10. *On the* APPLICATION *of* QUANTITATIVE METHODS *to the* STUDY *of the* STRUCTURE *and* HISTORY *of* ROCKS. By the late HENRY CLIFTON SORBY, LL.D., F.R.S., F.L.S., F.G.S. (Read January 8th, 1908.)

[PLATES XIV–XVIII.]

CONTENTS.

I. INTRODUCTION.

IN the case of nearly all branches of science a great advance was made when accurate quantitative methods were used instead of merely qualitative. One great advantage of this is that it necessitates more accurate thought, points out what remains to be learned, and sometimes small residual quantities, which otherwise would escape attention, indicate important facts. Since it applies to nearly all branches of geology, it is necessarily a wide subject, but so connected together that it seems undesirable to divide it.

My object is to apply experimental physics to the study of rocks.

At least six different kinds of physical questions are involved, some of which have been sufficiently studied, but others require experiments which would be very difficult to carry out, and all that I can now do is to endeavour to deduce plausible results from what is known. In doing this, it may be necessary to assume cases sufficiently simple for calculation, which may but imperfectly correspond to natural conditions, so that the results may be only approximately correct. In some cases, facts seem to show that there are important properties connected with subsiding material which cannot be explained in a satisfactory manner. Notwithstanding this, it appears desirable to do the best that I can with the material at my disposal, hoping to lead others to do what I intended to do, and correct such errors as are now unavoidable.

In order to clear the way for subsequent detail, I describe a few general facts. To learn the final velocities of subsiding sediment I made many experiments, and found it a most complex question, requiring much more study. Coarse and somewhat fine sand-grains subside and collect at once on the bottom in a fairly-settled condition, yet the interspaces between the grains may amount to half the total bulk. Very fine-grained material, when more than 1 per cent. of the water, behaves in a totally-different manner, and, as a whole, somewhat like an imperfect liquid; and, in extreme cases, even after standing for a year, it may contain 90 per cent. of water. Very fine-grained sand possesses this latter property to a slight extent.

II. Final Velocities.

In the year 1859 I made many experiments, in order to learn the laws regulating the subsidence of various solid substances in water, more especially of sand and flakes of mica. Their size was first carefully measured with a micrometer-microscope, and those were selected which were of fairly-uniform diameter in all directions, but only the smallest were adopted for the calculations. These grains were carefully introduced separately in a wet condition into a tall jar of water, and the number of seconds which they occupied in subsiding a foot and a half was observed. In experimenting with spheres, cubes, and thin plates of glass, as well as with grains of sand and mica, it was found that the relation between the final velocity and the diameter is of so complex a character that it cannot be expressed in any simple manner, as though the result depends on complex conditions. I therefore fall back on experiment, and deal only with such cases as are of geological interest; but I may say that spheres, cubes, and plates of glass yield similar results.

Grains of Sand.

My best experiments were with grains varying from $\frac{2}{5}$ to $\frac{1}{200}$ inch in diameter, which gave final velocities from about a foot down to ·055 foot per second. For diameters varying from two-fifths to a fifth of an inch, the final velocity was found to agree well with the supposition that it varies as the square root of the diameter. When the diameter is about a tenth of an inch this law gives too large a result, but when it is about a fiftieth of an inch this excess increases, and when the diameter is about a hundredth of an inch the final velocity is only half what it would be if the same law holds true for the small grains as for the large. In fact, for grains measuring a hundredth of an inch in diameter and somewhat less, the final velocity varies nearly as the diameter. It seems to me probable that these facts may be explained by supposing that an adherent film of water is dragged down with the grains, the relative effect of which would be greater as their diameter became less. The results are closely as if this film were about a thousandth of an inch thick. My data were, however, not sufficient to prove this, and yet they make it sufficiently probable to be adopted provisionally. It may thus be supposed that the thickness of the film may increase when the particles move more slowly through the water; and, if so, the final velocity must depend on very complex conditions, and in the case of extremely-small particles it may decrease more and more than the value of *d*. We may thus easily understand why the subsidence of extremely-minute separate particles is so very slow, even as slow as an inch per day. However, this is greatly influenced by the amount of suspended matter, for, when there is much, the granules collect together into small pellets, which subside far more rapidly than the separate smaller granules. These facts must be carefully borne in mind in studying very fine-grained rocks.

It may be well to give in tabular form the approximate final velocities in feet per second, deduced from many experiments for separate grains of sand of various sizes, it being understood that the results would vary to some extent with the shape of the grains.

TABLE I.

Shortest diameter	$\frac{1}{5}$	inch	·910 foot	coarse sand.
,, ,,	$\frac{1}{10}$	,,	·680 ,,	
,, ,,	$\frac{1}{20}$	,,	·430 ,,	medium sand.
,, ,,	$\frac{1}{100}$	,,	·106 ,,	
,, ,,	$\frac{1}{200}$	,,	·052 ,,	very fine sand.
,, ,,	$\frac{1}{1000}$	,,	·010 ,,	

These velocities are of fundamental importance in many questions connected with stratified rocks.

Flakes of Mica.

Flakes of mica of small size and varying thickness follow the same general laws as sand; but, when the thickness remains constant, and the area is greater, the final velocity increases with the area up to a certain value and then decreases. The value of this maximum velocity varies closely as the square root of the thickness. I have no observation of facts which would be explained by the above peculiarities; and the flakes of mica in stratified rocks are usually too small for their application. However, it follows from my experiments that thin flakes of mica of considerable area would subside much more slowly than grains of sand of small area; thus we can easily understand why, as is so common, mica and fine sand occur in separate layers.

