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ΣΕΙΡΑ ΔΕΥΤΕΡΑ: ΜΕΛΕΤΑΙ ΚΑΙ ΕΡΕΥΝΑΙ

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# ΠΡΑΚΤΙΚΑ

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## THE LAST CENTURY OF SCIENCE : PROGRESS, PROBLEMS AND PROSPECTS

About a hundred years ago, something very novel had taken place on the American educational scene. In Baltimore, grounds were laid for Johns Hopkins University. Unlike Harvard, Yale, Princeton, and other now famous places, Johns Hopkins was not a college. It was a university, in the sense that its courses were open only to candidates for doctor's degrees. It was an entirely new idea in the USA, where doctor's degrees were first given only a hundred years ago, and where hundred years ago graduate students, or doctoral candidates, were as scarce as hen's teeth. One of the first professors at Johns Hopkins was H. A. Rowland, the first American to achieve international status as a physicist. One day a professor from Yale, so the story goes, was visiting at Johns Hopkins, and became very curious about those so-called graduate students. So he asked Rowland the question: «What do you do with those graduate students?» Rowland's reply was: «Ignore them.» A hundred years later, if there is anything a professor cannot afford in an American university, is to ignore graduate students. Today, graduate students form a large body, and have an increasingly important say in university policymaking. So a professor had better not ignore them, or in a broader sense, he had better not ignore the social, cultural ambience in which he works. There is progress here, though the word progress immediately bespeaks its complications. The anecdote about Rowland is, therefore, symbolic in a broader sense, which is the burden of this paper to develop.

In Europe, the change in the status of science during the last hundred years can be illustrated with a few telling data. A hundred years ago Germany was just gaining leadership in science which it was to hold for two generations. The price of taking the lead was, by modern standards, ridiculously small. In my field, physics, Germany quickly created a dozen serious chairs in physics, twice as many as those in France and in England. But whether in Germany, or France, or Eng-

land, or elsewhere in Europe, requirements for a Ph. D. in physics were exceedingly low, compared with the requirements a hundred years later. A story will best illustrate the difference. A hundred years ago the famous Dutch physicist, Hendrick Lorentz, was just working for his doctor's degree in Leiden. He entered there at the age of 19 and two years later he was ready to write his dissertation. There was nothing original in it, a straight résumé of the scientific novelty of the day, Maxwell's electromagnetic equations. The jury was much impressed. Lorentz got the highest marks, and was almost immediately called to teach at Leiden.

Today Maxwell is usually mentioned as one of the half dozen greatest physicists who have ever lived. But a hundred years ago the really famous physicist was William Thomson, the future Lord Kelvin. For over forty years he produced one paper a month which hardly anybody reads today. He wrote thousands of pages on what did not exist, the ether, and the so-called vortex atoms, made of the ether. Kelvin was extremely popular as a speaker, but the beginning of his fame is connected with the first transatlantic cable. A hundred years ago, it was the big news, partly because when the whole length of the cable was in place, it did not work. That it finally did work was due to Lord Kelvin. There was some originality in his contribution which concerned the properties of very long conductors. Today, it is a commonplace in textbooks of electrical engineering. A hundred years after Kelvin, and the first transatlantic cable, a gigantic volume of messages flashes back and forth across the Atlantic. The messages are carried by devices about which Kelvin could have no inkling, because although he spoke much about the atomic structure of matter, he knew practically nothing about it. He was already dead when, in 1913, the systematic conquest of the world of atoms began with Bohr's historic paper. Due to that conquest, almost any message can be heard by anybody in any corner of the world with a small transistor radio.

Small transistor radios are based on man's newly acquired ability to deposit various types of matter in layers that are of the width of  $10^{-7}$  cm, that is, the width of about 10 atoms. A hundred years ago the resolution of the best microscopes was about  $10^{-3}$  cm, or the width of 100,000 atoms; today, electron microscopes can see 100,000 times better and it is even possible to obtain details about the nucleus which is smaller by another factor of 100,000. A little over a hundred years ago Kelvin formulated the notion of absolute zero temperature,

that is, 273 degrees below freezing point. But the coldest temperature that could be produced then was about 200 degrees warmer than absolute zero. Today, there is a special branch of science, cryogenics, which studies phenomena, very strange phenomena, that occur when the temperature differs from absolute zero by one or two degrees or by the mere fraction of one degree. A hundred years ago, the mercury vacuum pump was big news; the best vacuum it produced was  $1/100$  of the atmospheric pressure. Today, experiments are performed in vacuums a million times better, to say nothing of the almost perfect vacuum of outer space, which is now available for experimentation in orbiting laboratories.

