THE CHANGING FACE OF PHYSICS

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It is a great honour to address this distinguished gathering of scientists and guests at the close of a day of introductory lectures on critical phenomena, all the more so that the reasons for my being selected as after-dinner speaker, and therefore the choice of my theme, are not obvious. I have been a theoretical physicist long enough, however, to learn some of the tricks of our trade, for example how to guess tentative explanations for obscure events, or how to pin down the theme of a lecture half way through the lecturing period.

My theory for being here to-night shall be that I have had quite a vivid interest and a modest activity in several domains of physics, including some of the problems which occupy you this week. This is a dangerous theory, however, not only because it may be incorrect, which would not worry me too much, but because its systematic application would make life quite impossible in the world of to-day. How could a physicist with an interest in several fields still attend all the conferences which he would profit from? Thanks to the labour of the European Physical Society, we now know how incredibly numerous the physics meetings have become also on our side of the Atlantic. The latest EPS list offers in Europe a choice of 9 meetings for last week and 8 for this week, not including the Battelle Symposium. All subjects are very specialized. Thus, you could have gone last week to Brussels for mass spectroscopy or to London for measurement techniques of spectroscopic data, while right now hadron spectroscopy is being talked about on Lake Balaton and electron spectroscopy in Uppsala. Quite a spectrum of spectroscopies for two brief weeks. And, as we all know, conferences are not enough for physics communication, summer schools are available in addition. According to the same EPS list, not less than 25 are held this summer in Europe. Do we physicists not need a non-proliferation treaty of our own?

The fact of the matter is that the growth of physics research has been staggering in the last 20 years, thanks to ample public funding and sponsorship. Even specialization does not discourage high level sponsorship, as illustrated by the International Congress on Thin Films announced

Every branch of physics, big or small, had a more than fair chance to grow in the last two decades, most grew quite effectively, and a few led to truly important advances. Of course, none of the post-war achievements even approaches in fundamental significance the great discoveries of relativity and quantum physics which mark the first 30 years of our century. But such momentous steps are quite exceptional in the whole of science history, and it is neither funding nor official sponsorship which trigger them.

What intensive government support of research can achieve seems rather to be an acceleration of the normal growth of physics through experimental fact finding and improved instrumentation as well as through theoretical analysis and speculation. In addition, it makes the whole difference between building or not building certain giant instruments like large accelerators or reactors, large detectors, observation stations in outer space and other expensive objects characteristic of big science.

I am now far enough in this address to know what I am talking about. My theme is the changing face of physics, how different it is now from some 20 years ago when, as a young mathematically minded theorist, I was increasingly fascinated by its fundamental problems and by the marvellous power of mathematics to analyse and predict its phenomena. The changes are profound indeed, not only by the new knowledge acquired, but also in the style and atmosphere of research. In trying to summarize the latter, I am tempted to use two ugly words. Physics has gotten "chemicalized", and it has gotten "institutionalized". Let me explain what I mean, which is not quite as repulsive as the terminology I use.
It seems to me that physics now looks more like chemistry, in the sense that, in percentage, a much larger fraction of the total research activity deals with complex systems, structures and processes, as against a smaller fraction concerned with the fundamental laws of motion and interaction. This colloquium is a good example. Surely, we all believe that the fundamental laws of classical and quantum mechanics, of the electromagnetic interaction and of statistical mechanics dominate the multivariable transition and critical phenomena you discuss this week, and I presume that none of you expects his work on such problems to lead to modification of these laws. You know the basic equations better than the phenomena. You are after the missing link between them, i.e. the intermediate concepts of mathematical or phenomenological nature which should allow a quantitative understanding and prediction of the phenomena on the basis of the fundamental equations. This is a difficult and fascinating task, ranging from the strict mathematical treatment of simplified model systems to the intuitive hunt for phenomenologically relevant concepts and parameters. You are also after new and curious systems and processes, because the wealth and variety of behaviour found when one arranges electrons and nuclei in different ways turn out to be absolutely amazing, and each new behaviour has something to teach us.

