THE ANNIVERSARY ADDRESS OF THE PRESIDENT.

RICHARD DIXON OLDHAM, F.R.S.

DEATH has not dealt heavily with us in numbers, but among those whom we have lost during the last twelve months are three of our distinguished Foreign Members, and from the list of our Fellows we miss one master mind of his time.

In the death of Prof. CHARLES LAPWORTH on March 13th, 1920, we have to mourn one who stands in the front rank of geologists, worthy to be placed among the masters of the science. In every branch that he touched—stratigraphy, tectonics, petrology, palæontology, the work that he did was not only brilliantly original and enduring, but it has fertilized, and will continue to fertilize, the research of others.

Born in 1842 at Faringdon, in Berkshire, his early life was spent at Buckland, in Oxfordshire. His education, as well as the professional work which he discharged in the South of Scotland up to the age of 39, appear to have been purely literary, but he managed to acquire for himself a knowledge of science. Interest in the origin of landscape, and in the rocks to which it owes its features, was quickened by his discovery of fossils in strata regarded as barren; and the attraction became insistent between 1866 and 1869, when he began a systematic investigation of the rocks of the Southern Uplands, partly alone, and partly in the company of his friend James Wilson.

He soon saw that the Uplands area was much more complicated than had previously been realized, and that the only way in which its structure could be unravelled was by detailed mapping on a scale larger than had been employed. For such work he was eminently fitted, in the possession of an acute faculty for discriminating minute lithological differences and an excellent memory for lithological types, combined with a good eye for a country and for a fossil, and patience to search and collect exhaustively. Moreover, he was able and willing to supplement published topographical maps by personal survey, making large-scale plans of crucial areas on which there was room to record his own very detailed observations.

The results appeared in a succession of papers, culminating in the miners. He was elected a Fellow of our Society in 1868, served on the Council in 1902–1903, and died on December 13th, 1920.

George Hogben, born at Islington in 1853, graduated at Cambridge in 1877, and entered the teaching profession. In 1881 he went to New Zealand as mathematical and science-master of the Christchurch Boys' High School, and, after holding some other posts, was made Inspector-General of Schools under the New Zealand Education Department, up to his retirement in 1915. His natural bent was mathematical and physical, and he found time to prepare and publish a number of papers dealing with these subjects, despite his activity in educational matters. It was from this side that he touched our science, by his contributions to the study of New Zealand earthquakes, and by his exertions in rousing an interest in Seismology, both the older and the new, in Australasia. He was elected a Fellow of our Society in 1911, and died on April 26th, 1920.

George Sweet was born at Salisbury, but spent most of his life in Australia, where he was a manufacturer of pottery. He was always a keen geologist, and served as second in command to Sir T. W. Edgeworth David in his expedition to Funafuti. He made extensive collections of fossils from the Carboniferous and Cretaceous rocks of Queensland, and was joint author with C. C. Brittlebank of a description of the glacial deposits of the Bacchus Marsh District. He was elected a Fellow of this Society in 1890.

In the preparation of the foregoing notices of deceased Fellows I am indebted to Sir William Boyd Dawkins, Dr. Walcot Gibson, Mr. F. W. Harmer, Dr. John Horne, Mr. G. W. Lamplugh, Prof. J. E. Marr, Prof. A. C. Seward, Prof. W. J. Sollas, Sir J. J. H. Teall, Prof. W. W. Watts, and many others, too numerous to mention in detail, for assistance and information readily rendered. To all these I express my gratitude and thanks for the help accorded.

ΓΝΩΘΙ ΣΕΑΥΤΟΝ.

'Know your Faults.'

Custom has decreed that on these occasions your President shall deliver an address, which is usually devoted to a review of the past history, of the present condition, or of the future needs of some department of Geological Science. To-day I propose to follow neither of these courses, but to make a digression into the philosophy of our science, to examine the meaning of some of the words which we use, and to take for my text that motto which, blazoned in letters of gold from the ancient temple of Delphi, may be translated by geologists as 'know your faults.'