In more technical language, all this means that the precision of exact science increased by six to ten orders of magnitude during the last hundred years. Compared with what happened during the previous three hundred years, this is an enormous advance. Along with these technical advances there came new conquests on the theoretical level. A hundred years ago it was pleasant to believe that every physical process derived from strictly mechanical interactions, due to mechanical force or energy. Today, we know that all this was a pleasant myth. Modern physics counts four apparently disconnected physical forces or fields: gravitational, electromagnetic, nuclear, and weak forces. The best minds are at work to find a unity among these four forces and the price of advance is to know ever smaller details about the smallest parts of matter. The advance has been equally impressive in the opposite direction. A hundred years ago the greatest distance that could be measured in space was of the order of a few light years, derived from the parallax of the nearest stars. Today, the distances reliably estimated by astronomers are a billion times greater, the distances of the most remote galaxies.

So much in a way of illustration about the progress made by science during the last hundred years. My examples of that progress have been chosen, for the most part, with an eye on the most elementary meaning of what progress stands for. The etymology of the word means advancing in the space-time continuum. In that respect, the advance of science, and of its most exact form, physics, must be admitted even by those for whom the word progress stands for something far more than a gigantic foray across space and time, into the realm of the very large and into the realm of the exceedingly small. Happily, for the sake of cultural progress, most creative physicists are in agreement that progress in the full human or humanistic matrix must mean far more than



exploring and controlling the physical dimensions of space and time.

A chief reason for this agreement derives from the manner in which physics progressed during the last hundred, or rather, during the last seventy years. Instinctive thinking about progress pictures it as a steady, well-planned advance across previously unexplored areas. Such was the manner in which the progress of science was described by Bacon, by the founders of the Royal Society, by Immanuel Kant, and by the Encyclopedists. One price to be paid for that naive concept of the progress of science was the adoption of the cliché phrase that in science everything was darkness until Galileo let some balls roll down an inclined plane. This Kantian and Encyclopedist phrase was duly repeated throughout the 19th century. It formed perhaps the only point of agreement among Herbert Spencer, the Rev. William Whewell, and Friedrich Engels, who all tried to rest their widely differing philosophies of progress on the allegedly smooth, that is, inevitable and predictable progress of science.

The 20th-century reading of the progress of science is slightly better than that inherited from the 19th century. This is not to suggest that we are more intelligent than scholars, or historians of science, were a hundred years ago. Our advantage is largely independent of our mental abilities. In the 20th century science progressed in a way which defies a simplistic definition of progress. Or to do more justice to the facts of scientific history, one may say, that in the 20th century science began to do in a very obvious way what it has always been doing, namely, developing, or progressing in an unpredictable manner. As a result, 20th-century scholars have been forced to recognize that unpredictable pattern which had been largely ignored during the previous two hundred years.

Yes, the way in which science progresses is very unpredictable. This should be clear from the fact that the progress of science depends largely on the sudden emergence of geniuses. Their appearance at a particular place and time is something which has so far defied even a remotely adequate psychological and sociological explanation. This next year will come the 500th anniversary of Copernicus' birth. Thousands before him knew about the heliocentric theory of Aristarchus. But its spark ignited into a torch of flame only in the mind of an obscure Pole, and in a university town which until then had not produced anything extraordinary. During the next 50 years, thousands of scholars learned of the Copernican theory. But nothing happened, until there came along, defying all probability calculus, the enormously

strange mind and personality of Kepler. Without his laws, one of which was the result of two errors that almost miraculously canceled one another, Newton would not have reached even first base with his epoch-making development of a central force obeying the inverse square law.

