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THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

ADDRESS BEFORE THE SECTION OF MATHEMATICS AND PHYSICS.

MY predecessors in this chair have in general been able to make communications to the section conveying the results of investigations of their own, or enunciating some principle which would throw a fresh light on the discoveries of others. Mine is a far less happy lot. During the past four years and a half I have been engaged in administrative duties of such a nature that no time has been available for personal scientific work, and little energy even for the study of the work of others. In these circumstances it might have seemed more fitting if I had refused the honor which the council of the British Association conferred upon me by the request that I would undertake the arduous duties which fall to the lot of the president of Section A. Nevertheless, after much hesitation, I decided to accept the invitation, in the hope that as a looker-on at the struggle of others, and with the experience of an old participator in the fray, I might be able to communicate some impressions which had possibly escaped the notice of those whose attention was necessarily more directed to some special branch of inquiry.

I trust that these words of apology may to some extent explain the nature of what must appear a fragmentary discourse.

In the interval which has elapsed since the last meeting of the association we have lost many men whose names were household words within the walls of the physical laboratory. It is here only possible briefly to refer to the labors of a few of those distinguished seekers after natural knowledge.

The work of Dr. Sprengel has been by no means an unimportant factor in the advance of our knowledge of radiant energy, X-rays, etc., if only on account of the perfection of the apparatus for obtaining high vacua which will ever be associated with his name. The practical effect of his discoveries was considerable, for the business of electric lighting is undoubtedly greatly indebted to his labors. Born in 1834, he settled in England at the age of twenty-five. He was elected a fellow of the Royal Society in 1878, and resided in this country during the remaining years of his life.

The death of Charles Jasper Joly, F.R.S., at the early age of forty-one, robbed mathematics and astronomy of one of their most devoted disciples. His 'Manual of Quaternions' is well known, and those acquainted with his astronomical work are confident that, had his life been spared, he would, as astronomer royal of Ireland, have added luster to an office held by many distinguished predecessors.

Samuel Pierpont Langley was born in 1834. In 1866 he became director of the Allegheny Observatory at Pittsburg. His first work was the institution of a uniform system of time from the Atlantic seaboard to the Great Lakes. This, the first successful attempt to introduce uniformity of time over a large area, was subsequently widely imitated. In 1880 he invented the bolometer, and thus opened out a large new field of investigation into the invisible rays of long wave-length proceeding from heated bodies. He analyzed in minute detail the lunar heat spectrum, and, more recently, he conducted an inquiry into the nature of the radiations emitted by the glowworm. In 1881 he conducted his researches into the solar heat of the earth's atmosphere. In 1887 he became secretary to the Smithsonian Institution. The result of twenty years' labor is to be found in the accurate determination, by temperature alone, of more than seven hundred lines in the invisible red spectrum, lines which are fixed with an average probable error of about one second of arc. In 1891 he published his experiments in aerodynamics, in 1893 'The Internal Work of the Wind' and in 1896 he demonstrated by actual experiment that a body nearly a thousand times heavier than air can be driven through and sustained by it. His published works show great literary charm. He especially excelled in the presentation of abstruse subjects in simple and non-technical language. This is, perhaps, hardly the occasion to refer to his social qualities. Those who had the privilege of his acquaintance, however, can best testify to his quickness of insight, his intense sympathy, especially with the young, and the impression of capability which he produced upon all with whom he came in contact.

The tragic death of Professor Curie was felt as a calamity, not only by those closely interested in the march of scientific discovery, but also by those who had but a superficial knowledge of his work. A teacher for more than twenty years, he was, nevertheless, enabled by his enthusiasm and energy to perform those researches which will ever be connected with his name and that of his wife. So entirely has public attention been attracted to their joint work on the separation of the compounds of radium and their properties that we are apt to overlook other great services he rendered to science. His paper on 'The Effect of Temperature on the Magnetic Properties of Bodies' led to the discovery of the law that for feebly magnetic substances the coefficient of magnetism varies inversely as the absolute temperature. He also pointed out that the magnetization of diamagnetic substances appeared to be independent of the temperature and physical state, indicating diamagnetism as an atomic property.