Faults there are, and many, of observation, of description, of interpretation, but they will only be considered in connexion with faults in the technical meaning of fractures of rock, along which movement of the opposite sides has taken place. These, as the text-books tell us, are of two kinds, normal or reversed; the classification arose in the coalfields of England, where the phenomenon was first studied in detail, and where, with few exceptions, the hade of the fault is towards the downthrow, so that it was natural to regard this as the normal condition, and a 'normal' fault was synonymous with one in which the hade was towards the downthrow, the exceptional cases in which the reverse condition of a hade towards the upthrow was found being distinguished as 'reversed.'

So long as the nomenclature was confined to the region in which it originated, or so long as the purely geological connotation of the words was remembered, no harm could result from the terms made use of; but thought is by no means free, it is trammelled by the limitation of the human intellect and the impossibility of omniscience, by limitation of our vocabulary, and also by the variation in the meaning of words, according to their context or the occasion on which they are used. As a consequence of this, the 'normal' fault came to be regarded as normal in the untechnical sense of the word; the generalization was extended from the district in which it originated to the world at large, and text-books, even those of quite recent date, are found insisting on the prevalence of 'normal' faults and the rarity of reversed ones—yet it is very doubtful whether any such disparity of frequency really exists. Were I to draw on my own experienc alone, the conclusion would be the opposite, that reversed faults were the normal condition and the so-called 'normal' faults comparatively rare; this I find has equally been the experience of some other geologists whose detailed field-work has been mainly beyond the British Isles, but as a world-wide generalization it would probably be as incorrect as the opposite. The real truth appears to be that the prevalent type of faulting varies in different regions, and that there is not at present sufficient evidence to show which can be regarded as more frequent and therefore more normal in the ordinary sense of the word, or whether, taking the world as a whole, the one is not about as frequent as the other.

A few years ago, I had experience of what may be regarded as an instance of the effect of the double meaning of the word 'normal.' At that time I was interested in the amount of the vertical throw of faults, which had demonstrably originated as normal or reversed, in the special geological meaning of these words. The conditions of the enquiry excluded that large group of faults where the inclination from the vertical is so small that a transference from one class to the other might have been produced by tilting subsequent to the formation of the fault; and of those not so excluded instances of reversed faults with throws of 6000 to 10,000 feet were on record, but I could find none of a definitely normal fault of more than about a couple of thousand feet. Yet it would have been dangerous to conclude that the possible limit of vertical throw was markedly less in the case of 'normal' than of 'reversed' faulting, for the alternative interpretation was equally possible, that, where the fault was normal, the observer saw no reason for emphasizing what might be understood without special mention; while, if the fault was reversed, it was a matter for record, as something out of the common.

In this instance the former interpretation may be the true one, and the limit of possible vertical throw of a definitely normal may be much less than in the case of a definitely reversed fault, or of the intermediate class of those having so small a hade that their original classification is uncertain; but this question will not be treated here. At present I am not concerned with theories of the origin of faults, but with a consideration of the meaning of the words employed to describe them, and among those having a special meaning in geology, which have already been used, we may find examples of the opposite extremes of safety or danger. The word 'hade' is a good example of the former class.

It has a perfectly definite and precise meaning, as the inclination of a sloping surface, measured from the vertical; apart from an obsolete use in agriculture, the word is restricted to mining or geology, and, consequently, it is free from any risk of being misunderstood, for it either carries with it a definite and precise intention, or is absolutely meaningless to the reader or hearer. As an example of the other class we may take the word 'normal'; not only has this word a general dictionary meaning, and connotation in ordinary intercourse, but it is also used as a technical term in several branches of natural knowledge, and in each the meaning is distinct and different, from that which it bears in other sciences and from that which the uninitiated in any science would attach to it. Hence, when using this word, we must be quite clear as to the precise meaning in which it is used, and avoid the fallacy, only too common, of making it first express a definite fact and then extending its meaning by the connotation which it would have in a different context; it gives an extreme instance of the danger involved in taking a word out of the general vocabulary of our language, and giving it a special technical significance, yet I would not, on that account, advocate its abandonment. It would be impossible to devise another term wholly free from the same danger, unless some entirely meaningless, and probably cacophonous, word were invented; for, so long as the name is derived in any way from existing words and roots, it must from the outset carry with it a more extended meaning than the special one intended to be implied.

