevalent today, even the different protagonists occupying those of us today who see a continuous critical dialogue, all those have their source in the 17th century. It is very probable that with the elimination of the objectivist instrumentalistic and methodological fallacies, the 17th century will seem a direct continuation of the 15th and 16th centuries, while the Victorian 19th century will become the Dark Ages. It is well known that historiographic changes in the concept of the Middle Ages or of the Industrial Revolution underwent the pressure of changing images of the past as a reflection of changing self-images of the present [2]. In other words, a revolution is a revolution only with respect to a course of events which is considered normal and generally of long standing. Since it is our view that knowledge — true, scientific knowledge — all started in Greece but did not develop further due to lack of experimental method, and that in the Dark Ages humanity retreated into the

1. The evolutionary view, or rather the view that knowledge grows by a continuous series of micro-revolutions, has been forcefully championed by S. Toulmin. See his «Conceptual Revolution in Science» in R. S. Cohen and Marx W. Wartofsky (eds.): Boston Studies in the Philosophy of Science, Vol. III, New York, 1968, pp. 331 - 337, and his recent book «Human Understanding». Vol. I Oxford U. Press 1972.

2. The English never underrated the glorious 13th, 14th and 15th centuries. For them it was a time of relative peace and prosperity, when the continent was swamped by wars, famine, plague, and the repeated enthusiastic millennium movements. It is enough to compare a book like G. G. Coulton's «Medieval Panorama» (Oxford U.P.). Norman Cohn's «The Pursuit of the Millennium» (Paladin, 1970). John Huizinga's remarks on the discrepancy of images in his «The Waning of the Middle Ages» (Pelican).

obscurantism of superstition, while a spark of light was preserved only in the monasteries, this spark itself degenerating soon into the empty quibbling of scholastic philosophy, we consider the 17th century revolutionary. Such analyses apply to all intellectual revolutions (it is different with social revolutions which are very real indeed and serve as wrongly chosen models for intellectual history). When we look at these in the proper historical context, we shall find that continuous critical dialogue between the competing Scientific Research Programmes and the competing images: some trends will be seen more connected with the past, some with the future, some from our point of view (our 20th century) successful, some others will seem failures.

For the 17th century two important kinds of historical research had to be done: in order to dismiss the myth of the revolution, even if keeping the label for convenience. The one kind of research was initiated by P. Duhem, who showed how all our favourite characteristics of 'good' science, like the experimental method, the critical attitude, the heliocentric model - all have a history of many hundreds of years back before the 17th century [1]. The second approach was de-

1. In addition to Duhem, the great rediscoverers of pre-17th century science were George Sarton, Lynn Thorndike (in his majestic ten volume «The History of Magic and Experimental Science»), Anneliese Maier and Marshall Clagett, and A. C. Crombie. Crombie, on. p. 1 of his «Robert Grosseteste and the Origin of Experimental Science; 1100 - 1700» (Oxford U.P., 1953) says: «The history of science shows that the most striking changes are nearly always brought about by new conceptions of scientific procedure. The task demanding real genius is the revision of the questions asked, the types of explanation looked for, the criteria for accepting one explanation and not another. Underlying the conception of scientific explanation accepted, for example, by Galileo, Harvey and Newton, was the theory of formal proof developed by the Greek geometers and logicians. The distinctive feature of scientific method in the 17th century, as compared with that in ancient Greece, was its conception of how to relate a theory to the observed facts it explained, the set of logical procedures it contained for constructing theories and for submitting them to experimental tests. Modern science owes most of its success to the use of these inductive and experimental procedures, constituting what is often called 'the experimental method'. The thesis of this book is that the modern, systematic understanding of at least the qualitative aspects of this method was created by the philosophers of the West in the 13th century. It was they who transformed the Greek geometrical method into the experimental science of the modern world».

But in order to accept such a continuity theory one does not have to see the chief point in 17th century science in its experimental methods. The great A. Koyré, in his «The Origin of Modern Science, A New Interpretation, pp. 11-12, says: «As for myself, I don't believe in the explanation of the birth and veloped fairly recently by the historians who showed the 'irrational', 'unscientific' traditions which were predominant in the 17th century cultural scene like the Paracelsian, Hermetic, Rosicrucian and other Scientific Research Programmes [1].

Add to it that some of the chief attributes of modern science (like conservation laws) were completely alien to the leading Scientific Research Programmes of the 17th century (except to the synthesis-minded ones like Leibniz's - see below) and were introduced only in the 18th and 19th centuries, then the 17th century is not more revolutionary than the 13th or the 19th.

What we must admit, however, is that most of the scientific theories which we hold today as part of our commonsense science (not by working scientists) were formulated in the 17th century. The Scientific Revolution is sheer hindsight. Since, however, we do tend to see the world from our own vantage point, let me choose the 17th century as a convenient starting point for my general survey.