But let us move into the realm of modern science. There is no count of scientists who studied and measured the speed of light by the time a young boy in Munich reached the age of fifteen. His name was Albert Einstein. He was a strange boy. He hardly spoke until the age of seven. Eight years later, he was seized with the question whether it was possible to see faster than the speed of light. That strange youthful preoccupation played a seminal part, as he himself stated, in turning him into the kind of physicist he became. Or conversely, without that youthful vagary of mind, 20th-century physics would not have become what it actually did become. Or let us recall the strange meeting of a young Polish student, Maria Sklodowska, in Paris, with an equally strange French physicist, Pierre Curie. Anyone familiar with her biography should realize that the chances were inconceivably small that their paths should have ever crossed. Yet, without that meeting of theirs many things in 20th-century science would have taken place much later and perhaps in a very different form. Examples of this kind could be listed for hours on end, though I know all too well that no array of evidence of this kind would tell anything to one who is convinced that science is the inevitable, let alone a smoothly inevitable, product of socio-economical conditions and circumstances.

This unpredictability of scientific advance can readily be seen also from the invariable failure of predicting the future of discoveries even within a limited field and for a short period of time. Perhaps a few modern examples would not be out of place. A few years ago there came into my hands an article by the famous American astronomer, Harlow Shapley. The article, written in the early fifties, was on the advances which astronomy was to make during the second half of the twentieth century. His article was hardly fifteen years old when it was already very clear that few of the advances, which Shapley predicted, came to materialize, and a great many advances were made about which Shapley's article contained not a hint (orbiting telescopes, quasars, etc.). The same point could equally well be illustrated by what happened in elementary particle physics. A few of the new particles were predicted, but many more turned up unexpect-

edly, and played havoc with well established theories. In short, discoveries, crucially new discoveries, are being made which are wholly unforeseen, and things are accomplished which only a few years earlier had been declared to be simply impossible.

The latter point bears a little illustration. In 1951 V a n n e v a r B u s h, the great organizer of American scientific technology, flatly declared that intercontinental ballistic missiles were impossible to make. In 1933 Lord R u t h e r f o r d, the chief architect of nuclear physics, spoke of the industrial utilization of nuclear energy as being equivalent to talking moonshine. Much less known is the case of Sir G e o r g e D a r w i n, the son of C h a r l e s D a r w i n, and possibly the foremost expert on celestial dynamics around the turn of the century. In 1910, he discussed in a lecture in Cambridge the new theory which C h a m b e r l i n and M o u l t o n in Chicago had submitted on the evolution of planetary systems. The theory is known as the planetesimal theory because it pictured the formation of planets from the meeting or collision of many small planets, or planetesimals. D a r w i n could easily point out that the meeting of two bodies in space, even if moving in close and similar orbits, was a most unlikely event. And to illustrate the extremely small probability, he added: imagine the incredibly fine marksmanship needed to hit Mars from the earth with a bullet. It will be, he said, 10,000 years before scientists will solve that problem. He was wrong by two orders of magnitude. Instead of 10,000 years, 50 years were enough.

Playing the prophet is a risky business, but is especially so in science, and in modern science in particular. Why in modern science in particular? A chief reason for this lies in the relation between mathematics and modern science, or physics. I said *modern* science, with an emphasis. During the centuries of classical physics, mathematics played largely the role of a humble maidservant. Most mathematicians were physicists and the new pages of mathematics were written in the measure in which new problems in physics needed an exact treatment and solution. But around the middle of the 19th century the humble maidservant declared independence. She was from there on to have her own thoughts, select her own problems, pursue her own course of research, regardless of the needs of physicists. The new development went largely unnoticed even by physicists until about the 1920's. Then, B o r n and H e i s e n b e r g found with great astonishment that the mathematics needed by quantum mechanics was developed many years earlier by mathematicians, that is, by those



emancipated mathematicians who began to work on problems of mathematics which at that time seemed to have no connection whatever with physics and the physical world. The best known examples are group theory, the well-behaving equations, and matrix calculus. Oddly enough, more than half a century after they had been first formulated, they were found to fit marvelously the workings of nature on its atomic level. There are of course many other examples of that puzzling fact, which Wigner so aptly called about ten years ago, the «unreasonable effectiveness of mathematics in physics.»

