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THE CONSTITUTION OF THE EARTH[1]

WHEN one is confronted as on this occasion with the British Association in plenary session it is permissible, I hope, to indulge in a few reflections on the nature and purpose of science in general. The theme is no new one and has never been discussed so frequently as in our time, but the very range of our activities entitles us to consider it from our own point of view. The subjects treated at these meetings range, according to the titles of our sections, from the most abstract points of mathematical philosophy to the processes of agriculture. Between these limits we have the newest speculations of astronomy and physics, the whole field of the biological sciences, the problems of engineering, not to speak of other matters equally diverse. These subjects, again, have become so subdivided and specialized that workers in adjacent fields have often a difficulty in appreciating each other's ideas, or even understanding each other's language. What then is the real purpose of science in the comprehensive sense, what is the common inspiration, the common ambition behind such enthusiastic and sustained effort in so many directions? The question may seem idle, for a sort of official answer has often been given. It was deemed sufficient to point to the material gains, the enlarged powers, which have come to us through science, and have so transformed the external part of our lives. The general aim was summed up in an almost consecrated formula: "to subdue the forces of nature to the service of man." And since it was impossible to foresee what abstract research might or might not provide a clue to something useful, the more speculative branches of science were not only to be tolerated, but to be encouraged within limits, as ancillary to the supreme end. And, it must be said, the cultivators of these more abstruse sciences have themselves been willing sometimes to accept this position. The apologists of pure mathematics, for instance, have been wont to appeal to the case of the conic sections, which from the time of Apollonius onwards had been an entirely detached study, but was destined after some 2,000 years to guide Kepler and Newton in formulating the laws of the planetary motions, and so ultimately to find its justification in the Nautical Almanac. I will not stop to examine this illustration, which I personally think rather strained. We may recognize that practical utility has been a

[1] Address of the president of the British Association for the Advancement of Science, Southampton, 1925.

conscious though not the sole aim in much scientific work, and sometimes perhaps its main justification; but we can hardly admit that any such formula as I have quoted worthily conveys what has been the real inspiration of discovery through the ages. If we may go back to Apollonius and the conic sections, we can not suppose that he was thinking of posterity at all; he was engaged in a study which he no doubt held to be legitimate and respectable in itself. Or, to take a very recent instance, when Faraday and Maxwell were feeling their way towards an electric theory of light, they could hardly have dreamed of wireless telegraphy, though as we now know this was no remote development. The primary aim of science as we understand it is to explore the facts of nature, to ascertain their mutual relations, and to arrange them as far as possible into a consistent and intelligible scheme. It is this endeavor which is the true inspiration of scientific work, as success in it is the appropriate reward. The material effects come later if at all, and often by a very indirect path. We may, I think, claim for this constructive task something of an esthetic character. The provinces of art and science are often held to be alien and even antagonistic, but in the higher processes of scientific thought it is often possible to trace an affinity. The mathematician at all events is at no loss for illustrations of this artistic faculty. A well-ordered piece of algebraical analysis has sometimes been compared to a musical composition. This may seem fantastic to those whose only impression is that of a mass of curious symbols, but these bear no more resemblance to the ideas which lie behind them than the equally weird notation of a symphony bears to the sounds which it connotes or the emotions which these evoke. And it is no misplaced analogy which has led enthusiasts to speak of the poetical charm of Lagrange's work, of the massive architecture of Gauss's memoirs, of the classic perfection of Maxwell's expositions. The devotees of other sciences will be at no loss for similar illustrations. Is it not the case, for instance, that the wide-spread interest excited by the latest achievements of physical science is due not to the hope of future profit, though this will doubtless come, but to the intrinsic beauty as well as the novelty of the visions which they unfold?

It is possible, I trust, to insist on these aspects of the scientific temperament without wishing to draw a sharp and even mischievous antithesis between pure and applied science. Not to speak of the enormous importance in our present civilization of the material advantages which have come in the train of discovery, it would be disloyal to science itself to affect to depreciate them. For the most severely utilitarian result comes often as the result of a long and patient process of study and experiment, conducted on strictly scientific methods. We must recognize also the debts which pure science in its turn owes to industry, the impulse derived from the suggestion of new problems, and not least the extended scale on which experiment becomes possible. And a reference may appropriately be made here to the National Physical Laboratory, initiated mainly in the higher interests of industry, which by the mere pressure of the matters submitted to it is becoming a great institute of theoretical as well as applied science, informed throughout by the true spirit of research.