Where it came from.

During the bitter fight between Reformation and Counter-Reformation a sceptical crisis arose, a crisis that challenged the basic prin-

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development of modern science by the human mind turning away from theory to *praxis*. I have always felt that it did not fit the real development of scientific thought, even in the 17th century; it seems to me to fit even less that of the 13th and 14th. I don't deny, of course, that in spite of their alleged — and often real — 'otherworldliness', the Middle Ages, or to be more exact, a certain, and even a rather large number of people during the Middle Ages, *were* interested in techniques; nor that they gave to mankind a certain number of highly important inventions. Some of them, had they been made by the Ancients, would probably have saved the Ancient World from collapse and destruction by the predatory Barbarians. Yet, as a matter of fact, the invention of the plough, of the horse harness, of the crank, and of the stern rudder, had nothing to do with scientific development; even such technical marvels as the Gothic arch, stained glass, the foliot or the fusee of late medieval clocks and watches did not depend on, nor result in, any progress in corresponding scientific theories. Strange as it may seem, even such a revolutionary discovery as that of firearms has had no more scientific effect than it had scientific bases. Bullets and cannon balls brought down feudalism and medieval castles, but medieval dynamics resisted the impact. Indeed, if practical interest were the necessary and sufficient precondition of experimental science — in our sense of the word — this science would have been created a thousand years, at least, before Robert Grosseteste, by the engineers of the Roman Empire, if not by those of the Roman Republic.

1. Thus the contributions of W. Pagel, A. Debus, Ch. Webster, P. Rattansi, T. McGuire and P. Heimann.

ciples of theology, humanistic studies, moral studies and the sciences. Man's confidence to discover truth by using human reason — a confidence so typical of both Scholastics and Renaissance naturalists — became undermined. The chief spokesman of this devastating attack was the philosopher-essayist Michel de Montaigne (1533-1592). Let me illustrate this with two passages:

«There is no desire more natural than the desire for knowledge. We try all the ways that can lead us to it. When reason fails us, we make use of experience,

'Experience by diverse trails, art has made
Example pointing out the way.'

(Manilius)

which is a weaker and less dignified means. But truth is so great a thing that we ought not to disdain any medium that will lead us to it. Reason has so many shapes that we do not know which one to take hold of; experience has no fewer» [1].

and,

«Men do not know the natural infirmity of their mind: it does nothing but ferret and quest, and keeps incessantly spinning about, constructing and getting entangled in its own work, like our silkworms, and is suffocated in it. A 'mouse in a pitch barrel' (Erasmus, Adages). It think it observes in the distance some sort of gleam of imaginary light and truth; but while it is running to it, so many difficulties, obstacles, and new quests cross its path that they lead it astray ant intoxicate it. Not much different from Aesop's dogs, who, discovering something like a dead body floating in the sea, and not being able to approach it, attempted to drink up this water and dry up the passage, and so choked themselves» [2].

But total scepticism could not last long. The phyrrhonics — as they were called — became few, and the generation after Montaigne emerged optimistic. It gave three different answers to their teacher's scathing criticism: one was the religious answer of Father Pierre Charron. For him any scientific theory is actually blasphemy, since every theory limits God's power and ability to what man can understand. In other words, the very foundation of science, namely

1. Michel de Montaigne, Selected Essays, B. W. Bates (ed.), The Modern Library, 1949: «Of Experience», p. 537.

2. Op.cit., p. 541.

the presupposition of Nature's comprehensibility, is for him anathema. Certain knowledge can only be gained by Revelation. As Koyré[1] put it: «to the uncertainty of natural reason Charron opposes the supernatural certainty of faith». Here *Pistis* once again replaces *Gnosis*.

Francis Bacon offered experience as a remedy: «the sterile uncertainty of reason left to itself the fruitful certainty of experience». Bacon's programme failed scientifically but succeeded socially: his remedy became part of the ethos of science: our image of certain knowledge from the 17th century till today relies on experience.

The third reaction was that of Descartes: reason. «Going beyond common sense and classification (which Bacon aimed at just as intently as Aristotle) he followed 'In order of ideas, not that of things'».