III. ANGLES OF REST OF SAND AND OF SMALL PEBBLES.

Sand.

A knowledge of the angle of rest of different varieties of sand, under various conditions, is of great interest, since its sine is a simple measure of the friction which would otherwise be difficult to ascertain, and must have had a preponderating influence in connexion with the drifting forwards and deposition of the material of many rocks. It sums up the general effect of the density, size, and shape of the grains, the character of their surface, and the relative lubricating influence of the superficial layer of water.

I made many experiments in a glass trough, so that I could measure the angle of rest under different conditions. One very important point is that the angle at which moving sand stops and accumulates differs materially from that at which it gives way, after having become stationary; and this explains many important facts.

Experimenting with the coarse angular sand of the Millstone Grit, washed to get rid of the decomposed felspar, and sieved so that the grains varied in size from about ·03 to ·07 inch, and averaged about ·05, I found that the angle of rest in water was about 41° when coming to rest, but about 49° when giving way after being at rest. In the case of sand varying in size from ·005 to ·020 inch, and averaging about ·010, the angle when coming to rest was about 34°, and after being at rest it gave way at 36°. In the case of very fine-grained sand from Alum Bay, in which the grains varied from ·001 to ·005, and averaged about ·003 inch, the angles were respectively 30° and 33°. It is thus clear that, to a slight extent, the fine sands acted more as a liquid, the difference between the angles being 2° or 3° instead of 8°. When the above-mentioned coarse and fine sands were mixed in equal quantities, it was difficult to prevent separation, since the coarse grains ran down over the fine, and accumulated at a higher angle than the fine; but, by using great care to prevent this, I found that the angles were about 34° (or the same as for the fine sand named above) and 38°, or 2° more than when no coarse sand was present.

Pebbles.

A rounded quartz-pebble measuring ·4 by ·25 inch easily ran down a slope of the coarse Millstone-Grit sand at 25°, and came to rest when it was at about 20°. It scarcely sank at all into the surface of the sand. In the case of the finer sand, the pebble just ran down at 20°, but did not sink much. When in water the very fine Alum-Bay sand is extremely soft and mobile, so that even a pebble measuring only ·2 by ·15 inch half sank into it, and would not run down the surface until the whole gave way. All these facts agree well with the supposition that the thin film of water adhering to the grain has more and more effect on the properties of the material in proportion as the particles are smaller. Thus, if the thickness be the same for 0·1 as for 0·001 inch in diameter, I calculate that the relative effect would be 450 times as great in the latter case. This must have a most important influence in modifying the deposition of coarse and fine-grained material; and it seems to me that all the facts that I have described are of fundamental importance in studying stratified rocks, and well deserve a much more complete investigation.

Thus, for example, the above-described facts show that the different amount of sinking of the small pebble causes the friction to be 57 per cent. more in the case of very fine sand than in that of coarse. On the contrary, a current of water would have much less power, since only half its surface would be exposed. It would thus be almost impossible for a small pebble to occur in fine sand, since a current much less than sufficient to drift along the pebble would wash away the sand; whereas, in the case of the coarse Millstone-Grit sand, the pebble would be washed along much more easily, and the sand washed away with much more difficulty, so that it is easy to understand why pebbles are so common in such rocks as the Millstone Grit, and absent in the finer-grained sandstones of the Coal-Measures. The common occurrence of, as it were, a bed of small pebbles at the lower part of many beds of drifted sand is also easily explained, since on arriving at the top of the slope at the angle of rest of the coarse sand, the great majority of the pebbles would roll down to the bottom and only a few stick higher up in the sand. In a similar manner, these facts would probably explain many other details of structure which have attracted little attention.

Possible Explanation of the Angles of Rest of Different Sands.

As will be shown in the sequel, when sand is deposited and well shaken, the grains arrange themselves on an average so that the percentage of interspaces is the same as that deduced theoretically for such an arrangement as would give rise to depressions bounded by a mean slope of 30°, up which a grain would have to be raised to be carried away. If a general surface of such a kind were inclined at an angle of 30°, the above-mentioned small slope would be level: therefore the grain would not require any lifting, and the only force to be overcome to let it go free would be that required to start motion and overcome friction. Taking all the facts into consideration, it appears to me that the most probable supposition is that, when the slope is such that the grain slips off and slides down, the force of gravitation acting at that angle is just in excess of that which kept it in position, and that this force varies as the sine of the angle of the slope. Part of this is balanced by the conditions, and the rest by friction. If these suppositions are correct, it follows that the variation in the angle of rest for different kinds of sand depends mainly on these latter retarding conditions. Assuming that the sine of the angle of rest is a measure of all the forces just balanced, we may calculate as follows. For grains of ·05 inch in diameter the sine is ·755, and for ·01 it is ·588. Hence for each ·01 above ·01 the value of the sine increases about ·042. Subtracting this from ·588 we get ·546, which is the sine of 33·6°, or closely that of the angle of rest for grains ·003 in diameter. The excess over 30° may be due to the force necessary to start movement, o r to the somewhat different arrangement of the surface-grains from that assumed. These facts seem to show that the above-named resistance is of little value where the grains are small, but increases nearly as the diameter of larger grains. At all events, these suppositions explain the variations in the angle of rest sufficiently well for the purposes of this paper.