It is pleasing to reflect that the importance of his discoveries received immediate recognition. It was but three years before his death that he announced to the French Academy the discovery of the new element, and in the same year he and Mme. Curie received the Davy medal of the Royal Society and the Nobel prize; and in July of last year he was elected to the French Académie des Sciences. He was one of the most modest and retiring of men, and this honor came to him unsought; his name will ever be remembered as one of the most notable of that brilliant band of workers who have within recent years so greatly extended the domain of physics by the discovery radioactivity.

A quarter of a century has passed since this section, meeting in this city of York, had the privilege of listening to a presidential address by the pioneer of natural knowledge whom we now know as Lord Kelvin, and it may possibly be a not unprofitable task to review briefly a few of the advances which must render the interval a memorable one in the annals of science. Lord Kelvin summarized the stores of energy from which mechanical effects can be drawn by man as follows: (1) the food of animals, (2) natural heat, (3) solid matter found in elevated positions, (4) the natural motions of water and air, (5) natural combustibles, (6) artificial combustibles.

The twenty-five years which have since elapsed have not made it possible to extend this list. It is true that within the last few years the discoveries connected with radioactivity have enormously increased our estimate of the stores of energy surrounding us, but so far these additional stores can not be regarded by us as stores from which 'mechanical effects may be drawn by man.' It is possible that in the ingenious radium clock which we owe to Mr. Strutt we have a source of mechanical energy unsuspected in 1881, but, at all events, regarded from a commercial standpoint, it can hardly be considered as 'available by man.' Nevertheless, there is a sense in which it may be said that we are profiting by atomic energy, for we are no longer bound to limit our estimate of the energy due to the radiant heat of the sun and the internal heat of the earth by previously known dynamical considerations, and, in consequence, our opinions with regard to the limit of the ages which the physicist could allot to the evolutionist have undergone profound modification.

I here wish to draw attention to some of the conclusions to which we are led by the work of Mr. Strutt.

Assuming the earth to be in thermal equilibrium, then, even if the whole of this interior heat be due to radium alone, the mean quantity per cubic centimeter can not much exceed 1.75×10^{-13} gram. The conclusions of Rutherford, based on somewhat different values for the constants involved, give an equivalent of 1.52×10^{-13}. Now Strutt has found that the poorest igneous rock examined by him, namely, Greenland basalt, contains more than ten times this quantity, and an average rock fifty or sixty times the amount. The assumption that the earth is cooling only aggravates the difficulty, and facts appear to tell against the theory that it is getting hotter. Also, we must take into consideration the heat due to the existence of uranium, thorium, etc.

We appear, therefore, to be driven to one of two assumptions: either (*a*) that the rate of heat production by radium diminishes as we approach the center of the earth; or (*b*) that the interior of the earth differs markedly in constitution from the exterior crust.

It is true that Mr. Makower has shown that there is a slight change of activity in one of the radium products about the temperature of 1,200° C., and it is very desirable that this inquiry should be pushed to much higher limits. At the same time, it appears evident that but a very slight change in activity takes place at temperatures below 1,500° C.

Now Mr. Strutt has shown, arguing from known data, that the maximum temperature at the bottom of a crust of about forty-five miles in thickness, must be in the neighborhood of 1,530° C., although some amount of uncertainty is necessarily induced by our want of knowledge of the conductivity of rock at high temperatures. Anyhow, it is probable that at the depth indicated the temperature does not exceed the melting-point of platinum. Such a crust would contain about one thirtieth of the earth's volume, and if throughout it the radium heat energy were of the average of that exhibited by many samples examined by Strutt, the temperature of the earth could be maintained until our stores of uranium suffered sensible depletion. Such an assumption would lead to the conclusion that the whole of the central portion of the earth consists of non-radioactive substances at an approximate uniform temperature somewhat below the melting-point of platinum. A brief summary of the evidence previously at our disposal may not be out of place.

In the first edition (1867) of Thomas and Tait's 'Natural Philosophy' we find the tidal evidence summarized as follows:

It seems certain, therefore, that the tidal effective rigidity of the earth must be greater than that of glass.

In the 1883 edition of the same work a discussion of the question by Professor George Darwin is given. He states:

On the whole we may fairly conclude, whilst there is some evidence of a tidal yielding of the earth's mass, that yielding is certainly small, and that the effective rigidity is at least as great as steel.

In a later paper (*Proc. Roy. Soc.*, 1885) Darwin pointed out that this conclusion was based on the assumption that oceanic tides would have their equilibrium value, and that the validity of this assumption was open to doubt. Nevertheless, the evidence clearly indicated a high degree of effective rigidity.