Most of present physics is of this type, be it concerned with solids, liquids, neutral gases, plasmas or atomic nuclei. Especially through the discovery of complex but remarkable systems and effects, many significant applications to technology have been found, again in analogy with good old chemistry. As a consequence, industry and government like physics more than in the past, and we are back to our earlier story of generous funding and prestigious sponsorship. This in turn brings us to the "institutionalization" of physics. The cause is big money, the consequence is elaborate management of research and mushrooming of scientific committees.
It would be a mistake to believe that funding and sponsorship of advanced research by the state is a recent development. Learning and research were generously patronized by the Ptolemaic dynasty which ruled Egypt after the conquests of Alexander the Great, relying heavily on brain-drain from Greece and elsewhere. Thus Strato, a distinguished natural philosopher and the second successor of Aristotle as head of the Lyceum in Athens, was successfully attracted to Alexandria early in the 3rd century B.C. by the first Ptolemy, not a bad act of scientific policy for a former general of Alexander. It is probably under the second Ptolemy that the famous Museum, or "Temple of the Muses" was founded with state money in Alexandria. In essence, it was a research university with a small amount of teaching. Its head, somehow the ancient analogon of our University Presidents or Laboratory Directors, was a high-priest. In addition to its celebrated Library, the Museum had lecture and study rooms, dissecting rooms, an observatory, a zoo and a botanical garden. The investment, as we know, was a good one; among other things it produced the Elements of Euclid and the first heliocentric system of astronomy due to Aristarchus of Samos (unfortunately superseded around 100 B.C. by Hipparchus's return to the geocentric system). Also the list of graduates is impressive; Archimedes is one of them 1).

But I doubt that the Alexandrian Museum worried too much about efficient management of science, or that some of its senior scientists were spending half of their time in committee meetings. Why is our world of big science so fond of these things? Of course, one must accept that some of it is necessary if the flow of money is to be reasonably well used. After all, many decisions must be taken which no honest scientist, however competent he may feel, would like to take alone. What should you do when you have to make such a decision? You consult with other competent people in order to reduce the probability of mistake. If you have to do this 5 times a week, you set up a committee. Especially in Europe where science will only survive if it becomes international, the need to represent the interests and traditions of various countries is another factor in the multiplication of committees and the intricacies of management. Then,
research at the frontier now requires very huge instruments. This leads to extreme centralization, basically a bad thing for creative research. Some of all this is unavoidable, some of it is encouraged by the growing influence of an emerging class of scientific executives. Neither they nor the scientists have yet discovered how to reconcile managerial efficiency with scientific creativity.

Some dangers of the system are indeed already clear. Executive responsibility or membership of powerful committees confers to individual scientists an artificial increase of intellectual authority; they are rated more on the importance of decisions in which they participate than on their personal competence and achievements. Some of them grow so fond of administration and committee business that they keep in touch with science only through this business and imperceptibly lose the very foundation of their scientific judgment. While these evils can be effectively counteracted by turnover in executive tasks and committee membership, another danger is more subtle and more difficult to avoid. Some of the truly important decisions, let say on the best choice for a new large instrument, or on how and by whom an adventurous but costly experiment should be performed, require in an essential way the kind of purely intuitive, synthetic and qualitative judgment which makes a good scientist move in the right direction without his being able to explain why. It is very hard to put such judgments in the analytic form which carries collective approval, and decision making by committees will unavoidably be poorer by the lack of them. Crystal gazing is part of the art of making wise scientific choices, and no amount of staring at documents, graphs or budget figures can replace it. Hence, for some of the most important scientific decisions, informal, almost private discussion and consultation will be more reliable than official committee procedures. But how to circumvent the committees once they exist? And then, speaking biologically, they are so easily born and never die!
But back to the contents of present day physics and its chemical flavour. The bulk of it, as I mentioned earlier, consists of almost infinite variations on the themes of mechanics, statistics and electromagnetic interactions. These studies are very rich, important and worthwhile, just as chemistry is. I find it difficult to follow F.J. Dyson who, speaking at the dedication of a new physics laboratory in Princeton, recommended cosmic rays, biology and astrophysics as orientations of research. That some physicists move in these directions is excellent. But, as just mentioned, the more classical lines of physics research remain highly interesting and productive, and where will they be pursued in the spirit of pure science if not in the universities, where they must anyhow remain the backbone of physics teaching?

I have not said anything yet about the great open questions on the basic interactions. The fact that they are harder and that fewer people work on them do not make them less fundamental. If you bear with me a little longer, I shall devote my final remarks to them.

Experimental work on fundamental laws of physics proceeds along two main lines: push to higher precision in the study of known states of matter, or push matter into unknown states. In both directions, the hope is to find new effects revealing new features of the basic laws of motion and interaction. The high precision line has given one fundamentally new result since the Lamb shift of hydrogen triggered in 1947 the renormalization technique for quantum electrodynamics; it is the 1964 discovery of CP violation in neutral kaon decay. For the rest, it impressively confirmed the validity of relativistic quantum mechanics and renormalized electrodynamics. The line of forcing matter into unusual states is most spectacularly represented by high energy physics. The main discoveries are the big new world of hadrons with its wealth, complexity and curiously broken symmetries on the one hand, and on the other the V-A coupling and the second neutrino for weak interactions.
The great disappointment is the absence of any visible progress toward a fundamental theory of strong and weak interactions. For the latter, we can at least recommend a crucial experiment, neutrino electron scattering at center of mass energies of a few hundred GeV; the difficulty is that it will probably be decades before we can perform it. For hadron physics, crucial experiments do not seem to exist, data gathering and model making are the order of the day. The pessimists may say that the models will not be better than the epicycles of Ptolemy the astronomer (2nd cent. A.D., not a member of the Ptolemaic dynasty mentioned above). But the optimists can retort that the remarkable mathematical regularities which are extracted from the data may turn out to be as significant as Kepler's discovery that the orbit of Mars is elliptic. As to the future Newtons of the strong and weak interactions, one hopes that they will come in due time but it might be naive to assume that they are already born.