Relation between the Angle of Rest and the Velocity of a Current.

I trust that it will not be thought that I am making mountains of molehills, when I discuss in detail the properties of grains of sand; but it must be borne in mind that the very existence of some mountains must have depended on the properties of their constituent grains. As described above, the most probable angle of the slope of the minute depressions on the surface of sand is about 30°, up which a grain would have to be lifted before it would be washed along by a current. In addition to this, there would be friction to be overcome, so that the effective angle may be about 33°.

It seems to me, therefore, that these conditions are so closely the same as in the case of the angle of rest, that the sine of this angle may be looked upon provisionally as a measure of the resistance that must be overcome by a current just able to drift along the sand. At all events, this seems to give satisfactory results, when we compare one variety of sand with another.

IV. THE EFFECTS OF CURRENTS.

Velocity of Current Able to Start or Maintain Drifting.

As in the case of the friction of one surface on another, there is a difference between the current necessary to start the motion of sand and that which is necessary to maintain it. This is well seen in the case of the angle of rest—since the angle at which sand slips down after having been at rest, and that at which moving sand is brought to rest, differ materially. Very important results depend on this property. We may, I think, provisionally calculate the velocities of current required to start and maintain the drifting of different varieties of sand from these angles, as measured in each case. It is, however, important to bear in mind that drifting on a rippled surface is not the same as on one without ripples.

Conclusions to be drawn from the Change in the Angle of Rest in Various Rocks.

When material like sand or oolitic grains is drifted along the bottom to where the velocity of the current is so reduced by increased depth that the material can be no longer washed along, it falls down on a slope at the angle of rest. By this means a bed may have been formed several hundred yards long, and the angle of rest can easily be determined, allowance of course being made for the true dip of the strata. This sort of bedding should be carefully distinguished from irregular deposition, which may very properly be called false bedding. As it is formed by the drifting along of the material, I have always called it drift-bedding.

The value of the original angle of rest can usually be estimated from the nature of the deposit; but, in many cases, it has been subsequently much reduced by chemical or mechanical changes in the rock, and the alteration in the thickness of the bed can be learned by comparing the values of the tangents of the original angle of rest and of the present angle. In the case of irregular or thin drift-beds or drifted ripples, the original angle of rest may have been materially less than normal, because of the current sweeping down the slope, and thus it could not be relied on. There is also doubt regarding the original value when, as in some limestones, the rock has been so changed that the character of the material when deposited is imperfectly known. I much regret that I did not see the importance of these facts in years gone by, and did not measure the angle of rest in many cases suitable for these calculations; but fortunately I have sufficient data to show the kind of results that could be obtained by a more complete application of this method, now that all parts of the subject have been more developed.

Freshwater Limestone, Binstead (Isle of Wight).

The angle of good drift-bedding was found to be as a mean about 24°; and, assuming that originally it was about 34°, the reduction in thickness has been from 100 to 66, or a contraction of 34 per cent., which to a large extent may have been due to the filling-up of original cavities by carbonate of lime derived from aragonite-shells in close proximity.

Lower Greensand near Folkestone.

I measured several good cases, and found the mean angle of drift-bedding to be close on 20°, in sand which would originally have had an angle of about 34°. This would indicate a contraction of from 100 to 54, or of 46 per cent., which must to some extent be due to the filling-up of the interspaces, but also to removal of material by solution.

Magnesian Limestone.

There is so much uncertainty respecting many important particulars connected with this rock, that I should say nothing about it if it were not in the hope that it may help to clear up the difficulties. One thing that seems fairly certain is, that in South Yorkshire, North Derbyshire, and Nottinghamshire, the rock was not deposited as it now is. In many places the material has been drifted along the bottom, but it is difficult or impossible to know what would be the original angle of rest or the amount of the interspaces.

Many years ago I made a considerable number of chemical analyses, and found that, as a general rule, the specimens contained an excess of carbonate of lime above that which is found in a true dolomite. I then concluded that in many, if not all, cases this excess was due to infiltrated calcite. Sometimes there appeared to be good evidence that the rock had been changed from a limestone after deposition, but in others that dolomite-mud had been deposited originally. In the county of Durham the excess of carbonate of lime in the Magnesian Limestone is great; and it seems to me that some of the exceptional concretionary structures seen there may have been due, in part, to the original deposit having been to some extent aragonite, afterwards segregated and crystallized as calcite.

As determined by the boiling-water method described farther on, the empty spaces in the rock in South Yorkshire vary from 9 to 29 per cent., and in one case were increased from 11 to 21 per cent. by the action of dilute acid, perhaps due partly to the removal of infiltrated calcite. On the whole, these values do not differ materially from those found in rocks of Oolitic age, and, like them, indicate no great pressure.

I have fairly-complete particulars of two cases near Conisborough. In one at Crookhill the angle of a drift-bed is now 18°. If originally the material were of moderate grain, the angle of rest would have been about 34°: this would indicate a contraction from 100 to 48. The cavities might have been originally about 48 per cent. and are now about 8 per cent., so that, as indicated by them, the contraction has been to about 56·5 of the original. This indicates a removal of 8 or 9 per cent. of solid matter. In a good case at Cadeby the angle is 20°, but the cavities amount to 27 per cent.: this indicates a contraction of 54 per cent., and the removal of 17 per cent. of solid material. Taking the mean of the four determinations, it should appear that the rock is now about 52·5 per cent. of its original thickness and that 13 per cent. has been removed; these, however, must be looked upon as only rough approximations, because the original character of the rock is unknown.