A more weighty consideration, perhaps, is the desirability, in certain stages of knowledge, of making use of words which have not a rigid limitation of meaning, but rather of such as have ill-defined limits, capable of extension and modification as the advance of knowledge makes necessary or advisable. The old distinction of normal and reversed faults was made in the early days of our science; in the light of what we now know it is certainly inadequate, but, until our understanding of the processes, causes, and mechanism of the production of faults has advanced much beyond its present state, no approach to a final or complete classification is possible. For these reasons the old terminology may be retained, provided that we distinguish between the technical and untechnical meanings of the words, and remember that, though normal faulting in the former sense may also be normal in the latter in certain regions, it is most definitely not so for others, and not necessarily so for the world at large. We must also remember that these two classes are not each all of one kind and wholly distinct from the other; it may be, and indeed almost certainly is, the case that both normal and reversed faults comprise more than one group wholly distinct in origin and mode of formation, and that in some cases there is a closer relationship between normal and reversed faults than between them and others of nominally the same class.

There is, however, one very definite difference between normal and reversed faults, in that the former necessitate an increase in the horizontal distance of two points situated on opposite sides of the fault and the latter a decrease. In other words, normal faulting indicates an extension of the country affected by it, and reversed faulting a compression. This distinction has long been recognized, and more than half a century has passed since there appeared a paper by the Rev. J. M. Wilson[1]—whom I revere as my first teacher in geology—on the cause of contortion and faults. In this it was pointed out that the elevation of a tract of country would increase the length of the measurement across the elevated tract, while depression would give rise to a decrease, and calculation was made of the amount of the extension or compression which would be produced in this way. There can be no question that the cause assigned is a true one and that both extension and compression can be produced in this way; but, in the light of our present knowledge of the extent of contortion and faulting, it is evident that the cause is quantitatively inadequate, and that some other must be invoked to account for the amount of change in the original dimensions which is indicated by existing structure. This is not a matter to be dealt with here; but it may be noticed that, although extension of the horizontal dimensions of a faulted region would sufficiently account for the facts of normal faulting, provided that the necessary fractures were in existence, the case of reversed faults is much less simple, for, while they imply a reduction in the horizontal dimensions of the faulted region, it is easily demonstrable that, in many instances, they could not have been produced merely by compression in a horizontal direction. It is true that reversed faulting has been imitated in experiments on a small scale, and produced in those instances by compression; it is equally true that on the scale met with in Nature they might

[1] 'On the Cause of Contortion & Faults' Geol. Mag. 1868, pp. 205–208.

be so produced if there were no such thing as friction; but it is no less certain that when friction along the surface of the fault is taken into consideration it would be impossible for horizontal compression alone to give rise to displacement along a fault-surface, where the hade did not exceed 30° from the vertical, and doubtful if the hade were much less than 45°.

The reasoning is quite simple, clear, and conclusive; there is a certain angle of inclination marking the extreme slope at which one body will rest on another: if the slope is less no movement will take place, if steeper the upper one will slide over the lower down the slope separating the two. This limiting inclination is known as 'the angle of repose,' and varies according to the substance and nature of the surface; for highly finished and well-lubricated metallic surfaces it is only a few degrees, for dressed stone it is not far short of 30°, for an undulating surface, such as is found in even the cleanest-cut fault, the angle would be still higher. Now what is true where the bodies are affected by the vertical force of gravity is equally true of any other force acting in any other direction, the angle being measured from a plane at right angles to the direction of pressure, that is from the vertical, when compression takes place in a horizontal direction. Hence it results that horizontal pressure could not, by itself, give rise to movement along the fault-surface unless the hade were at least 30° from the vertical; where the hade is less, pressure would only lock the two surfaces more closely together, and increase the resistance to movement.

So far the reasoning is clear and conclusive; it is an error, however, to draw, as has been done, the conclusion that reversed faults of lower hade than this limiting angle could not have originated as such, but must have been formed as normal faults, to become apparently reversed through tilting subsequent to their formation. This is one, though not the only, possible deduction from observed facts; for the alternative is open to us that the forces which produced movement along the fault were either vertical, or possessed a considerable vertical component, in the direction in which they acted.