The word «unreasonable» could not have been better chosen. For it is beyond reasonable explanation that one out of so many mathematical formalisms should fit a broad range of physical phenomena, and would imply the prediction of until then unsuspected phenomena. Mathematics itself provides no criteria in this respect. It cannot tell us why some of its theorems are immensely more effective physically than many others. Gone are the days when Hilbert, Poincaré and others could still dream about an ultimate system of mathematics, assigning to all particular theorems a special place in the hierarchy of more and more fundamental theorems. Ever since Gödel formulated his famous theorem in 1930, it has become an open secret that mathematics is not a clear-cut, self-consistent pyramid of a finite number of propositions. In other words, mathematics is not going to provide for physics a logically and neatly arranged ladder for unfolding deeper and deeper layers of the physical reality. Thus, we may speak of the «unreasonable» role of mathematics in physics, a role very similar to the almost haphazard process which is the emergence of a genius, or the coming about of a stroke of a genius. As a result, physics, or exact science, is a very unpredictable enterprise. Its road of advance is anything but smooth. In fact, it is a road full of bumps, the impact of which is bound to dislocate an increasingly larger number of joints and with an ever heavier impact. This is very important to remember when we focus on science as a factor of cultural progress. Science makes for progress, but for a very bumpy one. The question which then offers itself, but which is not the purpose of my paper to investigate, is how many big bumps can be had if the advance is still to be called progress and not something else.

I have spoken of the dislocation of «an increasingly larger number of joints and with an ever heavier impact.» The «increasingly larger number of bumps» is proportional to the number of new discoveries and these keep growing larger in number as time goes on. The new data



about the physical world are being collected at a rate which seems to grow exponentially. From its first effective or viable birth in the late Middle Ages, science has been feeding on its own findings. But this feedback process, which until the 20th century could appear as a healthy, organic growth, seems to turn into a runaway mechanism. The great increase in research laboratories is only partly responsible for this. The real culprit seems to be the «unreasonable effectiveness» of mathematics. It is enough here to recall the famous formula  $E=mc^2$ . Who guessed its real contents when first printed in 1904? When a year later Einstein gave a more fundamental derivation of it, it was still a mathematical formula and nothing else. Many of our present-day agonies derive from the fact that the formula implies immensely much, and in a literally devastating sense.

Clearly, all these implications dawned on us only much later, and even then we were largely unprepared to deal with them, or even with smaller challenges. And yet, these challenges keep popping up at the most unexpected moments. They certainly secure excitement to the scientific enterprise, but they also make a steadily growing tension more acute. It arises from the difference between tools and goals. Science, as is well known, has an astonishing capacity to produce tools. This has been hopefully noted already in the 17th century, at the very rise of modern science. But it was only with the coming of the steam, gasoline, and electric engines that the problem of what to do with the tools has become a crucial one. The conflict between labor and capital, as it developed during the second half of the 19th century, was rooted in the manner in which the problem of the relation between tools and goals had been ignored.

Among those who ignored this question were the scientists themselves, who by their theoretical and experimental work created those very tools. During World War I scientists on both sides were eager participants in a propaganda warfare without ever asking the question of whether it was ethical to invent, to design, and to produce tools of massive destruction. I wish to emphasize that the sole point I want to make here is a matter of historical illustration. What should be illustrated is the fact that men of science did not feel it necessary to ask questions about the ethical dimensions of their research and discoveries. Of course, this ivory tower attitude was violently shattered with the coming of atomic weapons as the concluding phase of World War II.

It should tell a great deal about the tragic aspect of the human condition that more than a quarter of a century after the first atomic

bomb, the overwhelming portion of atomic fuel, that is, purified uranium and plutonium, is in the form of bombs or set aside for bombs. Because of the unreasonable effectiveness of science, and because of the unreasonable ineffectiveness of man, mankind is now sitting on the top of a global powder keg, while the supply of ordinary fuel is running out. Unless, indeed, the swords, to quote Isaiah, will be literally turned into ploughshares, mankind will progress, if this is progress, towards disastrous shortages of energy. Except for a most extraordinary turn of events, piles and mountains of atomic fuel will not be used to generate electricity and to desalinate ocean water, although it is an open secret that not only coal and oil, but fresh water too, is a commodity available only in limited quantities.