Hough (*Phil. Trans.*, A, 1895, 1896) discussed the variation of latitude, and, after correcting a small mistake of Newcomb's (who was the first to suggest the explanation), found the prolongation of the Eulerian nutation from 305 to 430 days as indicating an effective rigidity of the earth about equal to that of steel. Wiechert (*Trans. Roy. Soc. Göttingen,* 1897), of Göttingen, found that the mean density, ellipticity and precessional constant were consistent with the hypothesis of homogeneous core with lighter surface layer.

Mr. R. D. Oldham (*Phil. Trans.*, 1900), in a paper on the 'Propagation of Earthquake Waves,' came to the conclusion that the evidence pointed to a central metallic core, and to the existence of marked differences in the physical constants of the core and the surrounding crust. He, however, assigned a comparatively small radius to this core, viz., about 0.55 that of the earth.

I will now call your attention to the light thrown on this subject by the recent investigations of Professor Milne. The difference in the rate of propagation of earthquake waves through the earth's interior and through the crust has led him to the conclusion that the material below a depth approximating to thirty miles is of a uniform nature, and that the change in physical constitution is abrupt at some such depth as that indicated. He writes as follows:

For chords which lie within a depth of thirty miles the recorded speeds do not exceed those which we should expect for waves of compression in rocky material. This, therefore, is a maximum depth at which we should look for materials having similar physical properties to those we see on the earth's surface; beneath this limit the materials of the outer part of this planet appear rapidly to merge into a fairly homogeneous nucleus with a high rigidity.

In the *Transactions of the Royal Society* for 1905 will be found a paper by Lieut.-Colonel S. G. Burrard on 'The Intensity of the Force of Gravity in India.' Colonel Burrard writes as follows:

Geodetical observations have shown that the density of the earth's crust is variable, but they have not given any positive indications of the depths to which these observed variations extend. All calculations of the depths of subterranean variations in density and of the mountain compensation have, therefore, to be based on arbitrary assumptions of depth. The fact that the plumb-line seems generally to respond readily to the results given by the pendulum perhaps justifies the inference that the observed variations in the density of the earth's crust are not deep-seated. If an abnormal amount of matter exists in the crust near the surface, it will exercise direct effects upon plumb-lines and pendulums in the vicinity, but if it lies at a great depth its effects, especially on plumb-lines, will be less perceptible. * * * I have taken several instances of abnormal pendulum results from table, and have found in each case direct response from the plumb-lines at neighboring stations. This conformity could hardly ensue if the variations in density extended to greater depths than thirty or forty miles. Our results do not justify us in asserting that no deep-seated variations in density exist, but they do justify the belief that the variations in density which have been discovered are apparently superficial.

It is interesting to notice the agreement between results drawn from such dissimilar sources. On the one hand we have had to deal with effects produced by almost inconceivably small particles traveling with immense velocity; on the other, with effects dependent upon the behavior of 'the huge terrestrial globe.' That travelers starting from such opposite extremes should arrive at a common destination is in itself a striking example of the scope and accuracy of the work undertaken by investigators in physical science.

It is possible that the evidence from each source, considered independently, might be regarded as inadequate, but the cumulative effect is sufficiently strong to justify the belief that some marked physical change in the constitution occurs at a depth of some thirty to fifty miles.

At all events, we have indications that, with the exception of a comparatively thin crust, the earth consists of a non-radioactive substance with a rigidity approaching that of steel, with an average temperature in the neighborhood of 1,500° C., and a density at that temperature of about 5.6° C.

An interesting question awaiting solution is the probable constitution of this core.

The above is but an example of the many fascinating problems upon which fresh light has been thrown by the revelations of recent discoveries in radioactivity, and the temptation to dwell on such themes is correspondingly great: but I feel that such a task should be committed to hands more capable than mine.

Fortunately, in the discussions which will take place during our meeting ample opportunity will be afforded those entitled to speak with authority. Nevertheless, there are one or two further aspects of the matter which I will venture to touch upon, although but an onlooker. I would, first of all, urge the importance of a study of what may be termed the natural history of the elements. We require more information as to their comparative proportions in different localities. The fact that, given the amount of uranium in a sample of native rock, we can predict with certainty the amount of radium contained in the same specimen is of startling significance.

