SCIENCE

VOL. 74 — FRIDAY, OCTOBER 2, 1931 — No. 1918

SCIENCE: A Weekly Journal devoted to the Advancement of Science, edited by J. MCKEEN CATTELL and published every Friday by

THE SCIENCE PRESS
New York City: Grand Central Terminal

Lancaster, Pa. — Garrison, N. Y.

Annual Subscription, $6.00 — Single Copies, 15 Cts.

SCIENCE is the official organ of the American Association for the Advancement of Science. Information regarding membership in the Association may be secured from the office of the permanent secretary, in the Smithsonian Institution Building, Washington, D. C.

THE GROWTH IN OPPORTUNITIES FOR EDUCATION AND RESEARCH IN PHYSICS DURING THE PAST FIFTY YEARS[1]

By Sir J. J. THOMSON

HONORABLE PROFESSOR OF PHYSICS, THE UNIVERSITY OF CAMBRIDGE AND THE ROYAL INSTITUTION, LONDON

DURING the last year we have lost by the death of Professor Albert Michelson a physicist whose work was of quite exceptional importance. The famous experiment known everywhere as the Michelson-Morley experiment has, since it is the basis of the theory of relativity, been largely responsible for the trend of physical thought during the present century. It is a very striking example of the great philosophical consequences which can result from what might seem the rather mechanical process of improving the precision of physical measurements; the importance of the experiment depended entirely on the accuracy of the measurements being great enough to detect with certainty changes amounting only to one part in a hundred million.

The additions to our knowledge of physical phenomena and the number of new ideas introduced into physics since the last anniversary meeting have been so great and cover such a wide range that it would be impossible in the time at our disposal to give an account of them which would be at all adequate or even intelligible to those not already acquainted with them. There are, however, advances of another kind of great importance to the progress of physics which lend themselves more readily to a less inadequate treatment in such an address as this. Such advances are the increase in the opportunities for teaching and research in physics caused by the foundation of many new laboratories, the increase in the attention paid to the teaching of physics in our schools, the endowment of research workers and the increase in the opportunities for these to obtain remunerative employment, the increased recognition of the importance of research in industry and last but not least the improvements

[1] Address of the president of Section A—Mathematical and Physical Sciences, British Association for the Advancement of Science, London, September, 1931.

made in instruments used in research and the increase in the magnitude of the forces, mechanical, electric and magnetic, which are now at our disposal. The physical laboratories in the eighteenth century and the first half of the nineteenth were in the main collections of instruments suitable for experiments to illustrate the lectures of the professor, and the trouble taken over these experiments was, I think, comparable with that taken now. Thus, Wollaston, who was Jacksonian professor at Cambridge at the end of the eighteenth century, is said to have shown over 300 experiments in his annual course of lectures and at a later period Stokes, in his lectures on optics, showed experiments which have not been excelled, for beauty, for educational value, for simplicity or for certainty.

In spite of the fact that until the end of the last century there were but few laboratories available for research much scientific work of the highest importance was accomplished. This, to a very great extent, was due to men who made their own laboratories and bore themselves the cost of the experiments. Thus, Joule made his experiments on the mechanical equivalent of heat in his house at Manchester, Stokes, like Newton, made fundamental experiments on optics in his college rooms, Spottiswoode, Huggins, De la Rue, Lord Rayleigh, and one who has had a long and intimate connection with the British Association—Dr. E. H. Griffiths—also made in their own laboratories and at their own charges additions of great value to physical science.

These men, like Kelvin and Maxwell, had not passed through any course of instruction in practical physics, for no such courses were available; they were in this respect self-taught. Most of them had learned how to use their hands by having had when young some hobby, such as using a lathe, or dabbling in chemical experiments or photography. This training seems to have been effective, for no one can say that their work is amateurish. This raises the important question, may not the present practice in which our advanced students spend a great deal of time in acquiring dexterity in the use of instruments of all kinds be a wasteful one, and could not the student who has learned how to use his hands, has a good knowledge of physics and some practice in making accurate measurements, be trusted to master in a short time the technique of any instrument he might require in a special investigation?