A similar conclusion would result from field-observations of reversed faults. I have myself repeatedly found reversed faults in soft tertiary shales and sandstones, and on the rare occasions when it was possible to find the actual fault-plane unobscured by surface-débris or weathering, it was frequently a clean-cut surface, along which movement had taken place, without any indication of crushing or deformation of the friable or plastic material on either side. There was no indication of any such resistance to movement as would have resulted from friction at the fault-surface, if the upthrow side had been forced upwards by being thrust against the inclined surface of the fault. On the contrary, the appearance was rather as if the pressure of the overlying on the underlying mass had been temporarily relieved, at the time when the displacement took place. Much the same appearance is frequently presented by normal faults; sometimes there is considerable crushing and deformation close by the fault-surface, such as would reasonably be expected if the movement had been due to the overlying mass sliding, by its own weight, over the inclined surface of the fault; at other times, however, no such appearance is met with, and even soft rock, easily bent or broken, lies on either side of the fault-surface almost as uninjured as if the two sides had been separated from each other when the movement took place.

We must also consider that very large class of faults, sometimes of great vertical throw, in which the fault-surface is either vertical, or so nearly vertical that it is difficult to decide the direction of the hade. In these, and especially where the throw is large, we can hardly attribute their origin simply to extension or compression in a horizontal direction, and we seem compelled to invoke the action of some force acting vertically, or with a very large vertical component, in its direction; and more than that, it must have been one which acted with much greater effect on one side of the fault than on the other, or, possibly, in opposite directions on opposite sides of the fault.

Paradoxical as it may seem, this is by no means physically impossible. Some years ago, when discussing the displacements of the ground which took place along the San Andreas Fault, in connexion with the South Californian earthquake of 1906,[1] I had occasion to refer to the very complicated stresses which are set up in a body subjected to compression, or extension, in one direction, but free to change its form in another, and exhibited to you a model illustrating how a force acting in one direction on the body as a whole might set up stresses, and give rise to displacement, within it, in a wholly different direction. The displacements dealt with on that occasion were of comparatively small amount;

[1] Q. J. G. S. vol. lxv (1909) pp. 1–16.

but it is not possible to place a limit on those which could originate in a similar manner, and it is, at least, not impossible that the movements revealed by faulting may have originated in some analogous way, and that the direction of the forces acting immediately on either side of the fault may have been very different from that of the ultimate influence to which they were due.

Geologists have recognized that to some such cause we must attribute the origin of the fractures which traverse all rocks, frequently with a remarkable parallelism and regularity of direction; but they have not sufficiently recognized that the same cause which gives rise to the fractures may equally be the cause of movement along the surface of the fracture, this movement taking place simultaneously in opposite directions on opposite sides of the fault. Generally faults are too numerous, and form too complicated a system, to allow of such a cause as has been mentioned being recognized in the effect; but all who have had experience of geological survey must have come across instances of faults that can only be detected by close and detailed survey, or by underground workings in mines and quarries. In these cases the disturbance of the even course of the boundary-lines is limited to the immediate neighbourhood of the fault, dying out on either side, just as the displacements in 1906 were localized to the immediate neighbourhood of the San Andreas Fault.

It is no part of my present aim to enter on a discussion of the physics of faulting, the purport of such reference as has been made being to elucidate the meaning, and the limitation of meaning, of the words which we use in a special sense, and to point out that in using the words 'upthrow' and 'downthrow' we must be careful to avoid any implication that the displacement was restricted to one side of the fault; for it may well have taken the form of a simultaneous movement in the same direction, but of different amount, or in opposite directions, upwards on one side and downwards on the other, of the surface of separation.

I have already referred to two classes of technical terms, one of which is a word not used outside some particular department of knowledge, and consequently meaningless to the uninitiated, the other a word in common use, to which a special limited meaning is given when used as a technical term. A third class is that of compound words, in which one or all of the components may be in general use, though the compound is confined to one special branch of science; and of this a very typical instance is provided by that special form of reversed fault, commonly known as an 'overthrust.' This word is distinctly a technical term special to geology, it is not used as a noun in ordinary speech or writing, nor is it used in any other branch of natural knowledge, yet, unlike the word 'hade,' it is far from meaningless apart from its technical significance, for it is compounded of two common, characteristically English, words, and as such carries with it a whole group of connotations. First, that the rocks now resting upon the surface of separation ought not normally to occupy that position, but have been brought there by displacement from that which they originally occupied; secondly, that the displacement has taken place by a movement of the upper mass over the lower; and, thirdly, that this movement was produced by some cause or force external to the area occupied by the material displaced, which has been thrust as an inert mass, influenced by, but taking no part in the production of, the power by which it was moved.