The natural law which governs the proportions of these two substances may have a far wider-reaching scope than we at present suspect. Nature appears to present to us a grouping which would not naturally have occurred to the mind of the chemist; lead and silver, copper and gold, and, again, platinum and iridium seem invariably to be introduced to us by nature as if bearing to each other some kind of blood relationship.

The facts we already possess seem dimly to indicate some close relation between elements which we have hitherto considered as outside the bounds of consanguinity, and for a fuller knowledge of this important branch of natural history we require the assistance of the practical engineer, the geologist, the metallurgist and the chemist.

Many of the results arrived at by the investigators into the phenomena of radioactivity can apparently only be verified by the lapse of considerable intervals of time. It is, probable, for example, that we can estimate with some degree of accuracy the time required for the dissolution of half a given mass of uranium or radium, but the complete verification of our inferences must probably be left to a future generation. If we accept this view, it is our duty to provide our successors with data on which their conclusions may be based. If, for example, carefully determined masses of the more radioactive substances could be placed in such circumstances as to remain untouched until the meeting of this association some hundred years hence, our successors, who would doubtless be equipped with apparatus of research more accurate and more sensitive than any in our possession, would at all events be placed in a position to establish by direct methods the accuracy of inferences based upon the experimental data now at our disposal. This task is one which, it appears to me, might well be undertaken by Section A, and I trust that this suggestion may be held worthy of some consideration.

It appears probable that one gram of radium diminishes in weight by about half a milligram per annum; hence, if the funds of this society admitted of the imprisonment of some definite mass of radium, our successors a hundred years hence would, even if they possessed only the apparatus now at our disposal, be able to determine its loss with sufficient accuracy to enable them to verify the truth of the conclusions arrived at by the physicist of to-day, while the investigation of the radioactivity of the residue would possibly throw light on many problems now awaiting solution.

It would appear that if we made a similar imprisonment of uranium, a like degree of accuracy would not be attainable until after the lapse of half a million years, and I am afraid that our interest in the work of our successors can not be expected to cover so long a period. Nevertheless, it is probable that the presence of the products of decomposition could easily be detected after the lapse of a comparatively short interval of time.

The experiment might well be extended so as to include examples of all the elements capable of such treatment; and with each prisoner should be placed a full record of its physical constants, such as mass, density, electrical conductivity, specific heat, etc., with a clear indication of what is believed to be the probable accuracy of such determination.

During the past twenty-five years much thought has been devoted to the accurate determination of certain physical constants. This is very apparent in the case of one of the most important—namely, that commonly termed the 'mechanical equivalent of heat,' or, as I prefer to define it, the 'thermal equivalent of energy.' When Lord Kelvin addressed you in 1881, I think it probable that he would have indicated the value obtained by Joule—viz., 772.6 foot-pounds—at Manchester, as the quantity of work required to raise the temperature of one pound of water through 1° F. at 62° F. It is true that the results of Rowland's classical investigation were published in 1880 and 1881, but the discrepancy between his conclusions and those of Regnault regarding the change in the specific heat of water at temperatures between 0° C. and 30° C. introduced an element of uncertainty.

As a consequence of this discrepancy much experimental work on the subject has been performed in the last quarter of a century, and I think it may be said without hesitation that the value of this important constant is now ascertained with an accuracy of about one part in 2,000. The amount of labor which has been employed in the determination of this thermal constant is extraordinary, and, as I have pointed out elsewhere, it well illustrates the cosmopolitan character of scientific investigation.

I have given reasons (Griffiths, 'The Thermal Measurement of Energy') for specially selecting for consideration the determinations of Rowland, of Bartoli and Stracciati, of Ludin, of Callendar and Barnes, of Schuster and Gannon, and I have ventured to add my own. Thus Baltimore, Pisa, Zurich, Montreal, Manchester and Cambridge have all contributed to the solution of the problem, and we may now with some certainty say that 777.7 foot-pounds at Greenwich are very closely the equivalent of the amount of heat required to raise one pound of water through 1° on the hydrogen scale at 63.5° F.

It may possibly appear that the result just quoted is a somewhat poor return for the expenditure of so much thought and labor. I would call attention, therefore, to the fact that the value of this equivalent is dependent on the measurements of many other natural constants; hence any agreement between the results obtained by the observations of Rowland and some of the other observers I have mentioned would only be possible in the absence of errors of appreciable magnitude in the determinations of mass, of change of temperature, and of electrical resistance and current. Certain discrepancies have led to the discovery of a hitherto unsuspected cause of inaccuracy, especially in the determination of temperature, and thus the inquiry has rendered valuable service in many branches of physical inquiry.