But perhaps the most momentous consequences of the increased scientific activities of our time have been on the intellectual side. How profound these have been in one direction we have recently been reminded by the centenary of Huxley. Authority and science were at one time in conflict over matters entirely within the province of the latter. The weapons were keen, and the strife bitter. We may rejoice that these antagonisms are now almost obsolete; one side has become more tolerant, the other less aggressive, and there is a disposition on both sides to respect each other's territories. The change is even reflected in the sermons delivered before the association. The quarters where we may look for suspicion and dislike are now different; they are political rather than ecclesiastical. The habit of sober and accurate analysis which scientific pursuits tend to promote is not always favorable to social and economic theories which rest mainly on an emotional if very natural basis. Some of us, for instance, may remember Huxley's merciless dissection of the theory of the social contract. There is hence to be traced, I think, a certain dumb hostility which, without venturing on open attack, looks coldly on scientific work except so far as it is directed to purposes of obvious and immediate practical utility.

There is a more open kind of criticism to which we are exposed, which we can not altogether ignore, though it again rests on a misconception of the true function of science. It is to be met with in quarters where we might fairly look for countenance and sympathy, and is expressed sometimes with great force and even eloquence. The burden is one of disappointment and disillusion; we even hear of the "bankruptcy of science." It seems to be suggested that science has at one time or other held out promises which it has been impotent to fulfil, that vague but alluring hopes which it has inspired have proved delusive. It may be admitted that extravagant and impossible claims have sometimes been made on behalf of science, but never, I think, by the real leaders, who have always been most modest in their claims and guarded in their forecasts. It is true again that in the enthusiasm which attended the first sensational developments of modern industry hopes were conceived of a new era, where prosperity would ever increase, poverty would be at least mitigated and refined, national antipathies would be reconciled. When these dreams did not swiftly come true there was the inevitable reaction, the idols were cast down, and science in general has rather unreasonably come in for its share of depreciation. The attitude which I have been trying to describe is put very forcibly in a quotation from President Wilson which I saw not long ago, though its date is not very recent:

> Science has bred in us a spirit of experiment and a contempt for the past. It made us credulous of quick improvement, hopeful of discovering panaceas, confident of success in every new thing. . . . I should fear nothing better than utter destruction from a revolution conceived and led in the scientific spirit. Science has not changed the laws of social growth or betterment. Science has not changed the nature of society, has not made history a whit easier to understand, human nature a whit easier to reform. It has won for us a great liberty in the physical world, a liberty from superstitious fear and from disease, a freedom to use nature as a familiar servant; but it has not freed us from ourselves.

The tone is one of bitter disillusion, but we may ask why should science, as we understand it, be held responsible for the failure of hopes which it can never have authorized? Its province as I have tried to define it is vast, but has its limits. It can have no pretensions to improve human nature; it may alter the environment, multiply the resources, widen the intellectual prospect, but it can not fairly be asked to bear the responsibility for the use which is made of these gifts. That must be determined by other and, let us admit it, higher considerations. Medical science, for instance, has given us longer and healthier lives; it is not responsible for the use which we make of those lives. It may give increased vitality to the wicked as well as the just, but we would not, on that account, close our hospitals or condemn our doctors.

In spite of the criticisms I have referred to, we may still hold up our heads, let us hope without arrogance, but with the confidence that our efforts have their place, not a mean one, in human activities, and that they tend, if often in unimagined ways, to increase the intellectual and the material and even the esthetic possessions of the world. And in that assurance, we may rejoice that science has never been so widely and so enthusiastically cultivated as at the present time, with so complete sincerity, or (we may claim) with more brilliant success, or even with less of international jealousy.

Passing from these reflections which are, I hope, not altogether inopportune, it is expected that the president for the time being should deal with some subject in which he has himself been interested. For a mathematician this obligation is a specially difficult one, if he is not to overstrain the patience of his audience. I propose to speak briefly, and mainly from the mathematical and physical standpoint, about some branches of geophysics, and in particular those relating to the constitution of the earth. It is a subject which in the past has often engaged the attention of the association; I need only recall the names of Kelvin and George Darwin, and the controversies with which they are associated. Historically, it is of special interest to the mathematician and the physicist, for it was in his researches on the figure of the earth that Laplace initiated the theory of its potential, with its characteristic equation, and so prepared the way for Poisson, Green, Cauchy, and a host of followers, who developed the theory of electricity and ultimately that of light. To go further back, it was in this connection that Newton found an important verification of his law of gravity. Quite recently, the whole subject has been reviewed in a valuable treatise by Dr. Jeffreys, who arrives at conclusions which are at all events definite, and maintained with great ability.