In the early seventies when I first began to study physics at Owens College, Manchester, there were only six physical laboratories in England—the Royal Institution, the Clarendon Laboratory at Oxford, those at University and King's Colleges and the one at the Royal School of Mines in London and one at Owens College, Manchester; there were four in Scotland, one in Ireland and none in Wales. Now the number of physical laboratories at the universities, university colleges, schools, and institutes of technology at which instruction is given in physics, is considerably more than 300. Nearly the whole of this increase has occurred since our last anniversary meeting. The contrast between then and now would be even more marked if we took into account the size of the buildings. Some of the laboratories in those early days were very small affairs. Professor Ayrton described Sir William Thomson's laboratory at Glasgow as consisting at one time of one room and an adjacent coal cellar. When I was at Owens College, Manchester, though it was one of the first places where instruction was given in practical physics, there was no separate laboratory, but only a few rooms and little apparatus. Though the laboratory was small, it was large enough for the few students who worked there, and these had much more freedom and more initiative than would be possible with the large number of students that have now to be provided for. We were allowed to choose our own experiments, we fitted up the experiments for ourselves and if they did not work we tried again, we were not limited with respect to time and we could follow up any point of interest we happened to come across. This rather happy-go-lucky method would be quite impossible with large classes, but it was more interesting and, I think, a better training for research than the highly organized classes which large numbers necessitate. At any rate, I am glad that I came under the old and not under the new system. The new method, however, besides being inevitable has some very decided advantages. There are students who are quite immune when in the lecture room to infection from any physical idea and only get a grip of physical principles when these come before them in the concrete form of an experiment which they make with their own hands; these men learn their theoretical as well as their practical physics in the laboratory, and the nature of the experiments, their number and their sequence are of first-rate importance; all this requires a great deal of organization.

SCIENCE TEACHING IN SCHOOLS AND UNIVERSITIES

The movement for including science among the studies pursued in our universities and schools was born almost at the same time as the British Association and the men who took the most prominent part in it, Adam Sedgwick, Herschel and, above all, Whewell, were closely connected with the association. It was some time before the movement led to definite results, but in 1849 the University of Cambridge determined to establish a natural sciences tripos. It was not at first an avenue to a degree and the subjects were limited to those in which there were professorships in the university, *viz.*, chemistry, mineralogy, human anatomy and physiology, botany and geology. The first examination was held in 1851. There were only six candidates, four were placed in the first class with Professor Liveing at the top.

In 1853 the report of the commissioners appointed to report on the studies at Oxford and Cambridge appeared and contained recommendations in favor of further opportunities for the study of science at the universities. In 1864 the Royal Commission appointed to report on the seven most important public schools recommended that all the boys should receive instruction in science during part at least of their school career. This was not before such a recommendation was needed. For science in schools at that time has been described as being "regarded with jealousy by the staff, with contempt by the boys and with indifference by the parents."

In 1866 there was a valuable report on the teaching of science in schools by a committee of the British Association. In 1867 a commission appointed to consider the education given in those schools not included in the reference to the first commission reported in favor of including science in the curricula of these schools. It should be mentioned that before the publication of these reports, J. M. Wilson, who died only a short time ago, had started science teaching at Rugby, where he was a master.

The fight for the introduction of science teaching in our universities and schools was long, and at times bitter; for some time little progress seemed to be made, but in 1881, the period of our last jubilee, things began to go with a rush. Owens College obtained, in 1880, a charter as Victoria University and could give degrees to its students, and in the early eighties Mason's College, Birmingham, with Poynting as professor of physics, University College, Liverpool, with Lodge as professor, Yorkshire College, Leeds, with Rücker as professor, came into existence and later became universities.

The number of schools where science was taught rapidly increased and as a result of this so did the number of science students coming to the universities. Very clear evidence of this is the fact that in 1881, thirty years after the foundation of the Cambridge natural science tripos, the number of candidates had only risen to twenty-five, while in 1891 the number was ninety-four, practically as great as any other tripos in the university.

It can now, I think, be claimed that some science is taught in all schools, and a good deal in a great many; this is a great advance and has practically all been made in the last fifty years. It can not, however, be said that even now science occupies in our systems of education a place commensurate with its ever-increasing influence on human thought and with its importance in the progress of civilization.