For example, so far back as 1893 I ventured upon a prophecy that the value assigned to the E.M.F. of a Clarke's cell was somewhat too high, and that it was possible that 1.4328 represents more truly the potential difference of a Clarke's cell at 15° C. than the ordinarily accepted value of 1.4342. In the report of the Electrical Standards Committee for 1897 will be found a discussion of this matter, and one of the consequences of the deliberations of that committee is to be seen in the ampere balance now standing in the National Physical Laboratory.

The results of the observations obtained by this instrument will, I believe, shortly be published by Professor Ayrton and Mr. Mather, but I am at liberty to state that, so far as the observations have been reduced, they point to the conclusion that the prophecy to which I have referred is closely fulfilled. We may say, therefore, with some confidence that the values of those units which form the basis of our system of electrical measurement are not only practically determined with a high degree of accuracy, but that also our measurements of temperature and of energy are placed on a satisfactory footing.

The last few years have been fruitful in revelations which not only profoundly affect the views of students of science, but also are of such a nature as to catch the eye of the public. In some cases the applications of these discoveries to the purposes of mankind have been evident and immediate. Every well-equipped hospital possesses apparatus for the production of Röntgen rays, and I suppose that every bluejacket in the navy has some degree of acquaintance with those applications of science which have resulted from the discovery of Hertzian waves.

The ambition of the student is naturally fired by such examples, and there is a possible danger that the plodding but absolutely necessary work of accurate measurement may suffer by neglect. I, therefore, venture to repeat the well-established axiom that our advance in scientific knowledge is a function of accurate measurement, and that the student who devotes his energy to the determination of some physical constant is probably giving a 'point of departure' to the pioneer. For it must ever be remembered that to the scientific investigator the rule of three has ceased to hold any significance.

When Lord Rayleigh discovered that the mean weight per liter under standard conditions of chemical nitrogen was 1.251, and that of atmospheric nitrogen was 1.257, the believer in the rule of three would have been unlikely to suspect that this difference of 0.006 would supply the clue which led Lord Rayleigh and Sir W. Ramsay to the discovery of a new element, a discovery which in its turn led to others of possibly even greater importance. For all we know the next decimal place in any hitherto accepted value may afford another example of the truth of the statement that a part may be greater than the whole.

At the time when Lord Kelvin delivered the address to which I have already referred, the truth of the second law of thermodynamics was probably not so generally accepted as is the case at the present time. Each apparent example of violation of that law has on closer examination proved to be additional evidence of its validity. We seem unable to find those 'sorting demons' of Maxwell's, the existence of which appears necessary for its violation.

Mr. Campbell recently expressed doubts as to the application of thermodynamic considerations to osmotics. He contended that the errors in the determination of osmotic pressure were greater than those which could be attributed to experimental sources. Now, the theoretical relation between osmotic pressure and the freezing-point is based directly on thermodynamic considerations, and it was because I entertained a belief that the most direct evidence of this much-debated matter could be obtained from the observation of the freezing-point of a very dilute solution that I embarked on a series of somewhat elaborate experiments during the years 1897 to 1901. My removal from Cambridge and the death of my assistant, Mr. C. Green, compelled me to leave that inquiry in an unfinished condition. Nevertheless, I had investigated the depression of the freezing-point in certain solutions varying in strength from 0.0003 to 0.025 gm.-molecule per liter.

Subsequently to my departure from

Cambridge Mr. Bedford reerected the apparatus in another building. After having surmounted great difficulties, he repeated many of my experiments, and he informs me that the numbers he has so far obtained are in almost entire agreement with those previously obtained by me. The molecular depression in the case of cane sugar I found to be 1.858, of potassium chloride 3.720, and I understand that Mr. Bedford's experiments agree with these results with a discrepancy of less than 1 part in 1,000. The most probable number obtained from theoretical considerations would be in the former case 1.857, in the latter 3.714. As Mr. Whetham has pointed out, unless there is some balancing of opposite errors of a very improbable nature, it is difficult to imagine a more direct vindication of the application of thermodynamic considerations to the phenomena of solution. I may add that I also examined correspondingly dilute solutions of sodium chloride, barium chloride, sulphuric acid, potassium bichromate, magnesium chloride and potassium iodide; but, owing to the circumstances to which I have referred, I was unable to repeat these experiments in such a manner as to enable me to attach great importance to the resulting figures. Nevertheless, I obtained values which strengthened the conclusions to which I was led by the more exhaustive examination of the dilute solutions of sugar and potassium chloride.