I do not propose to deal with the fascinating speculations as to the past history of the earth and its reputed child, the moon, which will be more or less familiar. I must confine myself to a rapid survey of the information as to its present constitution which can be gathered from observations made in our own time, and capable of repetition at will. This, though less exciting, is at all events a region in which imagination is more subject to control.

The accurate investigation of the figure of the earth is intimately connected with the variation of gravity over its surface. In view of the local irregularities, some convention was necessary as to what is meant by the shape of the earth as a whole. The usual definition is that it is a level surface as regards the resultant of true gravity and centrifugal force: often that particular level surface of which the sea forms a part. I need not dwell on the immense amount of theoretical and practical labor which has been devoted in various countries to the determination of the geometrical surface which most nearly satisfies this requirement. Of more recent interest are the irregularities in the intensity of gravity, which have been found to exist over wide areas, by the highly trained Survey of India, by the Coast and Geodetic Survey of the United States, and by various observers on the continent of Europe. Briefly, the general result is this, that in mountainous regions the observed value of gravity is abnormally low, whilst on oceanic islands, and so far as can be ascertained on the sea, it is abnormally large, when all allowance has been made for altitude and the normal variation with latitude. The fact that this has been found to be the case in so many different places shows that we have here to deal with no casual phenomenon. The accepted explanation, originated by Archdeacon Pratt, of Calcutta, in 1859, and since developed especially by Hayford and Bowie, of the U. S. Coast and Geodetic Survey, is that if we imagine a level surface to be drawn at a depth of about 100 kilometers, the stratum of matter above this, though varying in density from point to point, is approximately uniform, in the sense that equal areas of the surface in question bear equal weights. The altitude of the mountains is held to be compensated by the inferior density of the underlying matter, whilst the oceanic hollows are made up for by increased density beneath. Leaving aside the technical evidence on which this hypothesis is based, there are one or two points to be noticed. In the first place it suggests, as is highly plausible on other grounds, that the matter in the interior of the earth, below the stratum referred to, is in a state of purely hydrostatic stress, *i.e.,* of pressure uniform in all directions. So far as this stratum is concerned, it might be floating on an internal globe of liquid, although no assertion is really made, or is necessary, to this effect. But in the stratum itself, shearing forces must be present, and it is necessary to consider whether the actual material is strong enough to withstand the weight of continents and mountains, and the lack of lateral support due to the oceanic depressions. The researches of Professor Love and others show that this question can fairly be answered in the affirmative.

The accurate determination of the acceleration of gravity at any place is, of course, a matter of great delicacy. Not to mention other points, in the pendulum method the yielding of the support due to the reaction of the pendulum as it swings to and fro affects the time of oscillation. It may be recalled that so far back as 1818 Kater, in his absolute determination of the length of the seconds pendulum in London, was on his guard against this effect, and devised a test to make sure that it was in his case negligible. In a portable apparatus, such as is used for comparative determinations, it is difficult to give sufficient rigidity to the support, and a correction has, in some way, to be applied. Recently, Dr. Victor Meinesz, of the Dutch Survey, who has carried out an extensive gravity survey in Holland, has sought to minimize this effect by the use of pairs of pendulums swinging in opposite phases, and so reacting on the support in opposite senses. This has opened out a prospect of accurate gravity determinations at sea. The use of a pendulum method on a surface vessel is hardly possible, but a submarine when sufficiently immersed offers comparative tranquility, and it is hoped that the small residual horizontal motions may be capable of elimination, and the diminished vertical oscillation allowed for. The methods previously employed at sea which could claim any accuracy are those of Hecker. In one method, the pressure of the atmosphere is found in absolute measure from the boiling point of water and compared with the gravitational measure afforded by the barometer. In a more recent method, also devised by Hecker, and followed with some modifications by Duffield, the idea is to carry about a standard atmosphere, *i.e.,* a mass of air at constant volume and prescribed temperature, whose pressure is measured gravitationally by the barometer. Both methods are highly ingenious, but can not compete as regards accuracy with the pendulum method if this should be found practicable.