One defect of the present system is that the entrance scholarships offered by most of the great public schools have in practice the effect of attracting the abler boys to classics. In the examination for most of these scholarships much greater weight is given to classics than to any other subject and a boy must have spent most of his time on classics if he is to do well in the examination. Thus, when he goes to the school he is much further advanced in classics than in anything else and, naturally, takes it as his main subject. It may not, however, be the subject in which his strength really lies. For unlike mathematics, in which marked proficiency is only attained by boys with a somewhat rare type of mind, in classics most able boys can under skilful teaching acquire sufficient proficiency to give them a fair chance of getting an entrance scholarship at a public school. These scholarships may thus entice them along a path which does not lead to their true destination. That this actually occurs is, I think, shown by the figures given in the report of the Committee on the Position of Natural Science in the Educational System of Great Britain, 1918. Of the entrance scholarships to Cambridge gained by boys from seven great public schools which give entrance scholarships, for one gained in science, six were gained in classics. This disproportion is far greater than the average for all schools, showing that it is not due to the rarity of scientific talent as compared with classical, but is an artificial one due to the systems in force at these schools.

The last thing I wish to do is to disparage classical studies. I think that for some boys a course in which classics predominates is the best and I think that in the early stages of the education of all boys classics should play a large, perhaps even the largest, part. What I think is desirable is that the school examination should not be so specialized as it is now, and that the papers in classics should not be so much more advanced than those in any other subject.

It is not enough to have introduced physics into schools, it is necessary to develop methods of teaching which will make its study produce its full educational effect. The problem is a difficult one, the teaching of classics and mathematics have long experience and tradition behind them. No such tradition exists for that of physics. The methods have had to be evolved and it can not be said there is yet anything like complete agreement as to which are the best. Science masters are attacking the problem with the greatest vigor and enthusiasm, are trying out one method after another. I think there is perhaps too great a tendency to concentrate on the method to the exclusion of the personality of the teacher; a good teacher will soon find the method which in his hands gives the best results and will do better with that than with one imposed on him from outside.

POST-GRADUATE STUDY AND RESEARCH

Research is now an integral part of the training of a considerable number of our students and the importance of research for the welfare of the nation universally recognized. This, however, is quite a modern development. It had hardly started sixty years ago and though a vigorous propaganda for the endowment of research was being carried on by Mark Pattison, Huxley, Roscoe, Lockyer and others, it was some time before it began to produce much effect.

Besides the apathy of the country there were at that time three great obstacles to research: (1) The lack of laboratories. We have already seen how this has been remedied. (2) The lack of scholarships to enable men to stay up at the university to research after taking their degree. (3) A third obstacle was that there was hardly any chance of obtaining a livelihood by research alone, so that the only men who made it a career were those who had money or were so enthusiastic that they were reckless about monetary affairs. The case is very different now when research is a recognized profession and a fairly lucrative one.

It is not a great exaggeration to say that in those early days there was neither room, money nor a career for those who wished to research.

Things, however, soon began to mend and research gradually came to be regarded as a suitable subject for the award of scholarships and fellowships. I am glad to say that one of the first, if not the first, to take action in this matter was Trinity College, Cambridge, who in 1874 determined to take into account for election to fellowship any original work which the candidates might submit. Before this the elections had been determined solely on the results of an examination held by the college. They carried out the scheme in no half-hearted way, for at the first election under the scheme in 1874 they elected Francis Balfour, the great zoologist, though it was an open secret that if the examination alone had been taken into consideration another candidate would have been elected. The scheme has been remarkably successful. Many papers of absolute first-rate importance have been submitted by the candidates and now the college has abandoned the examination altogether and only takes into account the original work submitted by the candidates.

In addition to awards for the results of successful research, scholarships began to be founded to enable students who had just taken their degrees to get a post-graduate training in research. The rate of increase in the number of these was very slow in the last century, but this century it has got faster and faster and now grants for training in research are given by most of the colleges, by some of the great city companies, who have done so much for the promotion of science and education, by bodies like the commissioners for the 1851 exhibition, and, above all, by the Department of Scientific and Industrial Research, who have in the last ten years made grants to students in training of £228,970, the average number receiving grants in each year being 184.