Doubtless the word was from the outset intended to carry with it the whole of these implications, and so long as it is used only in that extended sense and some other word made use of when a different group of connotations is intended, no objection could be raised; but, if it is to be retained as one of our special technical terms, it is eminently desirable that its meaning should be limited to the first, which expresses a fact, and that it should cease to imply the other two, which are of the nature of a theory of origin. In practice, however, it has generally been used in the extended interpretation, and on this has grown up a mass of controversy as to whether the upper mass was thrust over the lower, or the lower thrust under the upper, as to the direction from which the impulse came, and as to the ultimate cause to which it was due; but the greater part of these controversial writings resolves itself, on critical examination, into mere verbal dialectics, or is inconsistent with some of the fundamental principles of physics.

Taking these points in order, we may first consider that of the direction of movement. In the Scottish Highlands it is quite clear that the upper blocks have moved westwards relative to the lower, in Scandinavia the relative movement of the upper blocks has been eastwards, and in the Alps northwards; but it would be an equally true statement of the facts to say that the movement of the lower masses had been in the opposite direction, and more than this it is impossible to say. Leaving on one side the abstract

disquisitions of pure philosophy on the question of the possibility of there being such a thing as absolute motion, it is certain that it can only be expressed in terms of displacement, relative to some point which has to be accepted as fixed, and this point must necessarily lie outside the body regarded as in movement. From this it results that we cannot determine the direction of displacement of the masses, on either side of the plane of separation, by observations within the region of the displacements; and, as we have no means of reference to some external point, which can be regarded as unaffected, it results that we may only speak with certainty of the relative movements within the region of the overthrust. We are justified in speaking, or writing, for purely descriptive purposes, of an eastward or westward movement of the upper block over the lower, in order to avoid the long periphrases and digressions which would be unavoidable if the true meaning of the observations were always to be expressed in full; but in this case it is desirable to observe uniformity of practice, and always to regard the upper block as having moved relatively to the lower, and especially to remember that the expression is used merely as a convenience in description, not as implying any assertion of displacement or fixity relative to any point outside the area of the overthrust.

Having shown that argument as to the direction of movement is merely discussion of the words in which the facts are to be represented, I come to the question of the direction from which the pressure, to which the movement is attributed, was exerted. This discussion, again, involves a widespread fallacy that pressure can be one-sided; it permeates the great work of Suess, in which we find repeated reference to earth-waves advancing against resistant blocks, and in which the forms of the folds are repeatedly invoked as evidence of the direction from which the pressure came. The deservedly great influence of this work on geological thought has served to emphasize and perpetuate a very natural fallacy, derived from an imperfect interpretation of everyday experience. When a person pushes, for instance, against an unlatched door which yields to the pressure, it is natural for that person to attribute the result to the action of which he is conscious, and to take no account of the inanimate subject of his activities; properly considered, however, both take an equal and opposite part, and the door pushes back in exactly the same degree as the person pushes against it. This is easily recognized when the door is latched and cannot yield, in this case the resistant pressure of the door is felt and appreciated, yet the same takes place when it is free to move, though the opposing pressure is limited to that necessary to overcome the friction of the hinges and the inertia of the door. Once this limit is reached the door begins to move, and if the pressure exerted by the person is greater than that needed to move the door, at the rate which he wishes to impart, the result may be that he falls forward until he meets the greater resistance of the floor.