It is a matter of regret that the observational side of geophysics has, of late, been so little cultivated in this country. In India, with its wide opportunities, geodetic and gravitational work has long been carried on with high efficiency and has furnished essential material for the generalizations I have referred to. But in the home country, although we have an admirable topographical survey—whose headquarters by the way are here in Southampton—nothing so far as I know has been done towards a gravity survey since the time of Kater, more than a century ago. Proposals for the establishment of a formal geodetic institute, such as existed in some other countries before the war, which should embrace this as well as other subjects, have been urged, but have had to be abandoned owing to the exigencies of the time. It is therefore some satisfaction to record that a modest beginning has been made at Cambridge by the institution of a readership in geodesy, and that when the requisite pendulum outfit is complete, it is hoped that a gravity survey of these islands may be initiated. The physical features are hardly so rugged that sensational results such as were found in India are to be expected, but it is desirable that the work, which will involve comparatively little labor and expense after the initial steps, should be carried out. The example of Holland shows that in a country which has no outstanding features at all a survey may reveal peculiarities which are at all events of considerable interest. I may add that it is contemplated that the Cambridge apparatus should also be designed to eliminate the disturbing element I have mentioned, and that it should be available for determinations at sea. It is perhaps not too much to hope that with the cooperation of the navy, the gravity chart of the world, which is so far almost a blank as regards the ocean, may in this way be gradually filled in.

The distribution of the intensity of gravity over the surface of the earth gives by itself no positive information as to the distribution of density throughout the interior, though the contrary view has sometimes been held. For example, a spherical globe with a uniform intensity of gravitation over its surface would not necessarily be homogeneous, or even composed of spherical strata each of uniform density, however plausible this might be on other grounds. Consequently, there is room for hypothesis. There are certain tests which any hypothesis has to satisfy. It must account for the observed distribution of gravity, and having regard to the phenomena of precession, it must give the proper relation between the earth's moments of inertia about a polar and an equatorial axis. It may be added that it should be fairly consistent with the ascertained velocities of seismic waves at different depths, and the degree of elasticity which it is allowable to assign to the material. The somewhat artificial laws of density adopted by Laplace and Roche, respectively, mainly on grounds of mathematical convenience, have lost much of their credit. A more natural law, suggested indeed by Thomson and Tait in 1867 in their book on "Natural Philosophy," has since been proposed in a more definite form by Wiechert. On this view, the earth is made up of a central core of about four fifths the external radius, of high density, about that of iron, surrounded by an envelope of about the density of the surface rocks. This is, of course, only to be taken as a rough picture, but it satisfies the requirements I have mentioned, and is apparently not incompatible with the seismic data.

In all speculations on the present subject, considerations as to the thermal history of the earth and the present distribution of temperature in the interior play an essential part. The apparent inconsistency between the requirements of physics and geology was long a matter of controversy, and has given rise to keen debate at these meetings. Lord Kelvin's historic attempts to limit the age of the earth by consideration of the observed temperature gradient as we go downwards from the surface lost their basis when it was discovered that the rate of generation of heat in the processes of radioactive change was amply sufficient to account for the present gradient, and would even be far more than sufficient unless the amount of radioactive material concerned were strictly limited. Assuming an average distribution of such material similar to what is found near the surface, a stratum of some 16 kilometers in thickness would provide all that is wanted. Radioactive speculation has gone further. A comparison of the amounts of uranium and of the end-products associated with it has led to estimates of the time that has elapsed since the final consolidation of the earth's crust. The conclusion is that it must lie definitely between 10^9 and 10^{10} years. The figure is necessarily vague owing to the rough value of some of the data, but even the lower of these limits is one which geologists and biologists are, I believe, willing to accept, as giving ample scope for the drama of evolution. We may say that physics has at length amply atoned for the grudging allowance of time which it was once disposed to accord for the processes of geological and biological change. The radioactive arguments on which these estimates are based are apparently irrefutable; but from the physical point of view, there are reasons why one would welcome an extension even of the upper limit of 10^{10} years, if this could possibly be stretched. For if this barrier be immovable, we are led to conclusions as to the present internal temperature of the earth which are not quite easy to reconcile with the evidence as to rigidity to be referred to in a moment. In the space of time I have mentioned, enormous as it is, the great mass of the earth could hardly have cooled very much from the temperature when it was in a state of fusion. The central portion, whatever its nature, and however high its thermal conductivity, is enclosed by a thick envelope of feebly conducting material, just as a steam boiler, for instance, may be jacketed with a layer of asbestos. To take a calculable hypothesis, we may assume with Wiechert that we have a central core of three fourths the earth's radius, with an outer shell of rock. We may give the core any degree of conductivity we like, for mathematical simplicity we may even regard it as infinite. Then, if the outer layer consists of material having some such conductivity as the surface rocks, the internal temperature would take to fall to one half its original value a period of at least ten times the limit I have named. It is obvious that the details of the assumption may be greatly varied without affecting the general conclusion of a very high internal temperature.

