# SEEKING TO RECOGNIZE ORDER IN NATURE

**Do scientists seek material rewards? What are the changes in modern scientific work? Has the scientist a sense of responsibility to society? These questions were dealt with in a talk given at the Trieste Symposium on Contemporary Physics in June by E.P. Wigner, 1963 Nobel Laureate, one of the "Grand Old Men" who took part in an evening series under the Title "From a Life of Physics".**

I wish to tell you about what I believe is the fundamental motivation of the scientist. how the life of the scientist has changed during the period I have tried to be one, what he can expect from and what he should do for the society which enables him to enjoy a life of physics.

First, I want to mention a few sources from which I learned a great deal that is relevant to my present subject. My first teacher was Polanyi but if I enumerated everything that I learned from him I would never get any further. The next source was Wilhelm Ostwald's Grosse Männer. This is a collection of stories of several great scientists, with introductory remarks of a general nature giving a distillate of universal verities which he obtained when studying the stories of his heroes. Next in the line of my recollections are three long walks with James Franck, in Princeton, during the early but already gloomy days of the Hitler regime. We discussed just those questions about which I wish to talk today. Last there have been some recent conversations with historians and philosophers of science, including Dr. Mehra who is among us now, which helped me greatly to clarify my views.

## REGULARITIES IN A COMPLICATED WORLD

A friend of mine likes to quote me as saying that what I wish to accomplish in life is to leave a bit more order and understanding behind than I have found. I do not remember when I told him that but there is a great deal of truth in it. We have a complicated world around us, full of unforeseeable events, and it is calming to the soul to find and know something that is orderly and unchangeable. This is not all. If we think a little further about our relations to the world, we soon realise that if we could not find regularities in it, we could not influence the events around us. The regularities in question are connections between subsequent events such as that this eraser in my hand will fall down on the table if I let go of it above the table. If there were no such regularities, we could not exert influence on the events — I could not produce a thumping noise with the eraser and I could not see it jump if I

did not know that it will have this effect when I let go of it. Hence, the regularities make life possible in the sense I believe we understand life, to have an influence on events.

Of course, the regularities in which we physicists are interested are much more subtle. Nevertheless, I believe that the basis of the motivation, and of the striving to recognise some order, is common to all living beings and that, in fact, it is closely connected with the essence of life.

The question then presents itself "what are the limits of our search for regularities?". Would we be happiest if the regularity were complete so that we could foresee everything and know and understand everything? If the preceding analysis of the causes for our search is correct, the answer must be negative. If the order were complete, if we could foresee everything, we would be again in the situation in which we could not influence anything, in which all would be determined and our will and our desires would have no way to manifest themselves. Hence, in this sense, the existing world is the best one: there are some regularities, and we need them for what we call life. But there are plenty of irregularities, and they are equally indispensable for what we call life.

## "UNREASONABLE ACCURACY" OF PHYSICAL LAWS

This situation is magnificently reflected in physics. We have initial conditions which show no regularities, and there are laws of nature which express miraculously precise regularities. There is, however, a much more sharp distinction between the domain of regularities and of arbitrariness than we had any reason to expect and this is, perhaps, the most remarkable result of physical theories. Charles Pierce, the philosopher, commented on the unreasonable accuracy of physical laws and now Dr. Dirac has re-emphasized the fact that, offhand, we had no reason, and no indication, to expect laws of physics to be as accurate and, in a sense, as simple, as we have found them to be. Thus, in a deeper sense, science, far from having abolished miracles, has recognised and drawn attention to a miracle of overwhelming power which holds us scientists in awe and in bondage. More so, much more so, than people in other professions.

The desire for an order manifests itself not only in our striving to recognise regularities in nature, in the succession of events, but also in the structures which we have ourselves created, our theories and concepts. Mathematics is entirely devoted to the search for regularities in the relations between concepts, created just for this purpose. But physical theories also have an intricate structure, and the elucidation of this structure, for instance the recognition of the parts of the theory which are responsible for a certain conclusion, is also providing us with a great deal of satisfaction. The discovery of Klein and Noether, that a conservation law for energy is valid in any mechanics with a time independent Lagrangian (a theory of motion) must have given them a feeling of elation, the feeling of being suddenly cleaner and freshly bathed.

Those of us who were not favored by a discovery such as that of the elementary electric charge and the existence of its carriers in metals, or of the equation that most adequately describes this carrier, can derive, and have derived, enough satisfaction for a lifetime by having clarified, perhaps not the structure of the events, but at least the structure of the theories which are a condensation of the regularities between the events. The pleasure experienced in this way has much in common with the pleasure of the mathematician. It is a real pleasure, nevertheless.