It may be that the trouble lies in our inability to force matter in bulk into extreme conditions of density and energy, and experiment with it there. What would we dream of in this direction? Even the heaviest of super-heavy nuclei are probably not very useful for fundamental laws of interaction. Now that superfluidity and superconductivity are understood in principle, bulk matter at very low temperature would not be our favourite either. Very high temperature at normal densities would become quite exciting if the average energy per nucleon is of GeV order or more, but this means a temperature \( T \gtrsim 10^{12} \text{K} \). At lower temperatures, the really interesting densities would begin around \( 10^{16} \text{ g cm}^{-3} \), when Fermi motion of baryons reaches the GeV range. Clearly, what we talk about is high energy physics of bulk matter, and looks perfectly utopic for laboratory conditions. In fact, the future here may lie in the heavens, through astrophysics, where we also can expect to learn something more about gravitation, the fourth fundamental interaction on which experimental research is just making a new and welcome start.
However this may be, and while we have found no reason to doubt relativity and quantum mechanics, the mysteries around the basic interactions remain fascinatingly deep. Barring unlikely strokes of good luck, we shall still need much patient work at high energy accelerators, and much patient watching of the stars, detecting their photons, neutrinos and gravitons, before we can hope to grasp the true picture. The experiments deal with such exotic phenomena, so far removed from normal conditions and so hard to measure, that the pace of progress can only be slow. Also this is a significant change in physics compared to earlier decades. Machines and detectors have grown to gigantic dimensions, but the scope of individual results is mostly very modest. We have no right to fool the taxpayer on this, because he pays our bill. And we should not fool ourselves nor our students, which may be harder.

But more important than the rate of progress is the undeniable fact that progress is being made, and we should constantly recall and re-assert that elucidation of the fundamental laws remains the most essential task of physics. Our advanced society can certainly continue to afford the price of this pursuit, at least if, for the largest instruments, a sufficient selectivity is exercised and unnecessary duplication is avoided, so that in the long run a world-wide policy will become unavoidable. In a recent history of the Rochester Conferences, Marshak describes how an attempt was made in this direction as early as 1960 for high energy accelerators and how a cold war incident doomed it to failure 3). He suggests that the time has come to take this matter up again. With CERN, high energy physics has pioneered in European collaboration almost 20 years ago. It is reasonable to expect that this field will be well suited to pioneer in world collaboration and that it will do so long before the end of the century. A very hopeful sign is the successful collaboration of CERN with the 70 GeV accelerator laboratory at Serpukhov near Moscow. There is a chance here for much more ambitious initiatives, and we should hope that it will be taken.
All this points to yet another change in the face of physics, a profoundly significant and entirely positive one. Twenty years ago, in the public mind, physics was unavoidably associated with nuclear weapons and the cold war. Now it gets more and more associated with world-wide international collaboration. Ironically, despite its many committees and complicated management, it is big science which is bringing this about by the very logic of its development. Isn't it obvious indeed that we should have one day the very best brains of all developed and developing countries work together around the most advanced and costly research instruments which humanity will be able to afford and build to pursue its quest for fundamental knowledge?

One more question must be faced when we talk about the future. The best physics is done by young people, and we should therefore be concerned by Heisenberg's remark that most of the younger physicists show little interest for what he so aptly calls "die grossen Zusammenhänge", something like the unifying relationships 4). This is certainly true, especially in comparison with the generation of scientific giants to which he belongs, and the unquestioned reign of specialization is unfortunate indeed. Not only does it foster intellectual shallowness, but it ignores the profound methodological unity of physics. Over and over again we see how techniques and concepts developed in one branch of physics find new and fruitful applications in others. The present trend to extreme specialization makes such cross-fertilizations ever more difficult to achieve, and one should try to counteract it.

Still, in another respect, the lack of proclaimed concern about the unifying relationships is also understandable and should perhaps not be the source of too much pessimism. Most young physicists simply do not work on the great problems of the basic interactions, and those who do learn soon that the road toward the ultimate goal is long and hard, the fare per mile very high, and the daily scientific rewards quite limited. When progress is so slow, it may be better not to talk constantly about the glorious destination, all the more so that one travels without map and
often falls victim to mirages. But here again, what really matters is the basic believe in progress, the basic conviction that in due time the fundamental laws can and will be elucidated. This conviction is shared by young and old alike, and now as in the past it provides the motivation and driving force to carry on.
REFERENCES