Millstone Grit.

For some miles north and south on the west side of Sheffield, the Millstone Grit contains many excellent examples of characteristic drift-bedding. At one time, it was puzzling to find that the mean angle of the inclined beds in a quarry at Bell Hag, where the sand-grains measure on an average ·01 inch in diameter, is only about 25°, since the angle of rest for such sand is 34°. However, on further study it was found that this reduction in the angle agrees with what would be the effect of the alterations that have taken place in the rock. Pebbles of felspar are common, and on close examination it was seen that what was originally felspar-sand has been decomposed, and the resulting clay forced into the interspaces between the grains of quartz. In the case of such sand when recently deposited and not shaken, the interspaces amount to about 46 per cent., whereas they are now only 15 per cent., thus showing a contraction of 36 per cent. of the original volume. The tangent of the original angle is ·674, and therefore that of the altered rock should be 64 per cent. of this, which nearly agrees with the tangent of 25°, as seen in the rock. Hence, both the change in the angle and that in the amount of interspaces agree in showing that the thickness of the rock is about two-thirds of the original. As indicated by the angle of rest, the average contraction for all the above-described rocks is from 100 to 59, which is a very considerable change.

Drifting on a Horizontal Surface.

According to Du Buat ('Traité d'Hydraulique'), the velocity of a current near the bottom is about half the mean velocity of the whole depth. Possibly, however, that of the water in contact with the sand is still less. My experiments and observations showed that, in shallow water, the mean velocity of the current just able to wash up sand measuring about a hundredth of an inch in diameter is about ·4 foot per second. Before being moved, the surface would be very similar to that of sand inclined at the angle of rest, and it seems probable that, as in that case, the force necessary to lift the sand out of the depressions against gravity and overcome friction, so as to move it forward, must be nearly as the sine of the angle of rest, which in this case is ·59. The final velocity of such sand is ·106 foot per second, so that the calculated velocity of a current just able to move the sand would be $·59 \times ·106 = ·063$ feet per second, which is not quite a sixth of the observed mean velocity, or about a third of that near the bottom according to Du Buat. This lower velocity is, however, that in actual contact with the sand, which must certainly be considerably less than higher up. On the whole, considering all the circumstances, we may conclude provisionally that the velocity in contact with the sand is about a sixth of the mean velocity, although this might not be correct in the case of deep water.

Effects of Current on Sand.

Fifty-nine years ago, when I was living at Woodbourne, a country-house on the east side of Sheffield, there was at the bottom of the small park a brook entirely under my control. In order to investigate a number of questions, I constructed a place for experiment with some self-registering appliances. I could easily regulate and measure the depth and velocity of the current within certain limits. By these experiments, and by observations made in a clear brook at Fulwood (near Sheffield), I came to the conclusion that, when the velocity of the current is about 6 inches per second, sand with grains about a hundredth of an inch in diameter is drifted along slowly, and a surface is produced, grained in the line of the current, but no ripple-marks are formed. When the velocity is somewhat greater than 6 inches per second, ripples are produced. When it is about 1 foot per second, these are well developed and advance about 3 inches per minute, by the sand being washed up on the exposed side and deposited on the other; which velocity may be looked upon provisionally as an average for undoubted drifted ripples. If the velocity attains 18 inches per second, the ripples are destroyed by the washing-away of the sand; but the surface may still show graining in the line of the current. Much depends, however, on whether sand is or is not being deposited from above; since, when it is, ripples are produced at a somewhat lower velocity and advance more quickly. These results applied to the case of water varying from 1 to 8 inches in depth, and might be very different in the case of much deeper water. I have long felt that such experiments ought to be conducted on a much larger scale, but have never had the opportunity in a suitable and convenient place, free from disturbance. In the present state of the subject it may be assumed that, in the case of moderately-fine sand, the well-developed ripple-drift, so common in certain rocks, indicates a current with a mean velocity of about 1 foot per second.

Assuming that the sines of the angles of rest are a fair measure of the friction which must be overcome to move the sand when at rest, and to continue the motion when drifting over the same sort of sand, and also that the effective action of a current of about 1 foot per second varies as the velocity, I calculate out the following table. It must, however, be looked upon as little more than a provisional illustration, since it is possible that many other factors should be taken into account. They are not velocities, but the relative forces needed to move the sand along the bottom, where the current would be much reduced by friction.

TABLE II.

	Coarse sand.		*Fine sand.*		*Very fine.*	
Angles of rest	41°	49°	24°	26°	30°	33°
Sines of angles	·65	·75	·56	·59	·50	·54

Assuming, for the sake of simplicity, that the effective action of a current on grains of sand of varying size but of similar shape varies directly as the exposed surface, and that the frictional resistance to be overcome varies directly as their weight, the velocity of a current just able to drift them along would be when these two quantities are equal; that is to say, roughly speaking, that the necessary motive power would vary directly as the size of the grains. Now, as already described, the mean velocity just able to drift grains a hundredth of an inch in diameter is a little under ·5 foot per second, or say ·4 foot. Hence, in the case of Millstone-Grit sand measuring a twentieth of an inch in diameter, it would be about 2 feet per second; and for the very fine Alum-Bay sand $\frac{1}{300}$ inch in diameter, it would be ·13 foot. Combining these results with those deduced from the angles of rest, we obtain the following :—

TABLE III.