Yes, the unreasonable effectiveness of science and of scientific technology. Thirty years ago the best computers were gigantic, unwieldy units that needed large buildings for their housing. Today, computers can be as small as larger typewriters were yesterday. Although very small, their capacity for storage has increased by many orders of magnitude. Will their use be a blessing for mankind or a curse? Will they liberate man, or help destroy his freedom and privacy? Because computers are so small, they could be carried to the moon and back, and they were largely responsible for the opening of manned space travel. But most rockets are still launched for other purposes. Everybody agrees that this should be otherwise. And one may even question the number of rockets that should be launched at all. It is common knowledge that the troposphere can absorb only so much jet fuel, and the stratosphere can take only so much missile fuel before they get irreversibly polluted and turn the atmosphere into a deadly, global greenhouse. Everybody agrees, and yet, there is no willingness, that would match the measure of agreement. Everybody agrees that technology and ecology may be on a head-on collision course. But again, there is a painful disparity between agreement, intellectual, that is, and willingness or resolve to cope with the problem. Everybody agrees that rare animal species should be protected, but whales, for instance, are now becoming an almost extinct species. There is general agreement on that, which is even ratified in an international treaty. But then, two or three nations ignore the treaty and the general scientific or ecological reasoning. They only care about the fact that whales are great producers of animal oil and fat, and what is better, infrared photography made them easily identifiable objects from airplanes. So the hunt merrily goes on and the whales turn into a whale of a problem

to anyone who cares about cultural progress with an eye on science, or scientific technology.

The disparity between knowledge and will puts us at the doorstep of the third source of challenges and problems created by science, and especially modern science, for cultural progress. This third source is the problem of interaction between science and society. In a nutshell the problem is the measure in which society may or should respond to science, and the measure in which science may or should serve society. The problem is not new. At the first recognition of this problem a very characteristic solution was proposed in the *New Atlantis* of Bacon in the first decades of the 17th century. It represented a total subordination of society to science in the form of a regimented state ruled by a small body of scientists. Typically enough, these scientist-rulers did some of their research in utmost secrecy. They also had the supreme power of decision on the activities of each and every subordinate. Two hundred years later, around 1800, something similar was proposed by Henri Saint-Simon. He also suggested that mankind should worship in temples built and dedicated in honor of Sir Isaac Newton.

The total, if not totalitarian, response of society to science was advocated by many others in a way which usually gives itself away by its crudeness. Of the sophisticated advocacies of the same idea, one deserves to be mentioned in particular, and for two reasons. First, it was proposed because recently; second, because its message was uncritically swallowed in academic circles. What I have in mind is C. P. Snow's famous Rede Lecture of 1959, better known as *The Two Cultures and the Scientific Revolution*. From the viewpoint of composition it is certainly a masterpiece. They show Lord Snow at his literary best. He dazzles his reader with gem-like phrases and fascinating little stories. They seem to serve one principal purpose, namely, to disarm the reader's critical sense. For unless one's mind is dazzled, how can one accept a reasoning which runs like this: Among educated men, scientists know more about culture than non-scientists or literary people. Among scientists, practical scientists or engineers are more attuned to culture than are theoretical scientists. As a third step, Lord Snow claimed that among engineers the so-called inventive engineers, who usually work individually, if not individualistically, are less sensitive to culture than engineers who work in teams on the technologization of society. And finally, Lord Snow suggested that the capitalist West should take a leaf or two from the



program of the Soviet Union, because it trains far more engineers than do France, Germany, England, and the USA taken together. Six years later, in 1965, in a lecture given in Washington, Lord S n o w remarked that the Soviet Union is far more successful politically, because not a few of its leaders were originally trained as engineers, and, therefore, they think more systematically and more scientifically.