The question as to the degree of rigidity of the earth has so often been dealt with that a brief recapitulation will suffice. It was about the year 1862 that Kelvin first pointed out that if the earth as a whole were only as rigid as a globe of glass or even steel, it would yield so much to the deforming action of the solar and lunar tidal forces as seriously to affect the amplitudes of the oceanic tides, which are a differential effect. Unfortunately, the tides are so much complicated by the irregular distribution of land and sea that a comparison of the theoretical amounts which they would have on the hypothesis of absolute rigidity with the actual values is hopeless. The fortnightly tidal component, due to the changing declination of the moon, is probably an exception, but the difficulty here is to extract this relatively minute component from the observations, and the material is consequently imperfect. The problem was attacked in a different way by G. and H. Darwin in 1881. The horizontal component of the lunar and solar disturbing forces must deflect the apparent vertical, and it was sought to measure this effect by a pendulum. The quantities to be determined are so excessively minute, and the other disturbing forces so difficult to eliminate, that the method was only carried out successfully by Hecker in 1907, and subsequently by Orloff in Russia. The results on the whole were to the effect that the observed deflections were about three fifths of what they ought to be if the earth were perfectly unyielding, and were so far in accordance with estimates previously made by Darwin and others, from the somewhat imperfect statistics of the fortnightly tide. There was, however, a discrepancy between the results deduced from the deflections in the meridian and at right angles to it, which gave rise to much perplexity. The question was finally set at rest by Michelson in 1916. He conceived the idea of measuring the tides produced in two canals (really two pipes half filled with water) of about 500 feet long, extending one N. and S., the other E. and W. These tides are, of course, of a microscopic character, their range is of the order of one hundredth of a millimeter, and they could only be detected by the refined optical methods which Michelson himself has devised. The observations, when plotted on a magnified scale, exhibit all the usual features of a tide-gauge record, the alternation of spring and neap tides, the diurnal and semidiurnal lunar tides, and so on. The theoretical tides in the canals can, of course, be calculated with great ease, and the comparison led to the result that the ratio which the observed tides bore to the theoretical was about .69, being practically the same in both cases. The whole enterprise was as remarkable for the courage of its inception as for the skill with which it was carried out, and was worthy of the genius which has accomplished so many marvels of celestial and terrestrial measurement. The perplexing discrepancy in the results obtained by Hecker at Potsdam is no doubt to be explained by the attraction of the tidal waters in the not very remote North Sea, and by the deformation due to the alternating load which they impose on the bottom. In Chicago, near the center of the American continent, these influences were absent.

The question may be asked, What is the precise degree of rigidity which is indicated by these observations, or by others which have been referred to? Various answers have been given, based on observations of the tides, of the lunar deflection of the vertical, and of the period of the earth's Eulerian mutation, on which I have not touched. The estimates have varied greatly, but they are all high, some of them extremely high. That they should differ among themselves is not surprising. The material is certainly not uniform, either in its elastic properties or the conditions to which it is subject, so that we can only speak of the rigidity of the earth as a whole in some conventional sense. Larmor and Love have shown that all the information that can be gathered, whether from the tides or from the Eulerian mutation, can be condensed into two numerical constants. This leaves a large degree of indeterminateness as to the actual distribution of elasticity within the earth. It is at all events certain that in regard to tidal forces the great bulk of the material must be highly rigid.

In leaving this topic, it may be recalled that it was in this same connection that Kelvin was led to initiate the method of harmonic analysis as applied to the tides, as well as to accomplish much brilliant mathematical work, whose importance is by no means limited to the present subject. The whole theory of the tides and cognate cosmical questions afterwards became the special province of George Darwin, but after his death, work on the tides was almost at a standstill, until it was resumed by Professor Proudman and his associate, Dr. Doodson, in the recently established Tidal Institute at Liverpool. They have already arrived at results of great theoretic as well as practical interest, some of which I understand are to be brought before the association at this meeting.