Size of grains	$\frac{1}{20}$ inch.	$\frac{1}{100}$ inch.	$\frac{1}{300}$ inch.
Mean current just able to drift...	2·00 feet.	0·40 foot.	0·13 foot.
Mean current just able to wash up	2·24 feet.	0·44 foot.	0·17 foot.

Though deduced from entirely different data, these results agree well with the fact that the weight of the grains varies as the cube of their diameter, and the surface exposed to the current varies as the square.

As shown later, the velocity at the very bottom just enough to drift the sand up the ripples is only ·09 foot, which agrees with the fact that a velocity of 12 inches washes up the sand vigorously; and, when the mean velocity is 18 inches and that at the bottom 3, the sand can scarcely maintain itself. We thus have two important limits—one just sufficient to wash it along, and the other to wash it away, which had no upper limit.

V. RIPPLE-DRIFT.

Only that structure is considered which is produced by a current moving in one direction, as shown by the detailed characters. This is a most interesting structure, since it enables us to ascertain with approximate accuracy, not only the direction of the current and its velocity in feet per second, but also the rate of deposition in fractions of an inch per minute. This introduction of minutes and seconds into geology may probably surprise those who are accustomed to deal with long geological periods, but it must be remembered that my minutes and seconds can be verified by experiment, which cannot be done with their long periods.

The production of this structure (see Pls. XV & XVI) involves a number of variable conditions, namely, the depth and velocity of the current; the size, shape, and density of the drifted material; the length and height of the ripples, and the rate at which deposition is taking place from above; and it is necessary to enter somewhat fully into detail, in order to show the data from which conclusions may be drawn.

It is convenient, in the first place, to consider the case when no deposit is being formed from the superjacent water, material being merely drifted along the bottom. Also, for the sake of simplicity, we may assume that the sand consists of grains having an average diameter of about a hundredth of an inch, and that the length of the ripples is about 3½ inches, which I find is a common size in many rocks. I shall also consider only their length and height, since the third dimension may be looked upon as uniform and as having no influence on the ratios under discussion.

By very carefully studying some excellent ripples on the shore at Ryde and Sandown, I found that, although their average length varied from 1·3 to 12 inches, their shape was almost identical, the exposed side being inclined at about 18° and the sheltered at about 30°. Hence, for ripples 3½ inches long, the height would be ·72 inch, which corresponds to what is seen in older rocks. However, for a reason which I do not fully understand, some ripples 3½ inches long are only about ·36 inch high, and the slopes are inclined at 9° and 19°. Those formed in my experiments at Woodbourne seem to have been of this character, and I found that with a current of about 1 foot per second they advanced 3 inches per minute. So far as I can judge, their length was about 4 inches, which would give a minute and a third as the time in which they would advance their own length, which I call their period. It seems very probable that this period would be nearly the same for ripples varying considerably in length, since the exposed surface from which the sand is washed up would vary directly as the length of the ripples, and we may, therefore, assume that the period of ripples 3½ inches long would be 1·33 minute. The question then is, what would be the period for those that are ·72 inch high? The amount of material to be drifted forward in their period would be $\frac{72}{36}=2$, and there is no reason to believe that it would be drifted along more quickly. On the contrary, it would have to be washed up a slope of 18° instead of 9°; and, adding the angle of rest when such sand gives way for the effect of the small depression, the extra inclination is 54° instead of 45° and the extra force required $\frac{\text{sine } 54^\circ}{\text{sine } 45^\circ} = 1{\cdot}14$. Hence, probably the period for ripples ·72 inch high would be $1{\cdot}33 \times 2 \times 1{\cdot}14 = 3$ minutes; but this must be looked upon as merely an approximation, which needs confirmation by experiment on a larger scale. It must also be remembered that all my calculations refer to sand of medium coarseness, with grains about a hundredth of an inch in diameter. The tables given in this paper supply the data for calculating the results for coarser or finer sand.

Ripple-Marks, etc.

My experiments at Woodbourne showed that when the velocity of the current was only about 0·1 foot per second no ripples were formed, even when sand was deposited from above. When the velocity was from a quarter to half a foot per second, ripples were produced when deposit took place, though not otherwise, but they did not advance. This appears, therefore, to be the condition necessary to produce such ripple-marks as are seen in some thinly-bedded rocks, which show almost or quite symmetrical ripple-forms, but little or no effect of drifting. When this does occur a true rippled surface is seldom visible; but, in a section perpendicular to the stratification, inclined laminæ are seen, and a surface of peculiar character is shown when the rock is broken parallel to the plane of bedding.

Production of Ripples and their Relations.

As shown by my experiments and observations, when the mean velocity of a current decidedly exceeds 6 inches per second, ripples are formed; and when it is 1 foot per second they are well developed and advance about 3 inches per minute, by the washing-up of the exposed side and deposition on the sheltered side. The formation of these ripples makes a considerable change in the conditions, since the sand must be drifted up the slopes. Very careful observation of excellent ripple-marks on the shore at Ryde and Sandown showed that the angle of the slopes exposed to the current was very nearly 18° in the case of both long and shorter ripples. The surface would be very similar to that when a horizontal one is tilted up 18°, and the grains would have to be washed up this as well as over the small depressions between the grains, so that for calculation we have $18°+36°=54°$. The sine of this multiplied by the final velocity is $\cdot85 \times \cdot106 = \cdot09$ foot per second, which multiplied by 6 gives ·54 for the same velocity, in close agreement with observation.