Common to all three answers - faith, reason, experience, is their polarity. All three are analytic in nature and claim dogmatically to be capable of reducing the whole world to either faith, reason or experience. This polarity seems the cause of certainty: for whatever knowledge there is, on the basis of either faith or reason or experience, is certain knowledge - the rest is excluded, is not considered knowledge at all. Thus their image of knowledge is: the only possible knowledge is certain knowledge. Their counterpart is the vague, metaphysical synthesis which accepts all kinds of knowledge and believes in the comprehensibility of nature, in the existence of a pre-established harmony, or in a biologically conditioned fit between the human mind and the world which it is to comprehend; it creates great systems of thought. This kind of synthesis is broad, tolerant and much less certain than either Revelation, clear and distinct ideas, or induction. The greatest representatives of this tradition are Leibniz, Euler, Kant, Faraday, Helmholtz, Boltzmann and Einstein. All of them non-polar, all of them expounding a fruitful web of scientific metaphysics, all of them physical realists. Their contributions to the body of knowledge are well known. However, their image of knowledge was suppressed. From the 17th century onward, scien-

1. A. Koyré, Introduction to E. Anscombe & P. Geach (eds.): «Descartes Philosophical Writings» (Nelson, 1970). See also: R. Popkin, «The History of Scepticism from Erasmus to Descartes», (N.Y., 1961). H. G. van Leeuwen, «The Problem of Certainty in English Thought 1630 - 1690» (M. Neijhoff, 1970).

tific knowledge claimed certainty, and it claimed to have found it in either reason or experience — mostly in experience. The polar reactions to the 16th century scepticism, which became the hallmark of science first in the 17th century and then again more forcefully in the 19th century, can be characterized as *constructive scepticism*. The great natural philosophers of the 17th century: Galileo, Descartes, Gassendi, Newton, Boyle and others were all constructive sceptics.

The difference, however, between the two views did not result in differences in the quality of science. One should not make the mistake of brushing this distinction aside by a superficial «well, what you mean is simply that some were scientists while the others were at best philosophers». I am referring only to the best minds in the 17th century, and all of them were simply *natural philosophers*. Moreover, their *positive* contributions to present-day knowledge (a criterion for excellence which is not mine but of those positivists with whom I am debating here) are comparable. True, it is Newton who formulated the general law of universal gravitation, but it is Leibniz from whom all conservation ideas stem.

However, all these are attempts at classifying individuals according to their genius, i.e. according to inborn qualities, while my attempt is to distinguish different kinds of Scientific Research Programmes rather than different kinds of individuals. In my opinion, one can find all the various kinds of genius and all the various psychological characteristics among the representatives of both kinds of research programmes. Moreover, I do not claim that there are only two such general trends. What I claim is that, since all change in knowledge occurs dialectically, every question or problem creates its own dichotomy. Very many different questions occupy people's minds simultaneously, and it is only in a given context that a dichotomy seems so important. The context I chose here was the scepticism of the end of the 16th century, and in relation to this problem the two trends emerged: the dogmatic and the synthetic. One could concentrate on religious, social or economic issues which would point to other critical dialogues [1].

The answers given by the constructive sceptics and by the great synthesizers (Leibniz, the Helmontzians, the Paracelsians) established the source of the major themes in the 17th century. I shall deal

1. I will give an example of this in discussing Newtonianism in the 19th century below.

with some of them below, always taking up a critical dialogue centering on a given theme.

The issue of certainty.

One of the central problems in modern physics is the tangled issue of determinism, a probabilistic account of physical states. Here scientific considerations par excellence become indistinguishable from moral-religious issues and even from a deep psychological craving for certainty which seems to be inherent in the human being. Is it indeed so? Where does our quest for certainty come from? Is there anything specifically scientific in our preference for a deductive proof over an inductively reached conclusion? Is indeed a Laplacian deterministic world an unreachable ideal, in comparison to which all our present theories are only a second-best?

The 17th century sources of the quest for certainty we have just seen: all the three polar answers promised certainty. Religious certainty by revelation separated out of the world of knowledge and became a different branch of human consciousness. To us this will be of little concern for the next two hundred years. The Cartesian programme of certainty by ratiocination failed as a scientific research programme aimed at understanding the physical world, and by the mid-18th century it yielded to the Newtonian research programme which claimed to be following Bacon directly. As far as the Cartesians' social image of science goes, namely that the aim of science was the mathematization of all fields of enquiry, it was taken up by the Newtonians and became the dominant force in science for a hundred and fifty years. But this is true only of one of the Newtonian traditions. The Newtonian Research Programme split into two parts: the one based on the Principia, and it was mathematical and reductionistic; the other based on the Opticks, and it represented a programme of developing a theory of matter which would explain chemistry and biology and fit the 17th century image of God. The hard-core metaphysics of the one was that the world consists of discrete particles between which central forces are acting at a distance, the other a programme at the core of which was the nutshell theory of matter [1]. This second was taken up by natural philosophers — materialists who all believed in an all-pervasive

1. See A. Thackray, «Matter in a Nut-Shell: Newton's Opticks and 18th Century Chemistry», Ambix 15 (1968), pp. 28-53, and also his: «Quantified Chemistry — The Newtonian Dream» in D. S. L. Cardwell (ed.): «John Dalton and the Progress of Science», Manchester U.P., 1968, pp. 92-108.