So far back as the Liverpool meeting of this association I expressed a hope that the experimental difficulties of the direct measurement of osmotic pressures would be overcome, as such direct measurement would afford the most useful data by means of which to obtain further light on the much-vexed question of the nature of solutions. I remember, also, that it was the general opinion of those who had given attention to this matter that the experimental difficulties were insuperable.

I am glad, therefore, to have this opportunity of stating my high appreciation of the manner in which Lord Berkeley and Mr. Hartley have grappled with the difficulties of this investigation. They have proved that the osmotic pressure obtained by direct measurement agrees with that derived from vapor-pressure observations to within less than 5 per cent.[1] The agreement is of great importance, as it diminishes our doubts as to the extent to which the imperfections of semi-permeable membranes may affect the validity of results dependent upon their behavior, and points to the possibility of determining the osmotic pressures of concentrated solutions by measurement of their vapor pressures.

I trust it will not be thought out of place if I here refer to the interesting correspondence which has recently appeared in *Nature* on the thermodynamic theory of osmotic pressure, and the allied, but by no means identical, problem of the difference between electrolytic and non-electrolytic solutions.

On the one side we have Professor Armstrong, whose chief desire appears to be the vindication of the moral character of what he terms 'the poor molecule'; and Mr. Campbell, whose doubts concerning the second law of thermodynamics are closely connected with a lurking belief in the existence of Maxwell's 'sorting demons'; and by way of reserves we have Professor Kahlenberg, who contends that 'thermodynamic reasoning can not be applied to actual osmotic processes' on account of the

[1] Concentration.	Direct O. P. at 0° C.	O. P. deduced from Vapor Pressure at 0° C.
540 grains per lit. solution..	67.5	69.4
660 grains per lit. solution..	100.8	101.9
750 grains per lit. solution..	133.7	136.0

Proc. Roy. Soc., June, 1906.

'selective action of the membrane' and 'insists that the formation of crystals from a solution or the concentration of a solution by evaporation are not osmotic processes.'

On the other hand we have Mr. Whetham, who, I confess, seems to me to be capable of holding his own without need of reinforcements. He has pointed out that confusion has arisen from the use of the term 'osmotic pressure' to denote the actual pressure experimentally realized in certain conditions, as well as the ideal pressure required in thermodynamic theory. With regard to the theory of electrolytic dissociation, Mr. Whetham shows that the fact that the velocities of the ions are constant in dilute solutions and decrease slowly with increasing concentration, while the conductivity of a dilute solution is at most proportional to the first power of the concentration, appears irreconcilable with any assumption as to the existence of the active part of an electrolyte in the form of combined molecules when in solution. I would here join with Mr. Whetham in the request that those who oppose the theory of ionic dissociation would state their views as to the mechanism of electrolysis, and their reasons for supposing that the application of the principles of thermodynamics to the phenomena of solution is unjustifiable.

Professor Armstrong remarks that it is unfair to 'cloak the inquiry by restricting it to thermodynamic reasoning, a favorite maneuver with the mathematically minded.' He adds that such a course may satisfy the physicist, but 'is repulsive to the chemist.'

The inquiry, 'Why is the application of thermodynamic reasoning repulsive to the chemist?' naturally suggests itself. I confess that at one time I regarded the extreme advocates of the theory of ionic dissociation with a certain amount of suspicion, but I think that most of those who have studied the evidence now at our disposal, or who have been engaged in experimental investigation into this interesting branch of physics, can not fail to agree with Mr. Whetham that, as regards the fundamental conceptions of the theory, 'the cumulative evidence seems overpowering.' At all events, we may consider that the application to the phenomena of solution of reasoning based on thermodynamic considerations is justifiable, until we are presented with stronger arguments than those based on the repulsiveness to certain chemists of the conclusions to which it leads, or the doubt it throws upon the activities of Maxwell's demons and the selective action of semi-permeable membranes.