Within the last twenty years or so, light has come on the elastic properties of the earth from a new and unexpected quarter, *viz.*, from a study of the propagation of earthquake shocks. It is pleasant to recall that this has been largely due to efforts especially fostered, so far as its means allowed, by this association. To John Milne, more than to any one else, is due the inception of a system of widely scattered seismological stations. The instruments which he devised have been improved upon by others, notably by Galitzin, but it is mainly to his initiative that we are indebted for such insight as has been gained into the elastic character of the materials of the earth, down, at least, to a depth of half the radius. It may be remarked that the theory of elastic waves, which is here involved, was initiated and developed in quite a different connection, in the persistent but vain attempts to construct a mechanical representation of the luminiferous ether which exercised the mathematical physicists of a generation or two ago. It has here at length found its natural application. One of the first problems of seismologists has been to construct, from observation, tables which should give the time an elastic wave of either of the two cardinal types—*viz.*, of longitudinal and transverse vibration—takes to travel from any one point of the earth's surface to any other. It has been shown by Herglotz and Bateman that if these data were accurately known it should be possible, though naturally by a very indirect process, to deduce the velocities of propagation of the two types throughout the interior. Such tables have been propounded, and are in current use for the purpose of fixing the locality of a distant earthquake when this is not otherwise known. They are however admittedly imperfect, owing to the difficulty of allowing for the depth of the focus, which is not always near the surface, and is sometimes deep-seated. This uncertainty affects, of course, the observational material on which the tables are based. Some partial corrections have been made by Professor Turner, who almost alone in this country, amidst many distractions, keeps the study of seismology alive, but the construction of accurate tables remains the most urgent problem in the subject. Taking however the material, such as it is, the late Professor Knott, a few years ago, undertook the laborious task of carrying out the inverse process of deducing the internal velocities of the two types of waves referred to. Although it is possible that his conclusions may have to be revised in the light of improved data, and, it may be, improved methods of calculation, they appear to afford a fairly accurate estimate of the wave velocities from the surface down to a depth of more than half the earth's radius. Near the surface the two types have velocities of about 7.2 and 4 km per second, respectively. These velocities increase almost uniformly as we descend, until a depth of one third the radius is reached, after which, so far as they can be traced, they have constant values of 12.7 and 6.8 km per second, which, by the way, considerably exceed the corresponding velocities in iron under ordinary conditions. The innermost core of the earth, *i.e.*, a region extending from the center to about one fourth of the radius, remains somewhat mysterious. It can certainly propagate condensational waves, but the secondary waves are hard to identify beyond a distance of 120° of arc from the source of disturbance. Knott himself inferred that the material of the central core is unable to withstand shearing stress, just as if it were fluid, but this must at present remain, I think, uncertain.

It should be remarked that the wave-velocities by themselves do not furnish any information as to the elasticities or the density of the material, since they involve only the ratios of these quantities. The relation between the two velocities is, however, significant, and it is satisfactory to note that it has much the same value as in ordinary metals or glass.

It is to be regretted that at present so little is being done in the way of interpretation of seismic records. Material support in the way of more and better equipped stations is certainly needed, but what is wanted above all is the coordination of such evidence as exists, the construction of more accurate tables, and the comparative study of graphical records. These latter present many features which are at present hard to interpret, and a systematic comparison of records of the same earthquake obtained at different stations, especially if these are equipped with standardized instruments, should lead to results of great theoretical interest. The task will be a difficult one, but until it is accomplished we are in the position of a scholar who can guess a few words in an ancient text, possibly the most significant, but to whom the rest is obscure.

Even on this rapid review of the subject it should be clear that there is an apparent inconsistency between the results of two lines of argument. On the one hand, the thermal evidence points to the existence of a high temperature at a depth which is no great fraction of the earth's radius, so high indeed as to suggest a plastic condition which would readily yield to shearing stress. On the other hand, the tidal arguments, as well as the free propagation of waves of transversal vibration at great depths, indicate with certainty something like perfect elasticity in the mathematical sense. The material with which we are concerned is under conditions far removed from any of which we have experience; the pressures, for instance, are enormous; and it is possibly in this direction that the solution of the difficulty is to be sought. We have some experience of substances which are plastic under long-continued stress, but which behave as rigid bodies as regards vibrations of short period, although this combination of properties is, I think, only met with at moderate temperatures. It is conceivable that we have here a true analogy, and that the material in question, under its special conditions, though plastic under steady application of force, as for instance centrifugal force, may be practically rigid as regards oscillatory forces, even when their period is so long as a day or a fortnight. But beyond that we can hardly, with confidence, go at present.