## CONSEQUENCES OF THE SCIENTIST'S WORK

The scientist's activities satisfy not his desire to influence the world around him, but a sublimation, an ideal of this desire. I believe this is true. Nevertheless, it is clear that, surprisingly frequently, he does influence the world around him. Without modern science, we would have no radios, no television, no automobiles for the students to makes barricades of, no antiballistic missiles. These are very real consequences of the scientist's activities. Nevertheless, I do hold on to what I said because effects are consequences of, rather than motives for, the scientist's activities. In fact, some of our fellow scientists are unhappy when they learn that their results and conclusions have been used to produce a new drug, or some new equipment. They feel that their sublimated desires are somehow condensed and they feel that their pure and sublime science has been debased by being applied to the benefit of the society which should support them without reaping such benefits. I do not agree with this attitude, but surely it proves that the pure scientist's motive is a sublimation of the instinctive desire to influence the course of events, and not the desire itself.

Are there some negative traits in the makeup of the scientist which make it easier for him to turn away from the goals which most of his fellow citizens pursue, to refuse the participation in the quests which inspire most of his friends and acquaintances?

It seems to me, but I am less than certain in this, that his desire for influence is sublimated to such an extent that the common, everyday desire for power and influence is smaller than it is on the average. Until a few years ago, I believe that few of us thought much about the unfortunately very widely spread craving for power and influence. When the frequency of this craving dawned upon me, about six or seven years ago, I brought up the subject with colleagues, and with friends outside the world of physics. Most of my colleagues did not understand what I was speaking about, and most of my non-physicist friends did not understand why I was talking about the matter, it was such an obvious fact to them. I then recalled many observations on the subject which I had heard in the past, including my father's explanation of the reason for so many people's coveting of great wealth, and a number of events which had been mysterious to me became clearer.

At any rate, I believe that at least the scientists who are my contemporaries had a good deal of inclination towards retirement from the struggles which go on in our society, a certain fondness toward the monastic

way of life and that indeed this was characteristic for those who chose Science as their vocation. Franck said, on one of our joint walks, that we scientists use science as an opiate enabling us to forget what goes on around us and to disclaim responsibility therefor. The young scientist of those days wanted to learn in seclusion, create new ideas in solitude and retirement.

## CHANGES IN SCIENCE

Whether this characteristic of withdrawal and a penchant for a monastic life, is true of the present day scientist to the same extent as of the scientist thirty or more years ago, is not sure. This brings me to the next subject, the great changes that took place in science during my own Life of Physics.

I believe I was 17 when my father asked me what I thought I would do with my life. I expressed the desire to become a scientist, a physicist by preference. He must have suspected that and, at any rate, his answer was "Hm, How many positions for physicists are there in the whole of Hungary?". I gave a somewhat exaggerated figure and said "Four". He overlooked my exaggeration and asked me whether I expected to get one of those four positions. We agreed that it might be best if I studied something of greater practical value such as chemical engineering and, indeed, this is the subject in which I acquired a degree. However, during the relatively short period which elapsed between my 17th year and the granting of my degree, the world changed a great deal. First, it shrank, the distance between Germany and Hungary decreased, not so much in travelling time as spiritually, and the idea to assume a position outside of Hungary did not appear so absurd any more. Second, the number of positions for physicists increased greatly. Polanyi, my doctor-father, had a serious conversation with my father and myself pointing out that a career in science did not appear something romantic any more and that we should seriously consider it. Indeed, the status of the scientist had changed enormously during the six years in question. In 1919 he was regarded, at least in Hungary, as a venerable but very queer bird. By 1924 it became a career which did mean a great deal of retirement from the world but nevertheless a career which could be seriously considered in Germany. Even in Hungary the smile it evoked became a smile of tolerance.

This development has continued ever since. Maybe I am old-fashioned when I expect people chosing a career in science to do this without the expectation of obvious outside rewards, in the spirit of craving for a life of learning and, hopefully, creativity. The fact is that many of our young men choose the scientific career in just this spirit but the fact is also that many others expect outside rewards, influential positions, high distinctions and a life of what we call success. I do not know the spirit of which group will ultimately prevail. Perhaps there will be a mixture of the two, perhaps those in the more self-asserting group will eventually leave science and assume administrative positions inside, or outside, academic life. But surely the spirit and the traits which were taken for granted in a scientist earlier in this century cannot be taken for granted any more — the scientist of today is, in his attitude toward life, more similar to his non-scientist contemporary than was the scientist of thirty years

ago. This is neither necessarily good, nor necessarily bad, it may be even less of a change than it seems to me, but there surely is some change. The self-assurance of today's physicist is very different from the attitude his older colleague exhibited in his youth — he was almost apologetic for the unconventionality of his interests and strivings.

## EMERGENCE OF BIG SCIENCE

Another very significant change is the emergence of big science, that is laboratories with several thousand members. We all feel that being a scientist in such a laboratory is very, very different from being a scientist who works in solitude, that the use of a 70 Bev accelerator by a team of a score of scientists, approved by the administrative committee of the accelerator, is very different from the contemplative life that was the essence, though perhaps not the whole, of science as late as the early part of this century. I do not want to discuss what Alvin Weinberg has called big science in detail. It is clear that is has accelerated the acquisition of knowledge enormously. It is also clear that it needed the less retiring scientist, with the more conventional and more aggressive attitude which I described.