Washing-away of Ripples.

I found that in shallow water ripples are washed away when the mean velocity of the current is 18 inches per second and upwards. This is when, at the very bottom, it is so strong as not to allow sand to remain at an angle of 18°. This result may not apply in the case of deep water. I found, however, that some sand may remain at the bottom with a current of 18 inches per second, grained in the line of motion, though not in the form of ripples. Hence, although the normal conditions for a horizontal grained surface are not much above 6 inches per second, cases may occur when the velocity is 18 inches, as may often be seen in clear brooks; but it would usually be easy to distinguish the two conditions by studying their relation to ripple-drift. Considering all the complex factors, it is satisfactory to find that the observations can be harmonized quantitatively by a few probable suppositions.

Ripple-Drift with Deposition from Above.

The structure of ripple-drift shows that when deposit is formed from above, it is accumulated on the protected side of the ripple in thin layers at the angle of rest. That this deposition would reduce their period admits of no doubt, since the protected side would advance more rapidly. As the ripples move forward a portion is washed up from the exposed side, and an amount equal to that deposited from above is left, and covered up by the next ripple advancing from behind. It thus seems to follow that the amount drifted forward, independent of the deposition, is the same as when there is no deposit from above. The question is, what is the effect of the deposit on the rate at which this normal amount is washed along? Since the total to be removed would be greater, it is quite possible that it would have a retarding influence and lengthen the period of the ripple. At the same time, since, as I have shown, the velocity of the current some little distance from the bottom is considerably greater than on the actual surface of the sand, the subsidence of material would increase the velocity of the current at the bottom, and therefore shorten the period of the ripples. All these suppositions ought to be verified by experiment; but, in the meantime, it seems to me that we may assume provisionally that the above-mentioned two influences may so far compensate one another, that they may be neglected. I therefore calculate as follows:—The normal area of the section of a ripple perpendicular to the surface in the line of the current is $\frac{3{\cdot}5 \times {\cdot}72}{2} = 1{\cdot}26$ inch. The area of the material deposited is $3{\cdot}5\,d$, when d is the thickness of the deposit in inches. Then the period of the ripple would be $3 \times \frac{1{\cdot}26}{1{\cdot}26 + 3{\cdot}5\,d} = \frac{3}{1 + 2{\cdot}8\,d}$.

These values, however, must be looked upon as only approximate, but yet most probably of the true order of magnitude, and sufficiently near the truth to warrant the conclusions described later.

The rate of deposition would be $\frac{d}{\text{period}}$.

Length of Ripples.

The exact relation between the size of ripples and the conditions under which they are formed requires further study. If consideration were confined to those usually seen in rocks, it would appear most probable that their size depends to a great extent on the character of the sand. The smallest that I have seen are only about three-quarters of an inch long in a very fine-grained sandstone.

The usual size is 3 to 4 inches in medium sandstone. They are not well seen in the coarse-grained Millstone Grit, in which, however, they may be perhaps about a foot long. To judge from these facts alone, it would seem as though the length varied somewhat as the velocity of the current necessary to wash along the sand. The ripples seen on the sea-shore would in many cases agree with this supposition. The mean length at Ryde was 3·7 and at Sandown 6·9 inches. Now and then I have come across some which, so far, I am quite unable to understand. The most remarkable were at the northern end of the Menai Strait, where there was an extensive development of ripples some feet long. I was unable to learn the exact conditions under which they were formed, but still they make me think that other factors besides the size of the grains of sand may occasionally play a very important part. The current in the Menai is certainly strong, and the chief difficulty is, not to understand why the ripples are long, but why they are not washed away. It may be that the current along the bottom itself was not particularly strong, while that higher up was much greater, which determined the length of the ripples.

VI. Varying Size of the Grains.

Another question of much importance in connexion with the structure of sandstones and some limestones is the relative size of the grains found mixed together. In some cases these are nearly all of the same size, but in others, between certain limits, they differ almost as much as possible. In many sandstones, although there is considerable uniformity, numerous smaller grains occur in the spaces between the larger. The exact cause of this occurrence deserves more study; but, as bearing on the question, I may refer to the fact that a good many small grains may exist among the coarse without producing any marked change in the angle of rest. Many years ago I paid much attention to the general question, and contrived a simple instrument for readily measuring the size of the grains; but it is only lately that the study of a different class of rocks in Herefordshire has thrown light on the true nature of the problem. Unfortunately, direct observations are difficult in the case of consolidated rocks.

Taking everything into consideration, the most important general conclusion appears to be that more or less perfect similarity in the size of the grains usually indicates a sorting of the material by a current at the very bottom of comparatively-shallow water; whereas great irregularity in the size indicates that the material was deposited from much deeper water, in which there was little current at the bottom, though a good deal of current higher up. This is, of course, one of the most important points in connexion with many rocks.

Possible Connexion between the Structure of a Rock and the Depth of the Water.