I have chosen the preceding subject for this address, partly because it has not recently been reviewed at these meetings, and also for the opportunity it has given of urging one or two special points. It is evidently far from exhausted—the loose ends have indeed been manifest—but this should render it more interesting. It furnishes also an instance, not so familiar as some, of the way in which speculations which appear remote from common interests may ultimately have an important influence on the progress of science. It is true that the secular investigations into the form of the earth's surface have an importance in relation to geodesy, but certainly no one at the time of Laplace's work on this matter would have guessed that he was unwittingly laying the foundation of the whole mathematical theory of electricity. The history of science is indeed full of examples where one branch of science has profited by another in unexpected ways. I would take leave just to mention two, which happen to have specially interested me. It is, I think, not generally understood what an important part the theory of elasticity played in Rayleigh's classical determinations of the relative weights of the gases, where it supplied an important and indeed essential correction. Again, the mathematical theory of hydrodynamics, in spite of some notable successes, has often been classed as a piece of pure mathematics dealing with an ideal and impossible fluid, elegant indeed, but helpless to account for such an every-day matter as the turbulent flow of water through a pipe. Recently, however, at the hands of Prandtl, it has yielded the best available scheme of the forces on an aeroplane, and is even being appealed to to explain the still perplexing problem of the screw-propeller.

To promote this interaction between different branches of science is one of the most important functions of our association, and differentiates it from the various sectional congresses which have from time to time been arranged. We may hope that this meeting, equally with former ones, may contribute to this desirable end.

Let me close with a local reference. The last fifty years have seen the institution of local universities and university colleges in many parts of this country and of the Empire at large. Through these agencies the delights of literature, the discipline of science, have been brought within the reach of thousands whose horizons have been enlarged and their whole outlook on life transformed. They have become centers, too, from which valuable original work in scholarship, history and science has radiated. The University College of Southampton is now contemplating an increased activity and a fuller development. In this ambition it has, I am sure, the best wishes of us all.

HORACE LAMB

SCIENCE AND SOCIAL ETHICS[1]

PRIMITIVE man, with his rudimentary knowledge of good and evil, could not attain a level of existence much above that of the brutes, in spite of the superiority of his brain. Even to-day, men live almost as wild animals in the tropical forests of South America. The remains of Paleolithic man in Europe show us that he had a brain as large as ours, and his art proves his capacity for understanding; yet he lived in what we consider a barbaric state. Gradually, by slow and painful steps, he acquired knowledge and with its aid developed skill and undertook what we call, with boastful exaggeration, the conquest of nature. In reality, he learned to play a game with nature, increasingly complex and productive of results as he learned more and more of the rules. This game, as we now find it, is what we call civilization, and it needs little argument to prove that for its maintenance we require all the knowledge we can obtain, organized into what we call science. We can not even remain where we are; we are compelled by the logic of events to go forward or backward, and progress depends on knowledge. Good intentions are of little avail without it, and the ignorant are like poor players who, doing the best they can, ruin the music of an orchestra. Thus it is impossible to be good without being wise, if we understand the word good in a pragmatic sense, as meaning good for something. Yet we must agree that science alone can not adequately minister to human needs. If a human being is nothing more than a temporary arrangement of atoms of carbon, hydrogen, oxygen, nitrogen and some other elements, our whole conception of human values seems to have little basis in reality. Or rather, is what reality it possesses unstable, evanescent, insignificant in relation to the universe? Is human life a tragedy because a comedy, a thing so ridiculous with its serious poses and heroic gestures that the gods, if there be such, must be convulsed with laughter? Well, we do not believe that for a moment; we could not believe it and be sane. Huxley was perhaps the most typical exponent of modern science, yet his great friend Michael Foster had this to say of him:

> Great as he felt science to be, he was well aware that science could never lay its hand, could never touch even with the tip of its finger, that dream with which our little life is rounded; and that unknown dream was a power as dominant over him as was the might of known science; he carried about with him every day that which he did not know as his guide of life no less to be minded than that which he did know.

[1] Read at the symposium held by the Southwestern Division of the American Association for the Advancement of Science, Boulder, June 8, 1925.