In Cambridge the number of students doing post-graduate research increased rapidly after 1895 when a regulation came into force which enabled students who had graduated at other universities to obtain a Cambridge degree after two years' satisfactory research work in Cambridge. This degree was at first the B.A. degree, but in 1920 a new degree, the Ph.D., was instituted by the university for which Cambridge men as well as graduates of other universities are eligible. There are now forty-five of these students in residence taking physics. Of these, by far the greater number hold scholarships or are in receipt of grants. Indeed, I think it can now be said that a really first-class man has an excellent chance of getting, if he is in need of it, sufficient assistance to enable him to get a training in research.

The problem of training a large number of students in research in physics is by no means an easy one; many things have to be taken into consideration and provided for, otherwise the post-graduate course may do more harm than good.

It is very necessary to remember that the importance of the research work done by these students lies not so much in the scientific results obtained as in the training it affords. On this point I should like to read an extract from the report of a commission on the place of science in education, of which I was chairman:

> The training afforded by the study of natural science will be incomplete unless the student undertakes some piece of research in which, relying as far as possible on his own resources, he applies his knowledge of science and of the methods of scientific investigation to the solution of some scientific problem. The effect of a year's work of this kind on the general mental development of the student is most striking. He gains independence of thought, maturity of judgment, self-reliance, his critical powers are strengthened and his enthusiasm for science increased, in fine he is carried from mental adolescence to manhood. We think that whenever possible a year spent mainly on research should form part of the course at the university of those whose work in life will be concerned with the industrial applications of science as well as those who will devote themselves to research and teaching. It is important, however, that at this stage the

teachers at the university should regard research mainly from the point of view of its value as an educational training and not as a means of getting within the year as many new scientific results as possible. The student should be encouraged to overcome his difficulties by his own efforts and the assistance given by the teacher should not be more than is necessary to keep him from being disheartened by failure and to prevent the work from getting on lines which can not lead to success.

I should like to emphasize the last part of this quotation. A year or two spent on research under proper conditions is an educational training which can not easily be overrated, but under others it may be positively harmful. It must always be borne in mind that the primary object of a university laboratory is not the same as that of a laboratory where there are no students in training, such as the National Physical Laboratory or the laboratory of a great firm. In such laboratories the main object is to get results, to discover as many new facts as possible. In a university laboratory the most important thing is to produce well-trained and well-educated men rather than to turn out the largest number of small papers. To get scientific results rapidly, the best plan is for the staff to select the subject for investigation, to determine the kind of experiment to be made, to exercise daily supervision over the work and to leave to the student little besides the taking of the observations. The intellectual development of the student is injured rather than benefited by a training like this. You can not without disaster apply methods of mass production to education. Even in university laboratories where the importance of affording mental training is fully realized, over-specialization is the great danger of these courses of research and one that requires much care to avoid. The student gets so engrossed in his experiments that he grudges the time spent on going to lectures or on reading books which are not on his own special subjects. He often spends too much time in making the experiments and too little in thinking about them. Sometimes, too, he neglects to take advantage of the opportunities afforded by a resident university for social intercourse with men of all shades of opinion and of experience. There is danger, too, of his getting into a groove and to go on working for the rest of his life on the particular subject on which he was first engaged.

I think it helps one to get new ideas if the mind does not dwell too long on one subject without interruption and if every now and then the thread of one's thought is broken. It is, I think, a general experience that new ideas about a subject come when one is not thinking about it. I am not a psychologist and do not know the views held as to how new ideas originate, but to my mind there is considerable practical analogy between this process and one about which we have been hearing a good deal during the last few days, the induction of currents in a magnetic field. For this to occur change as well as the magnetic field is necessary. If a circuit is in such a field nothing happens as long as it is in repose, but if you disturb this repose, currents begin to flow through it. Now compare the circuit to the brain, the magnetic field to the state produced in the brain by long thought about a subject, the starting of a current to the starting of an idea. No ideas will come as long as the brain remains in the same condition without any change in its point of view, but if this changes, then currents or ideas are produced in the brain, the change as it were strikes sparks in the brain. This is one reason why I think it is desirable that the student should do a little teaching, another is that it would give him experience which may be valuable in afterlife and help him to obtain a post.