With all this, I wish to emphasize, it is not politics that I wanted to bring up. My sole purpose was to illustrate the trend which thinks that culture and progress should be entrusted to engineers, and specifically to one type of engineer. One of the troubles with this trend is that it does not even understand what it purports to explain, the so-called social roots of science. Lord S n o w himself unwittingly admitted this as he commented on the very effective use of science in industry during the late 1800's in Germany. As Lord S n o w put it, the spectacular rise of German industrial organization around 1880 or so, made for him «no social sense». The trouble with that remark is that if the meaning of making sense is restricted to the meaning of «making social sense», then many important things are not going to make sense. Among these things is science itself.

Although science heavily depends on interaction among scientists, the cultivation, the progress of science cannot be regimented, it cannot be entrusted to so-called scientific cadres or brigades. Wherever, or whenever this is done, the poor, self-defeating results speak for themselves. The soul of science is discovery, invention, coming from the spark of the individual genius, and its productivity is wholly different from that of carefully engineered production lines. Again, the dependence of science on this or that social ambience looks very small when compared with the driving force of that mysterious urge to know more about the workings of nature. Would it make sense, social or common sense, to derive P l a n c k's all-consuming curiosity about black-body radiation from the social structure of the First Reich? Would it make any sense to derive R u t h e r f o r d's struggle with radioactivity from the twilight of the British empire? Would it make any sense to connect L a n d a u's theoretical work on superconductivity with any theoretical or practical facet of Marxism, or to derive E i n s t e i n's and H e i s e n b e r g's insights from the decay of the Weimar Republic? To try to do so would make no social sense. It would make no sense at all.

For all its interconnection with society and culture, the scientific enterprise shows a baffling independence of its social matrix, and of



social conditions and structures in general. If there is anything specific and really essential which science requires from society and culture, it is non-interference with the mysterious process of discovery. As a small but tangible support of this claim, let me refer to a very recent book which I have just been asked by the editor of *ISIS* to review in its next issue. The book, *Science as a Cultural Process*, was written by Maurice N. Richter, professor of sociology at New York State University at Albany. The book, in the words of its author, «is an attempt to analyze science as a social phenomenon». Or again, in the words of the author, the purpose of the book is «to contribute to the clarification of the sociological meaning of science». The clarification consists in the admission that the scientific enterprise cannot be fitted in any of the conceptual categories sociology works with. It is certainly no small thing to hear it from a prominent sociologist that science, both historically and actually, is a unique social phenomenon. Historically, science is most unique. It came to an aborted birth in seven great cultures: Chinese, Hindu, Maya, Egyptian, Babylonian, Greek and Arabic. Science came to a viable birth only once, in late Medieval Europe, between the 13th and 16th centuries. Actually, science is still unique; science, as Prof. Richter states, is not merely a profession, or merely an occupation, or merely a method. As a social form, it is not like a family or a state. From the cognitive viewpoint, science is also rather peculiar. Therein lies among other things the persistent tension between the so-called humanistic studies and scientific investigations. This manifold uniqueness of science as Professor Richter states, «has a crucial implication: it means that science cannot, by definition, have its course of development determined by society... In the long run, society can encourage science or inhibit it, but not shape its course, or even predict its course on a long-range basis with reasonable confidence».

This lack of one-to-one correspondence between social existence and scientific endeavor, should tell a great deal about the complications which science presents to the cultural progress of society. This tension between science and society is worth exploring a little further. Let me go back once more to Professor Richter's book. He suggests that if sociology is ever to cope with the phenomenon of science, it should be in the direction of looking at science as a «cultural process». The two reasons he offers for this are as follows: 1) the term «cultural process» is vague enough; 2) according to him, the concepts of «culture» and «cultural change» do not entail goal-directedness and functionality.

Such is a strange reasoning and it provides only an additional though unwitting proof that sociology is indeed at a loss to cope with the phenomenon of science. My quarrel is not with the vagueness of the concepts of culture and progress. What is amiss in Richter's reasoning concerns the alleged absence of goal-directedness in cultural development. I think it is very easy to show that the great spokesmen of a particular culture were very much goal-directed. There was a distinct goal, or ideal, which the men of the Middle Ages or of the Renaissance tried to implement. There was a distinct goal which inspired the leaders of the Enlightenment, and there is a very distinct goal-directedness in 20th-century cultural aspirations.