A similar fallacy is commonly to be found in the interpretation of experiments on the small scale, intended to illustrate the foldings and faultings of rocks. These generally take the form of a box-shaped receptacle, filled with sand, clay, or other material, the bottom and three sides being solid with each other, while the fourth side can be advanced by means of a screw or other mechanical contrivance. Here we appear, at first sight, to have a case of a solid immovable obstacle and a pressure, combined with movement, exerted from one side in the direction of the fixed obstruction; but when the circumstances are more closely examined, we see that whatever pressure is exerted by the movable side on the contents of the receptacle must be met by an equal and opposite pressure on the nut of the screw, the fulcrum of the lever, or generally on the fixed point from which the purchase is obtained. Looking still farther into the matter, we find that this fixed point must be connected, directly or indirectly, with the body of the receptacle, and so we see that the sides are only nominally fixed or movable, and that the one is drawn in exactly the same degree as the other is pushed; consequently, the pressure on the contents is not from one side towards the opposite one, but in equal amount and opposite directions from each towards the other. When, however, we transfer our consideration from the movements of the sides of the receptacle to the resistance offered by the contents, the conditions become much less simple: in the one case, we have three sides and the bottom all locked together and moving in unison, so that there is no frictional resistance to the forward movement of the contents as a whole; in the other, any displacement of the contents would be resisted by friction against two sides and the bottom of the box, this resistance being apart from that opposed by the contents to deformation, resulting from change in the dimensions of the receptacle. Hence it results that, while the pressure must in every case be equal and opposite, the resistance

to change of form will not be equal in opposite directions, and so the dislocations which result will not be symmetrical with reference to the apparently fixed and movable parts of the receptacle; but this want of symmetry must be attributed to inequality of distribution of the resistance, not to an unsymmetrical disposition of pressure or movement.

In Nature the conditions are further complicated by the fact that the material involved is much less uniform in character throughout the disturbed tract than in the small-scale experiments, and the character of the deformation even less dependent on the direction of the compression, so that if this were in a north-and-south direction the strike of the resulting folds or overthrusts may depart very considerably from the general east-and-west direction. A further complication is introduced by the fact that the rigidly-fixed sides and bottom of the box-shaped receptacle used in the experiment are not repeated in Nature, so that there is a possibility of relief being found laterally or downwards, instead of only upwards; or there may be compression simultaneously exerted in different, possibly widely different, directions.

To unravel all these conditions in detail is beyond our power, in the present state of knowledge, but the important point to be remembered is that we may not deduce from the character of the deformation which rocks have undergone any conclusion of absolute movement of one side of the compressed tract or of the other; all that we can learn, from observation within the disturbed tract, is that the horizontal dimensions have undergone diminution, but whether by movement of one side only or of both, measured relatively to some point outside the tract, cannot be determined. The same reasoning and conclusions apply with equal force to the compression indicated by overthrusts, and the deformations which have, at one time or other, been taken as evidence that the upper mass moved over the lower, or the lower under the upper, are seen to be merely disputation about words, for the structures appealed to are the expression of the resistance offered to deformation by the rocks in which they occur.

As in the case of the direction of movement, it may be a convenience to accept the common usage, incorrect though it be, when referring to the cause to which the displacements are due, so long as the language is understood to be merely descriptive, and so long as we do not allow ourselves, in further following up the train of thought, to be influenced by the words in which the facts of observation have been presented, rather than by the facts themselves. Yet it would be better that we should abandon this mode of expression altogether, for it is not only fraught with danger to ourselves, and likely to lead to erroneous reasoning, but also it is liable to misunderstanding by ungeological readers who, attaching a different meaning to the words from that which was intended, will conclude that we do not understand the subject with which we are dealing, and so our Philistines be led to scoff.

From the consideration of these two matters which have given rise to controversy, concerned almost entirely with words, by which things that really matter may be described, I now come to one which is a vital one, for it may involve a modification, and in some respects a radical change, in some of the fundamental principles, which have rather been tacitly accepted than definitely proved. In discussions, as in descriptions, of the phenomena or of the origin of these overthrusts, the masses involved have generally been regarded as passive, moving under the influence of external forces in the production of which they took no part. The notion is a natural one, it is the simplest and easiest way of interpreting the facts of observation; but its general acceptance must be very largely attributed to the influence of experiments on a small scale, which have themselves been suggested and directed by the hypothesis which they were intended to illustrate and investigate. In these we have an inert mass, variously composed to imitate, more or less, the rocks of the Earth's crust, and this mass is subjected to deformation by the application of external forces. In this way many of the structures which have been worked out by geological observations in the field were imitated on a small scale in the experiment, and the resemblance was accepted as evidence that the large-scale structures, met with in Nature, were produced, like the small-scale structures of the experiment, by the application of external forces. Difficulties, however, arise when we consider the conditions which are introduced by an increase of dimensions to the scale of Nature; and, when the mechanics of overthrusts are investigated, these difficulties become insuperable.