CAREERS AND RESEARCH WORKERS

I now come to the subject of the careers open to men who have had a training in research. Sixty years ago the only posts open to these were teaching posts in the few physical laboratories then in existence. The number of such posts increased very rapidly towards the end of the last century, as did also the demand for science masters for the schools; but until then and indeed for some time after it may be said that roughly speaking the only posts open to research workers were posts associated with teaching. At the beginning of this century, however, the importance of research to our industries began to be realized. The most striking instance of this is the establishment, in 1901, of the National Physical Laboratory for research both in pure science and in subjects which have an immediate application to industry. The growth of this under Sir Richard Glazebrook and Sir Joseph Petavel has been phenomenal; there are now about 160 research workers employed in the laboratory and the budget has increased twenty-fold. Other methods of linking up science with industry are also being employed in this country. Probably the most efficient is for a firm to have its own laboratory where its own problems can be investigated; in this case the inducements for success are greatest and knowledge of the technique and processes involved most accessible.

There are several such laboratories, each with a large staff in this country. They are, however, so expensive as to be beyond the reach of any but very large firms. To extend the benefit of research to the industries generally, the Department of Scientific and Industrial Research was started by the government in 1915. At the instigation and with the aid of grants from the department, the members of various industries have combined and formed research associations with laboratories suitably equipped for research in matters relating to the particular industry. There are now more than twenty of these associations. They have had to contend with many difficulties; at first there was plenty of money but no well-trained men, now there are plenty of men but no money. There are, however, good reasons for thinking that in spite of these difficulties the financial gain to the industries has far exceeded the expenses of the laboratories. In addition to granting aid to these associations, the department has established boards for research in matters which concern all industries. There is the Fuel Research Board, which deals with problems vital to the country on the production of power from coal and other fuels; there is a Food Research Board, for research on the storage and transport of food; there are boards for building, forest products, radio and chemical research. All these have laboratories and staffs of research workers, as have also the research departments of the army, navy and air services.

I have tried to find how many workers are employed in these applications of science to industry, but have not been able to get any estimate which would be of any value; one great difficulty is to draw the line between posts which seem adequate for those who have gone through a long and expensive training in research and those which do not. One thing, however, can be said, that the demand we have had in Cambridge for workers trained in research has, until this year of acute and long-continued depression, exceeded the supply; and although it is possible to have overproduction in research workers, we do not at present seem to have reached that stage for normal times.

In considering research as a profession, it must be remembered that especially in research of a pioneering kind the worker may spend years without getting results of any very striking importance. He may get depressed, lose hope and be inclined if he gets the chance to go into administration and organization where there is a greater certainty of work yielding an adequate result. The researcher, if he is to have a happy life, must regard the game and not the score as the chief thing. In every research difficulties and apparently anomalous results are constantly turning up. To overcome these, to make clear and consistent what before was obscure and confused, is to some minds one of the keenest of pleasures and one which may be produced by discovering the source of a persistent leak in a discharge tube just as well as by finding a new ray. Experience shows that men with minds of this type are not very common. There are many who when they are young and just fresh from a laboratory, where there is an atmosphere of research and many research workers, are so enthusiastic about research that they think nothing else matters. Often, however, this enthusiasm soon fades and they become more interested in organization and administration than research. Thus, those who begin by working in the research department of a firm tend to drift into the other departments. I think this, on the whole, is an advantage, for it diffuses the scientific spirit and outlook throughout the work of the firm; this may be as important as discoveries in the laboratory and quicker in its effects.

The increase in the number of research workers has naturally led to a corresponding increase in the number of papers on physics. From one point of view this is very gratifying, from another it is embarrassing. *Science Abstracts* for 1930 contains abstracts of 4,165 papers on physics, corresponding to very nearly a dozen a day. It is obvious that no one can read more than a small fraction of these. It is generally more than one can do to read even those in a particular branch of physics, this leads to great specialization. Volumes such as those of *Science Abstracts,* which give the gist of a paper in a small space, are of great value, especially for looking up the literature of a subject over a definite period. For this purpose, however, the subject index is of vital importance; in making this index it is not enough to go by the title of a paper, the contents of a paper can not all be got into its title. The makers of the index should have read the papers. This seems a council of perfection, but it would practically be secured if the maker of the abstract were to send in with it cards for the subject index. This is work that requires great care and sound judgment.