If the final velocity (f) at which a grain of sand subsides in a deep current is the same as in clear still water, so that my experimental results can be utilized for calculations, it follows that the time taken to subside through a given depth (d) would be $\frac{d}{f}$ seconds. Also, if it were carried along by a current of V miles per hour for a distance of L miles the time taken would be $\frac{L}{V}$. If, then, we considered a case in which during the time a grain subsided from the top to the bottom, we should have $d = f \times \frac{L}{V}$. Since f is known by experiment, the depth depends on two independent variables, the value of which can be roughly estimated, so as to see whether the result is in any way probable. Assuming then that the grain of sand is a hundredth of an inch in diameter, having a final velocity of ·11 foot per second, and that the distance to which it can be drifted is 10 miles, by a current of 4 miles per hour, we have for the depth $\cdot 11 \times 9000 = 990$ feet $= 165$ fathoms, which appears to me so unreasonably great as to indicate some flaw in the argument. Possibly, in subsiding in muddy water the bigger grains collect into pellets with the finer and with organic matter of little density, so that the rate of subsidence is much less than ·11 foot per second. Supposing that it were only a tenth of that, the depth calculated as above would be only 16½ fathoms, which is not unreasonable. On this principle we could explain how fairly-coarse sand could be carried for some miles and accumulate with fine-grained material which had subsided from a lower level, where the velocity of the current was less. Quantitative results are at present out of the question, but it seems extremely probable that the difference in the structure makes it possible to distinguish between deposits formed from deep water and those formed from shallow. It is even possible that further study would enable us to form some estimate of the actual depth.

The chief defect in some of the foregoing conclusions is that the influence of the depth of the water is so imperfectly known. This probably cannot alter materially the relation between the sand and the current, on the actual surface of the bottom, but might considerably modify the relation between this and the mean velocity and that of the upper surface, so that some of the velocities given may not be strictly correct.

VII. Drift-Bedding.

What I have always called drift-bedding is formed when sand is drifted along, if the water is of proper depth and the current sufficiently strong to carry it on, until it arrives where the depth is so much increased, and the current so greatly reduced, that it is unable to wash sand any farther, the sand being therefore thrown down at the angle of rest, forming a bed the thickness of which corresponds to the increased depth. Numerous examples of this structure, on a small scale, may be seen on sandy roads after rain. The thickness of the bed (t) does not bear a constant relation to the depth of the water, either before or after deposition. It may be abnormally small, when the reduction in the velocity of the current merely causes the sand to be thrown down at a less angle than the normal angle of rest, owing to the current sweeping down the face of the slope, and causing the stratula to be S-shaped, curved at the top and bottom. On the contrary, the increase in depth may be indefinitely great, so as to give rise to a thick bed at the true angle of rest, perhaps modified at the bottom by a talus due to the giving way of the deposit, caused by breaking waves or other disturbances. Between these two extremes is what may be called a normally thick bed, where the increase in depth and the diminution of the current are just sufficient to allow of the sand accumulating at the true angle of rest, in a bed the thickness of which bears a definite relation to the depth of the water and the character of the sand, so that the depth may be determined.

Judging from the Millstone Grit near Sheffield, when the thickness of the bed (t) and the angle of the stratula (a) are abnormally small, $\frac{t}{\sin a} = \frac{t'}{\sin a'}$, and therefore in each case we may calculate the value t. Considering the independent evidence of great variation in depth, this yields reasonable conclusions, and assists in giving the true value of t.

In studying particular rocks, allowance must be made for the contraction which has occurred since deposition. What should be learned in each case is the smallest thickness of the beds when the angle of rest is just true. This may vary considerably in different parts of the same rock, since the depth has often been reduced by as much as 20 feet by one continuous drift, over a flat bottom of considerable area, leaving the water so much more shallow. In varying currents in shallow water, there is often much confusion and much that may be called false bedding, from which little can be learned by calculation.

Having then determined, in a more or less satisfactory manner, the normal thickness in one or more particular cases, the question remains, what was the actual depth of the water before and after deposition? Experiments are wanted on a fairly-large scale, with plenty of water under complete control, in order to ascertain the general facts. In the absence of these, it is necessary to fall back on what I learned in another manner. I found that sand of the average diameter of a hundredth of an inch is drifted along with a current of 1 foot per second, and, if it arrives where the depth is twice as deep, so that the velocity is reduced to 6 inches per second, it is thrown down and accumulated at the true angle of rest. Both these limits are doubtless subject to variation, according to circumstances, but they may probably be looked upon as a fair average from which to determine approximately the depth: the result being that when the sand is drifted the depth is t, and when deposited $2t$. However, since the bottom past the slope would be somewhat protected by the slope, the possible depth in the two places may be $2t$ and $3t$. These depths seem small, but, so far as is known, they are of the right order of magnitude. In any case $d+t$ must be decidedly greater than d, which means that d is not great compared with t, and may be small.

Application to Particular Rocks.

I made many observations in the Great Oolite near Bath. In one good case near Box were two drift-beds, each about 15 feet thick, drifted from nearly opposite quarters. These were separated by a bed full of borings showing little evidence of current. In other places were beds only 1 to 3 feet thick. My data are imperfect, since I did not determine the angle of rest; but they indicate a depth varying from a few feet up to perhaps 20 or 30 feet, or more in places where there is no evidence of depth.

I have records of very many measurements in the Millstone Grit near Sheffield, and the general conclusion seems to be that the water was of extremely-variable depth, and generally shallow. Thus at Bell Hag the true angle of rest, as altered by consolidation, is 25°, and the rock is now 70 per cent. of its original volume. With this angle are beds 25° and 5 inches thick, 20° and 36 inches thick, and 17° and 15 inches thick, which can be explained approximately by supposing that in one part or another, before and after deposition and filling up, the depth varied from 12 to 18 feet down to 1 or 2 feet, with a general average of 6 to 10 feet.