At any rate, the scientific enterprise is very much goal-directed. Its goal is an ever simpler and an ever more universal explanation of nature. To realize that goal, science had to renounce the Aristotelian or Socratic search for goals. Modern sociology in order to become an «exact science» tries to do the same thing. The trouble is that the subject matter of sociology and of science are very different. Unlike physical nature, society is a matrix of conscious efforts, and conscious efforts are always made for goals. This is a most elementary common-sense experience which can only be expressed in common-sense language. This goal-directedness of the human experience is such an elementary datum that any meaningful attempt made at refuting it betrays itself by its very goal-directedness. And herein lies the fourth principal source of problems and challenges which science creates for culture. It is the challenge of the difference between mere successions of events and chains of events subordinated to a goal. It is also the challenge of the difference between the abstract mathematical formulas of physics and their explanation which, if it is to be satisfactory, must be cast into common-sense parlance.

In view of the almost esoteric abstractness of modern mathematical physics, this challenge is very great. A hundred years ago Faraday already complained to Maxwell about the «obscurity» of mathematical formulas in Maxwell's electromagnetic theory. Well, in mathematics Faraday knew little more than elementary algebra. So his complaint may be somewhat suspect. But there can be no suspicion about the crucial contribution which Faraday's common-sense ideas about electromagnetic fields made to Maxwell's theory. Fifty years later Rutherford, another giant in physics with little familiarity with higher mathematics, used to remark: «A good physical theory is such that can be explained to a barmaid». Apparently, bar-

maids are unusual persons in more than one way. At any rate, one of the big breakthroughs in atomic physics came around 1911 when alpha particles were found to be reflected by almost 180 degrees from very thin gold foils. This meant that atoms were incredibly empty structures, with all their positive charge condensed in a center, the diameter of which was 10,000 times smaller than the diameter of the electron orbits around it. The dynamic aspects of the recoil of alpha particles can, of course, be put in algebraic form, and also in that far more expressive phrase into which R u t h e r f o r d put the startling fact of recoil: «It is», he said, «as if cannon balls had been stopped and shot back by a thin paper». About the same time, in the 1910's, E i n s t e i n was already employing a personal mathematician, who cast his revolutionary though still common-sense ideas into the mathematics of four dimensional geometry. Quite recently, H e i s e n b e r g described the whole problem in words which deserve to be quoted in full: «The physicist may be satisfied when he has the mathematical scheme and knows how to use it for the interpretation of the experiments. But he has to speak about his results also to non-physicists who will not be satisfied unless some explanation is given in plain language. Even for the physicist the description in plain language will be the criterion of the degree of understanding that has been reached».

The tension between exact science, highly mathematical, and common sense, is the tension between science and society, technology and culture. Since the rise of science, but especially during the last hundred years, there have been many evidences of a one-sided approach toward resolving this problem. One of these is to turn man into a machine, on the ground that man has reliable knowledge only about quantities. H u m e claimed this, V o l t a i r e too, and another figure of the Enlightenment, B a r o n d' H o l b a c h, authored the famous phrase: «all errors of man are errors of physics». The other one-sided approach is primitive romanticism, ready to discard machines, and busy creating suspicion about the value of science. The classic originators of this approach were the Luddites and J e a n - J a c q u e s R o u s s e a u. The latter, in his *Emile*, or philosophy of education, took pains to dissuade his young charge from studying the sciences. And to discredit men of science, R o u s s e a u told Emile: «It has become more evident than daylight that the scientific societies of Europe are public schools of lies and that there are more mistaken notions [entertained] in the Academy of Sciences in Paris than in a whole tribe of American Indians».