I do not think that abstracts alone are sufficient to cope with this avalanche of papers on physics. As far as I know we have nothing in physics corresponding to the annual reports issued by the Chemical Society on the progress of various branches of chemistry. I think it would be a very good thing if we had, and that it is a thing on which money and time might well be spent. In addition to these, there should, I think, be fuller and more critical reports issued regularly at a longer period, say quinquennially, of the character of those which from time to time have been published by the British Association and by the National Academy of Washington. Another minor suggestion is the publication each month of the titles (without any abstracts) of the physical papers published in scientific periodicals and the proceedings of scientific societies during the preceding month. This used to be a feature of *Wiedemann's Beiblätter* and I found it very useful.

In addition to the advances we have been considering, the instruments and appliances in laboratories are very much better and more convenient than they used to be. The most vivid impression I have of my early work in the laboratory is that of Grove's cells; these had platinum foil immersed in nitric acid for one electrode, zinc in dilute sulphuric for the other, and what with the fumes which assailed one's throat and the acid which destroyed one's clothes, the assemblage of a battery of cells was a most disagreeable business. I have not seen a Grove's cell for forty years and do not want to see another. Now instead of making up a battery we just put a plug into a hole. Another instrument which was exasperating to work with was the old quadrant electrometer; this not infrequently refused to hold its charge and neither prayers nor imprecations would induce it to do so; it has, fortunately, been replaced by more sensitive and convenient instruments. With regard to galvanometers, I have the authority of Mr. Whipple in saying that one suitably selected for the purpose for which it is required may be at least ten thousand times more efficient than the instruments available fifty or sixty years ago. The extensive use of electrical instruments in connection with electrical lighting and engineering has caused a great deal of attention to be paid to their design with the result that they are far more convenient and reliable than they used to be. The improvement of instruments is of first rate importance for the progress of physics, a considerable increase in the efficiency of an instrument may open up a new region of physical phenomena. The most striking example of this is the effect produced by improvements in the methods of producing high vacua. Roughly speaking, we may say that modern physics depends on our power of studying individual atoms and electrons and not merely large crowds of these particles. To do this, one atom must not be hit by another while under observation, as it would make more than ten thousand collisions in a centimeter if the pressure were atmospheric; a very high vacuum is required. Until early in this century this had to be got by Sprengel pumps, which involved one raising and lowering a vessel filled with mercury for hours on end and getting what would now be considered a very poor result, but a vivid appreciation of the intensity with which nature abhors a vacuum. All this was changed after Sir James Dewar introduced the method of producing high vacua by means of charcoal cooled by liquid air. This was not only much more rapid and convenient but produced very much higher vacua and made it possible to make experiments and measurements which could not have been made before the introduction of this method and which have revolutionized our ideas of the structure of matter. If science helps the industries they in return help science. An illustration is that the need of a high vacuum for hot wire valves and electric lamps made its production a matter of commercial importance with the result that the physicist has now at his command pumps so powerful that they can maintain an exceedingly high vacuum in spite of the influx into the vessel of a stream of the particles we wish to study; this is exceedingly important when investigating charged atoms and electrons.

The hot wire valve is another instrument which has greatly helped research in physics; the immense magnification of weak effects which can be produced by it enables us to detect with certainty phenomena which before its introduction were almost beyond our ken. Those who like myself repeated, more than forty years ago, Hertz's experiments will contrast the difficulty we had in detecting electrical waves even when the source was only a few yards away with the ease with which modern methods using hot wire valves detect waves which have traveled thousands of miles.

I have alluded to advances in the efficiency of the instruments. There is another advance in them which is not so gratifying, that is the advance in price. The cost of research in physics is much greater than it used to be. Before the war when about thirty research students were working at the Cavendish Laboratory, the cost of their researches was about £300 per annum. Now it would be at least five times that amount. To balance this there are now far greater sums available for research than there were in those days.