Having spoken about the years preceding my becoming a physicist, it would be the right thing to continue and to tell you about my development and the work on which gave me most pleasure. However, it would be difficult to review my work. Somebody said that I have made infinitesimal contributions to an infinity of subjects. This is, of course, an unjust accusation; I have not contributed to infinitely many subjects.

## COURAGE IN GUESSING

My doctoral dissertation was an attempt — which has turned out later to have been correct — to calculate the rate of chemical association reactions, such as the one mentioned in this symposium by Dr. Salpeter: two hydrogen atoms colliding and forming a molecule. There were two problems. If we consider the collision of the atoms in the center of mass-coordinate system, the two atoms have to form a molecule at rest and the energy of the molecule is quantized. It is then infinitely unlikely that the kinetic energy of the atoms be just so large that the energy of the system coincides with one of the energy levels of the molecule. Born and Franck, in a joint paper, also made this point. It was concluded, therefore, that the association reaction was infinitely unlikely. The situation was worse than this: the angular momentum of the molecule was also quantized and it was equally unlikely that the atoms which collide have just the right amount of angular momentum about their center of mass. All this was, of course, years before quantum mechanics was discovered. It would have been, therefore, natural to conclude that simple association reactions are impossible, or have zero probability, had there not been a wealth of experimental information available, from actual chemical reactions, that they actually do take place. The solution to the problem which I proposed, on the basis of experimental information and the study of the establishment of the

chemical equilibrium was then (a) that the energy levels are not sharp but have a certain breadth and that the reaction can take place if the energy of the colliding pair of atoms falls within that breadth and (b) that the limitation with respect to angular momentum should be disregarded, the angular momentum of the pair being filled up automatically and mysteriously to the next integer multiple of Planck's quantum h. These two prescriptions then guaranteed the proper establishment of the chemical equilibrium of dissociation. They also give a fair picture of resonance reactions in general and I remained interested in these reactions, as most of you know. I told this story because I thought you might be interested in some other corner of the picture of the frame of mind of people in the pre-quantum mechanical days. One had to guess more at that time than demonstrate and one's courage in guessing was much greater than it is now when the inadequacy of the available theory is not established. Nothing that I said contradicts, of course, Dr. Salpeter's conclusion that the simple association reaction, the forming of an $H$ molecule from the collision of two H atoms, is a very, very unlikely process: the energy levels of the $H$ molecule are narrow and far from each other. I calculated the rate of the formation of the molecule as a result of a collision of three H atoms very much later.

## THE PLEASURE OF EXPLORATION

Having heard the story of one of my calculations, I am sure you do not want to hear the story of all the others. I really cannot tell which gave me most pleasure. I always enjoyed the work and when and if I was able to conclude it, I always felt that there was a bit more order in my mind and thinking. The same was true, more often than not, after reading an article which I could understand and in many cases I felt afterwards a high elation, almost a euphoria. Furthermore, the pleasure of exploration has not diminished in the many years that I have enjoyed it. Age brings a happiness and relaxation and as long as one is not constantly reminded of one's failing powers, is the happiest period of life. Let me just add that, except for the concern for the success of the work, and the deep concern about the eventual outcome of the war, the work for the government during the war was also interesting and satisfying. The friendships I formed as a result of association with other physicists is also a continuing source of pleasure and satisfaction.

The last subject on which I wanted to share some thoughts with you, is the relation between scientist and society.

As long as there were four physicists in a population of seven million, this relations was not of major importance. Now, however, when the U.S., for instance, spends 20 billion dollars a year on research, out of a total national income of 800 billion dollars, so that, directly or indirectly, about 5 million people work on research of one kind or another in a country of 200 million, the importance of the question has a different order of magnitude. This remains true even if you find one reason or another — and there are such reasons — to change any numbers by a considerable factor.

What I am advocating is that we realise how much we owe to society. It keeps us — and if I look around myself I find that it keeps us in luxury — for doing what we want to do anyway, for doing what gives us most pleasure. I believe that we should show, in return, some helpfulness and be less than annoyed if one of our conclusions or discoveries finds a practical application. The book of Ostwald points out that almost every one of his Great Men has, at one time or another, devoted time to some practical problem, to the combatting of disease, the increase of production, or something similar. He also points out that almost every one has devoted, usually toward the end of his career, time to advise his government on questions of the administration of a scientific enterprise, and on the possibility of practical applications thereof. We, who are generously supported by our society, should show a sense of humility and gratitude rather than contempt for the non-scientist. I know that it can be argued that society derives benefits from supporting us — but so does the man who jumps into the water for rescuing another. I find, therefore, statements of the sort "the worth of the society can be well judged by the extent to which it supports its scientists adequately" simply repelling. Such statements naturally provoke counterstatements like that of Professor Harry S. Johnson's. He said: "The argument that individuals with a talent for research should be supported by society differs little from arguments formerly advanced in support of the rights of the owners of landed property to a leisured existence, and is accompanied by a similar assumption of superior social worth of the privileged individual over the common men". I believe we should do all we can to avoid such criticism; the resulting confrontation can do only harm, harm to both society and to science, particularly big science.