I will now trespass upon your forbearance and pass from the consideration of such special departments of natural science as usually engage the attention of members of this section to some more general considerations, which naturally arise in any comparison of our knowledge of to-day with that which we possessed when we last met in this city.

It will, I think, generally be admitted that during the last twenty-five years the increase in our 'natural knowledge' has been greater than in any previous quarter of a century.

Day by day we are adding new facts to our storehouse of information, until it has now become impossible for the individual to have more than a superficial knowledge of the contents of the building. And although this accumulation is one which we may well regard with satisfaction, it necessarily gives rise to difficulties unfelt by our predecessors.

I venture to indicate one of such difficulties, one which has been brought home to me both by my experience as an examiner and by the fact that during the past few years I have had to preside over many meetings of examiners, and to mark the effect of examinations on the teaching in our universities.

We now expect a student to acquire in a three years' course a far greater amount of information than was considered necessary, say, twenty-five years ago. The attention both of the teacher and of the taught is naturally directed to those extremities of the branches of science in which the growth has been most marked in recent years, and I venture to think that there is in consequence some danger of our neglecting the roots of the whole matter. Compare, for example, a final paper in chemistry in any one of our universities with its predecessor of a quarter of a century ago.

The enormous advance of organic chemistry has necessarily reacted on the examinations, and thus the student is unable to devote an adequate proportion of his time and attention to the foundations of the subject. The same remark applies in the domain of physics. There is a danger, therefore, of our educational edifice becoming top-heavy.

I have heard complaints, on the one hand, from the examiners that while the candidates frequently exhibit considerable knowledge of the most recent scientific developments, they show a lamentable ignorance of the simple phenomena and the principles they illustrate. On the other hand, I have heard from candidates that many of the questions were too simple—that they were concerned with principles and facts to which their attention had not been directed since they first began the study of natural science.

My own experience has been that the simplest questions are those answered in the least effective manner. A candidate unable to give satisfactory illustrations of Newton's laws will discourse upon the mass of an electron or the nature of the Röntgen rays, and attempt the solution of problems on such subjects as Hertzian waves and electric convection.

I hope that the attention of both examiners and teachers may be directed to the best methods of dealing with what appears to me to be not only a serious but an increasing evil.

To pass from one of the inconveniences which inevitably arise from growth, it is pleasant to dwell upon its more gratifying consequences.

Perhaps one of the most marked characteristics of the progress of science in recent times is the increasing public appreciation of the importance of original investigation and research.

The expansion of the university colleges in number and importance has greatly assisted and quickened this movement.

Twenty-five years ago there were comparatively few laboratories which held out any possibility of research to the English student. True, there were giants in those days, men, as a rule, working under difficulties greater than those encountered by their successors of to-day. The better equipment of our laboratories and the growth in the number and activity of our scientific societies have played no small part in stimulating public interest. Nevertheless, much remains to be done. Those who have read Professor Perry's somewhat pessimistic words on England's neglect of science must admit that, however rapid our progress, the British people have not yet so fully awakened to the national importance of this question as some of our competitors.

The idea that a degree is one of the chief objects of education yet lingers amongst us. The conviction that it is a national duty to seek out and, when found, utilize the latent scientific ability of the rising generation for the purpose of adding to our stores of natural knowledge still needs to be brought home to the 'man in the street.' And here I would venture to indicate my personal belief in the necessity of more free communication between the laboratory and the market-place. It is possible that the language of science is becoming too technical, and that the difficulties with which scientific inquirers have been faced in past times have tended to habits of exclusiveness. For example, complaints are frequent that our manufacturers are less alert in grasping the practical applications of scientific discovery than their competitors in Germany and the United States. I confess, however, that it seems to me possible that the fault is not altogether on the side of the manufacturers. We want missionaries to preach the doctrine that one of the greatest of national assets is scientific discovery. If we can convince the men of business of this country that there are few more profitable investments than the encouragement of research, our difficulties in this matter will be at an end.

It is my lot to serve on the education committees of three county councils, and I have been much struck by the readiness of the members of those bodies to extend such encouragement whenever it has been possible to convince them that the results may conduce to the prosperity, the comfort and the safety of the community.