I have in this address confined myself to what may be called the machinery of research in physics. I will now say a word or two about another point. The additions to our knowledge of physical phenomena and to physical conceptions made in the last sixty years have not been excelled by those made in any period of the history of the science, and yet I remember that at the beginning of this period the view was prevalent that all the fundamental principles of physics had been discovered and that the work of the future would be to develop and coordinate those principles and to measure more and more accurately the value of known physical constants. This view seems ludicrous when we know that within a few years Röntgen rays, the electron and radio-activity were discovered. The existence of these was quite unexpected, and no hint of the possibility of their existence was given by any of the physical theories then extant; this view was, however, to my knowledge, held by some eminent physicists. The great generalizations expressed by the first and second laws of thermodynamics loomed so large in those days that it was thought that nothing was beyond their purview. This state of mind is apt to occur after a great discovery; it occurred after that of universal gravitation; there are signs that it exists now. Yet it has always been falsified by experience, and I think always will be. There are no signs that physics is approaching an asymptotic state in which the progress gets slower and slower as time goes on. The additions to our knowledge of physics made by our generation do not get smaller and smaller as one generation succeeds another, each great discovery is not a terminus but an avenue leading to new knowledge. An improvement in technique may, as we have seen, lead to fundamental changes in our views of the nature of matter and of physical processes. There is far more in physics than is dreamt of in our theories; and nature herself, if we observe her carefully, is more suggestive of ideas than the minds of the most imaginative of us. The ideas which revolutionize science are just those of which our theories give no indications. Theories are the very life-blood of physics. Most of the researches in our laboratories originate in an attempt to test a theory; theory, however, may be injurious if it makes us concentrate our attention exclusively on the particular problem it suggested and to treat as an annoyance, to be avoided by a change in method, any anomaly in the experiment which interferes with our progress to the goal; the anomaly may be the outcrop of a vein rich in new phenomena. After Röntgen had discovered x-rays, another physicist who had been working with somewhat similar apparatus said that he had noticed that any photographic plates near his tube got fogged and spoiled; he moved his plates further away and left it at that. The discovery of argon by Lord Rayleigh arose from some vexatious discrepancies in a series of weighings.

I do not think that there is any danger of the supply of new physical phenomena being exhausted and of physicists joining the ranks of the unemployed. Rather do I believe that as each successive centenary comes round the president of section A will be able to say that the growth of physics in the century which has just passed is comparable with that in any of its predecessors.

POST-EHRLICH IMMUNOLOGY[1]

By Dr. W. H. MANWARING
STANFORD UNIVERSITY, CALIFORNIA

FROM a philosophical point of view, classical immunology was equivalent to the postulate that pathogenic microorganisms and the animal body are each vitalized by a unit force, purpose, plan or will that transcends ordinary biochemical laws. Thus supermaterially stabilized, test-tube microorganisms are biochemically identical with the specific infections from which they were isolated and each and every specific serum change in convalescence and artificial immunization is a purposeful defensive hormone. Since this supermaterial somatic organization is immutable in its plan and purpose, it follows that a minute sample of each and every specific serum component that can be formed in the animal body is preexistent in its hereditary tissues, together with a physiological mechanism for its emergency increase in times of specific need. This is but a restatement of the specific receptor hypothesis in non-conventional language.

LOSS OF FAITH IN CLASSICAL THEORY

Recognizing the theological origin of this implied vitalistic theory, and its basic rôle in practical immunology and research, many attempts have been made toward its experimental verification or disproof. Expressed in conventional language, inquiry has been made as to whether or not clinicians are justified in assuming that each and every convalescent serum component is preexistent in normal animal tissues, and whether or not each specific convalescent serum property is a purposeful defensive hormone.

To technical specialists probably the most convincing evidence against this ancestral logic is Otto's alleged separation of "specific sensitizin" from "specific precipitin" by electroosmotic methods. This separation proves that these two convalescent serum components are not chemically identical, and, therefore, can not be mere quantitative variations of the same hereditary defensive hormone.[2] To non-specialists equally suggestive evidence is contained in the war-time researches of Ostromyschlenski and Petroff. These two Russian biochemists incubated mixtures of diphtheria toxin and normal horse serum and found that among the numerous resulting chemical products there are certain denatured serum colloids apparently identical with diphtheria antitoxin.[3] Such test-

[1] Presented before the Society of American Bacteriologists, Pasadena, California, June 18, 1931.

[2] R. Otto and T. Shirakawa, *Ztschr. f. Hyg. u. Infektionskrankh,* 103, 426, 1924.

[3] Ostromyschlenski and Petroff, *Rus. Gesell. f. physical. Chemie,* 47, 263, 1915. (This work was not brought to the attention of international medical science till 1925, and not confirmed in Russian laboratories till 1929, when Kryshanowski prepared several variants of the Ostromy-