One of the reasons for these two extremist attitudes toward science



as a cultural force is rather easy to pinpoint. Science, physical science, that is, had until the advent of the 20th century a certain monolithic appearance from the conceptual viewpoint. *Descartes* could plausibly claim that everything derived from extension. For Newtonian science, every process was mechanics, depending on physical contact between bodies. During the 19th century, the concept of energy created the same monolithic appearance for science. Moreover, this conceptual monolithic appearance was looked upon as something to be carefully cultivated. A good example of this is the story of theories of light prior to 1900. Although evidence both for the corpuscular and for the wave theory had been available, classical physicists by and large preferred a monolithic solution. Thus, during the 17th and 18th centuries, the corpuscular theory ruled supreme, whereas during the 19th century references were endless to the final victory of the wave theory. The victory existed only in the minds of those desirous to have a monolithic solution on the conceptual level.

The 20th century has greatly changed all this. One of the hallmarks of modern physics is the so-called principle of complementarity, which states, for instance, that all particles, not only light, have wave properties, and all waves would in certain circumstances act as if they were particles. In other words, modern physics rests on the conviction that a successful explanation of physical reality demands a pluralistic conceptual apparatus. Other examples of that situation are provided by the set of the so-called conjugate variables. The set is composed by such conceptual pairs as energy and time, position and momentum, both linear and angular, moment of inertia and angular velocity. In a given physical situation it is impossible to know something about one, without knowing something about the other. A perfectly accurate knowledge of one would entail a complete ignorance about the other.

From the cultural viewpoint this new pluralistic conceptual framework of exact science should be of very great significance. It should help reinforce the conviction that many various ingredients are needed to make a healthy culture capable of progressing and growing. Clearly, science is far from sufficient to provide all that is necessary for making that kind of culture. In fact, science itself is under various kinds of challenges that disqualify it from an exclusive cultural leadership. Because of its high degree of unpredictability, science often does not act as a stabilizing factor in culture; science does not contain the criteria of the proper use of the fantastic tools it creates and will keep creating; science as a social phenomenon shows a baffling uniqueness



which should discourage attempts to reshape in its terms all other cultural aspirations. All those other aspirations, arts, politics, letters, religion, depend on common-sense wisdom, judgments, and appraisals, which science itself needs as an ultimate vehicle of its explanations.

To unpredictability, to social independence, and to dependence on common-sense judgments both with respect to interpretation and to utilization, there should be added the pluralistic conceptual matrix as the fourth major feature of science, especially of modern science. Such a listing of four major features of science may be conspicuous by the absence there to any reference to the so-called operational status of scientific knowledge and theories. My reason for slighting the much vaunted operational interpretation of science can be stated briefly. One may give the definition of the operational method with Bridgman as «doing one's damndest with one's intellect». Such a definition, with which I fully agree, distinguishes the operational method only from intellectual laziness and thus any further argumentation becomes unnecessary. Or one may define the operational method as is usually done, as an ever more convenient reshuffling of mathematical functions which tell us nothing about reality. But such a definition tells us little more than nothing about science itself as it actually does exist, is practiced, and is carried out. On the other hand, the four features which I have listed are about science as it actually does exist, is practiced, and is implemented.

Last but not least, a growing awareness of those four features should entitle us to some modest optimism. On the basis of such an awareness one may hope to find a broad and common basis in which a *Gleichschaltung* of culture by science might be effectively forestalled. I said «modest optimism» because the danger of that *Gleichschaltung* is always present, ever since it was first spotted 2400 years ago by Socrates. True, he overreacted to the atomism of Leucippus and to the mechanistic physics of Anaxagoras. In their atomistic and mechanistic physics, there was no room whatever for purpose, goal, and values. To vindicate purpose, Socrates declared in the *Phædo* that everything, even the fall of stones, had to be for a purpose. The result was the historic sidetracking of physical science by Aristotle. But one thing cannot be denied to Socrates. He saw that there were problems, very serious problems, about human culture vis-à-vis science. While Professor Rowland at Johns Hopkins ignored his problem, the graduate students, Socrates refused to ignore the broad problems of his own ambience. By making this Second International Symposium pos-

sible, the Hellenic Society for Humanistic Studies, and its President, Prof. V o u r v e r i s, followed in that great Socratic tradition of awareness of cultural problems. Socrates kept saying, and this was the essence of his famous method, that all solutions depended on one's awareness that there were questions to be answered. My lecture was not meant to provide answers but only to point out directions in which the answers were indicated by the problems themselves.

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