When one body is pressed against another by any force at right angles to the surface of contact, it may be caused to move by another force acting at right angles to the first, and the magnitude of the second force needful to produce movement bears a definite

ratio to the first, a ratio which depends on the nature of the material and the character of the surface of separation. This ratio is known as the 'coefficient of friction,' and is, numerically, the same as the tangent of the angle of repose. For a flat-dressed surface of stone the coefficient is about three-fifths of the weight of the stone, for a surface such as that of a so-called 'thrust-plane' it would not be less: consequently, to move a block of rocks 5 miles wide would need a pressure equal to that due to the weight of a column, of the same rocks and of the same cross-section, having a height of at least 3 miles, or just about the limit of height of column which average hard rock can bear without crushing.

From these figures it appears that the maximum possible width of the overthrust must be somewhere about 5 miles, if it moved as an inert mass under the influence of some external impulse: for, if the width exceeded this limit, the stresses would be greater than those which rock could bear or transmit, and relief would be found in some other way than by a general displacement along the whole width of the overthrust; but 5 miles is less than half the width of the mass moved in the Highland overthrusts, it is not more than a tenth of that of the Scandinavian, and a still smaller fraction of those which have been deduced in the region of the Alps. From this it might seem to be established that none of these overthrusts could possibly have been produced, and that there must be some error in the observations, or the inferences which have been drawn from them as to structure.

This reasoning, however, is not justifiable. We have again a case very like that which has been mentioned in connexion with what are ordinarily understood as reversed faults, and once more we have to face the alternative that the hypothesis of origin needs correction, not the facts of observation; but, before examining this, it is necessary to refer to one possible means of getting over the difficulty which has been encountered. If we might believe that the coefficient of friction along the surface of the thrust was less than that adopted in the calculation, the width of the blocks which could be moved would be correspondingly increased; but not in this way can sufficient increase be obtained, for even with the most perfectly formed and lubricated surfaces in mechanism the coefficient is not materially less than one-tenth, and the maximum width of block which could be moved would not be increased beyond about 30 miles. The actual surface along which movement took place being, to say the least, much less perfect than those which give so small a coefficient of friction, the maximum width that could be moved would in any case be less than has, in some instances, been shown by observation in the field. Resistance to movement might, however, be reduced if the downward pressure due to the weight of the upper block were, in some way or other, temporarily relieved, and if this relief were complete there would be no limit to the width of block that could be moved. It is not easy to conceive the means by which this could be brought about, nor is it necessary to consider the possibility, for the existence of mylonites, and other indications, of resistance to movement, given by the deformation and fracture of rock, are eloquent of the resistances which had to be overcome when the existing displacements were brought about. Taking these into consideration, it is evident that the frictional resistance must have been at least as great as is represented by the coefficient made use of, so that the width of 5 miles must be regarded as a maximum rather than a minimum limit of the width of the overthrust which could be moved by pressure from without.

From this we are led to the conclusion that the thrusts did not move simultaneously over the whole of their extent, but partially, first in one part then in another, each separate movement involving an area limited by the strength of the rocks and their power to transmit, or resist the effect of, pressure. Some years ago it might have been said that any supposition of this kind was physically impossible; but at the present day the change of volume which results from an alteration of the molecular grouping of the same chemical elements, expressed geologically as a different mineralogical constitution of rocks having the same chemical composition, or more briefly as a change of mode of the same norm, has opened up at least one means by which the desired effect might be produced. Doubtless the advance of knowledge will open up other possibilities, some of which might be indicated, though I shall not refer to them, as my present purpose is not to deal with things themselves, but with the words in which they are expressed.