It has also been my privilege to address meetings of the men who work in the coal-fields of South Wales. I have attempted to direct their attention to the advantages which they have derived from the labors of those who have endeavored to probe the secrets of nature in the laboratory; I have tried to show how discoveries based on the researches of Humphry Davy, Faraday, Joule, for example, have not only diminished the dangers to which miners are exposed, but have also, by increasing the demands upon our stores of energy, given employment to thousands of their fellow-workers.

My experiences lead me to the belief that these men are ready to support the action of their representatives in extending support and encouragement to all efforts to assist the advance of scientific discovery.

It is possible that in dwelling on this matter I am trespassing on your forbearance, but I can not resist this opportunity of pleading for the extension of your sympathies beyond the walls of the laboratory. The old toast, 'Here's to science pure and undefiled; may it never do a ha'porth of good to anybody,' may possibly be an excellent one in the laboratory; for, so far as I know, no great scientific principle has ever been established by labors prompted solely by desire for financial gain. Nevertheless, if we wish for the support of our fellow-countrymen, that toast is not one for public dinners. There is no scientific society which is brought into such close contact with the public as is the British Association, and affiliated with that association are some scores of local scientific societies, containing many thousands of enthusiastic observers and inquirers. If this great organization were seriously to take up the task of bringing home to the minds of the people of this kingdom the enormous value of the results of scientific inquiry, I believe it might be possible to change the indifference and apathy of our public bodies into active interest and encouragement. If each affiliated society would institute a series of public non-technical lectures, of such a nature as to bring home to the minds of the hearers some comprehension of the results of the work of Faraday, of Wheatstone, of Pasteur, of Maxwell, of Lister and of Kelvin, the change in the public attitude would be real, evident and fruitful.

In conclusion, one is tempted to seek for the underlying cause of the acceleration in the rate of advance of natural knowledge. Is it to be found in the increase in power of the human intellect, or the diversion into one particular channel of activities previously otherwise employed? It is possible that the human intellect has, by the processes of evolution, become more powerful, and that man's ability to decipher the secrets of nature has thereby increased. I think, however, that it would require a bold advocate to support this thesis. If any such mental evolution has taken place, it is strange that it should be restricted to one particular sphere of activity. Are our poets and authors of to-day greater than Homer, our statesmen than Pericles? Or, passing into the domain of science, can we say with confidence that, in pure power of reasoning, Maxwell was undoubtedly the superior of Archimedes?

I have elsewhere indicated what appears to me to explain the mystery of this acceleration, namely, *the extension of our senses* by mechanical appliances. When we supplement our eyes by the bolometer and the electric coherer, the range of our vision is augmented a thousandfold. By the use of the electroscope and the galvanometer we have extended our senses of sight and touch until we can detect the presence of an electron.

Having realized the imperfection of our faculties, we have called upon nature in all departments of science to supply our deficiencies, and are thus enabled to walk with confidence where previously all seemed dark.

From the time of Archimedes to that of Bacon we despised natural knowledge while we deified intellect and authority; hence for nearly 2,000 years our record was one of retreat rather than advance. When the philosopher left his study and applied his powers of observation to the phenomena of the universe, progress became a reality, and thenceforward the march of discovery has known no backward step. We have, therefore, every reason to believe that when the association again visits this ancient city our president will be able to chronicle an increase in natural knowledge even greater than that which has been one of the distinguishing characteristics of the last quarter of a century.

E. H. GRIFFITHS.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

SPECIAL MEETING, ITHACA, NEW YORK, JUNE 28–JULY 3, 1906.

SECTION E—GEOLOGY AND GEOGRAPHY.

THE section organized at 11 A.M., Friday, June 29, directly after the adjournment of the first general session of the association, in the geological lecture room, McGraw Hall, with Vice-president and Chairman A. C. Lane in the chair and thirteen members and seven visitors present. Before proceeding with the program of papers the following preamble and resolution were offered by Mr. George H. Chadwick, of Albany, New York:

WHEREAS in the decease of Professor Israel C. Russell Section E has lost an efficient officer and one of its foremost workers and best loved members.

Resolved, That the section express its deep sorrow and its sense of the great loss to geologic science through the event.

Remarks to the motion were made by Messrs. A. C. Lane, H. S. Williams, D. S. Martin and E. O. Hovey.

The following papers were then read in accordance with the printed program:

Revision of the Geological Section passing through Ithaca, N. Y.: Professor H. S. WILLIAMS, Cornell University. (By permission of the Director of the U. S. Geological Survey).

The author explained that the revision