One result of the acceptance of any such process as has been suggested is that the origin of overthrusts ceases to have any

resemblance to thrusting in the ordinary sense of the word; the movement would not be like that of a sledge, pushed bodily forward over the ground, but more akin to the crawl of a caterpillar which advances one part of its body at a time, and all parts in succession. A further result is that the motive power would have originated within the area of the overthrust, and, as we cannot conceive of this taking place in the dead rock of the upper block involved, we must put it in the lower one, more directly associated with those lower layers of the crust, in which many imperfectly understood changes are certainly going on, and probably many that are wholly unsuspected at present.

Without entering into advocacy of this hypothesis of origin of overthrusts, the claim must at least be made that it is a possible one, consistent with the facts revealed by observation, and not incompatible with our present knowledge, or ignorance, of the physics of the Earth's crust. If accepted, it follows that the word 'overthrust' suggests something quite different from what actually took place, and that the word 'undercrawl' would more nearly express the manner in which the thing referred to was brought about; yet I have no desire, certainly no intention, to suggest that a well-established term should be abandoned and replaced by one which may be just as misleading, if we define it otherwise than as a reversed fault of very low inclination from the horizontal. So long as the connotation is thus limited, one word is as good as another, and when, wishing to discuss origins and processes of formation, we go beyond this meaning, all words may be equally bad, if we allow collateral meanings of the constituent parts to influence our reasoning.

The theme might be expanded indefinitely, but enough has been said to point the moral, of the danger of loose use of words, and of the necessity of distinguishing clearly between things themselves and the terms in which they are described or mentioned. The lesson is no new one, for the fallacy, of using the same word in more than one sense, must be as old as language and logic; it is so well known that, for over 2000 years logicians have used a special term to describe, and have consistently warned us to avoid, it, yet, old as it is, it is ever new, and the warning needs repeated reiteration, for it is a form of fallacy to which mankind is naturally prone, and almost impossible of avoidance, in the finite limitation of human intellect, and of the vocabulary at its disposal. It is also one against which geologists must be especially on their guard, for the language which they use is of such modern origin that their special terms have not fully lost the connotation of their origin, and, consequently, the difficulty of differentiating between the special technical significance of the word on the one hand, and the literary, or literal, meaning of its derivation, on the other, is ever present and especially great.

February 23rd, 1921.

Mr. R. D. Oldham, F.R.S., President,
in the Chair.

James McCormick, M.Inst.Mech.E., Chief Inspector of Materials, etc., North-Eastern Railway Dock-Office, Hull, was elected a Fellow of the Society.

The List of Donations to the Library was read.

The following communications were read:—

1. 'On *Saccammina carteri* Brady, and the Minute Structure of the Foraminiferal Shell.' By Prof. William Johnson Sollas, M.A., Sc.D., LL.D., F.R.S., F.G.S.

2. 'Notes on the Views of the late Prof. Charles Lapworth with regard to Spiral Movements in Rocks during Elevation or Depression.' By Dr. Theodore Stacey Wilson, B.Sc., F.G.S.

Lantern-slides and microscope-slides were exhibited by Prof. W. J. Sollas, in illustration of his paper; and diagrams and models by Dr. T. Stacey Wilson, in illustration of his paper.

Impressions of moth-wings on stalagmite, from a cave at Burrington Combe (Somerset), were exhibited by Cecil Carus-Wilson, F.R.S.E., F.G.S.

March 9th, 1921.

Mr. R. D. Oldham, F.R.S., President,
in the Chair.

Ralph Walter Segnit, B.A., Balliol College, Oxford; Frederick Murray Trotter, B.Sc., Beehive Inn, Seghill (Northumberland); and Thomas Warde Whitfield, Fern Bank, The Avenue, Trowbridge (Wiltshire), were elected Fellows of the Society.

The List of Donations to the Library was read.

The following communications were read:—

1. 'The Surface of the Marls of the Middle Chalk in the Somme Valley and the Neighbouring Districts of Northern France, and the Effect on the Hydrology.' By William Bernard Robinson King, O.B.E., M.A., F.G.S.

2. 'The Bala Country: its Structure and Rock-Succession.' By Miss Gertrude Lilian Elles, M.B.E., D.Sc., F.G.S.

Specimens of fossils were exhibited in illustration of Miss G. L. Elles's paper.