General Conclusion respecting Sandstones, etc.

The facts now described enable us to divide sandstones and analogous rocks in the following manner:—

1. Thinly- or thickly-bedded rock, without ripples or drift-bedding, and showing little or no graining of the surface in the line of the current. Good examples of this occur in the Old Red Sandstone of the Black Mountains, in the Llanthony valley. This could be explained by supposing that the water was at considerable depth, and the material mainly deposited from above, not drifted along the bottom, where the velocity of the current was much less than 6 inches per second.
2. Thinly-bedded rock, with well-marked graining of the surface in the line of the current, indicating a mean velocity varying up to about 6 inches per second, but showing few or no ripple-marks.
3. More or less thick masses of rock almost entirely made up of ripple-drift. This must have been when the velocity of the current was something like a foot per second, but varying with the character of the sand, which drifted it along the bottom accompanied by more or less rapid deposition from above; and, as the ripples advanced, more was deposited on the sheltered side than was washed up on the exposed side, so that the rate of deposition would be known if our knowledge of the advance of ripples were more complete. But,

so far as the facts are known, deposition at something like the rate of a quarter to half an inch per minute may be looked upon as a common average.

4. What I have called drift-bedding in numerous published papers is when the sand is drifted along the bottom to a point where the depth is so much greater and the velocity of the current so reduced that it is thrown down at the angle of rest. The velocity of the current is indicated by the nature of the sand; and probably further experiments would enable us to learn the approximate depth, which probably was small, since an increase of a very few feet made so great a difference in the strength of the current. Excellent examples of this structure are common in many rocks, and the direction and character of the current are sometimes found to have been very uniform over a wide area.

Some examples of what has been called 'false-bedding' are irregular accumulations from which no accurate conclusions can be deduced.

VIII. Joints of Encrinites, etc.

Each plate and spine of echinoderm and each joint of encrinite is, as it were, a single crystal of calcite, having a complicated external and internal organic structure. The minute, twisting, hollow, internal spaces of big spines are full of air in dry specimens, expelled and replaced by water on boiling, and must be full of sea-water when the animal is alive. It is no doubt by this that the carbonate of lime is introduced when these joints, etc. become almost or quite solid on fossilization. The structure of the test of all species that I have examined, and of the joints of the living *Pentacrinus* is practically the same, so far as the cavities are concerned. In the case of a big spine of *Echinus*, I found that the hollow spaces amounted to 51 per cent. of the volume, and the specific gravity when dry was 1·32, and when full of water 1·83; hence, the excess of weight over water is less than one half of that of a solid shell of the same bulk. These facts fully explain the very special characters of fossilized echinoderms. They do not decay, but are filled with infiltrated calcite in crystalline continuity with the original, so that the structure is similar to that of a single crystal with the usual cleavage. The specific gravity being so small and their form so very favourable, joints of encrinites would be washed along by a current which would not move fragments of more solid shells and corals of similar size, the specific gravity of which is from 2·7 to 2·8. We can thus easily understand why they so often occur almost or quite free from other material. The same general principles would, to some extent, apply to foraminifera and small univalves. Separate valves of bivalve shells, on the contrary, easily turn over and lie with their convex side upwards, so as to offer much resistance to a current, and may thus be sorted by being left alone.

IX. Very Fine-Grained Deposits.

The properties of extremely-minute particles of clay and chalk differ in some remarkable particulars from those of sand, as though a thin adherent film of water played a most important part, when they subside and afterwards become more or less consolidated. Sand subsides quickly, and almost at once attains a state of comparative stability. On the contrary, in the case of very finely-divided clay and chalk, although they may not take long to subside and leave the water almost clear, yet the accumulated deposit, after settling for a day, acts like an imperfect liquid, and contains no less than 86·5 per cent. of water in the case of clay, and 74·3 per cent. in that of chalk, so that the particles must be comparatively far from touching one another. With such fine-grained material an extremely-thin layer of water would suffice. Thus, for particles $\frac{1}{10,000}$ inch in diameter, a film $\frac{1}{25,000}$ inch thick would explain what occurs when no pressure is present, squeezed out thinner when under pressure. On keeping, the material slowly settled; but, even after a week, the amount of included water was still 79·8 per cent. in clay, and 68·1 per cent. in chalk. After no further contraction in volume occurred, the amount of included water was 75·5 per cent. in clay, and 64·6 per cent. in chalk. In one case, pipe-clay which had been kept for about a month until no further subsidence was visible, was kept for a whole year without further contraction, and was found still to contain about 75 per cent. of water. In the case of some fine-grained mud from a depth of 2500 fathoms, collected by the *Challenger*, after it had stood until no further subsidence took place, the amount of included water was no less than 89 per cent. This permanent state is reached most probably when the downward pressure of the particles is equal to the cohesion of the surface-film of water. Hence, we may conclude that, since this pressure would increase with the thickness of the deposit lying above, the amount of included water would decrease as the depth became greater, a conclusion which agrees well with observed facts.

When actually dried, after having subsided as much as they would, both clay and chalk gave evidence of considerable contraction, and the volume of included air was in clay 37·9 per cent. and in chalk 41·4 per cent. On water being again added without disturbing the material, this swelled up considerably, as though the water forced itself in energetically, and the final volume of water was in the clay increased to 62·9 per cent., and in the chalk to 57·2 per cent. It was extremely interesting to observe the difference in the two materials, for the clay easily broke up into laminæ in the plane of subsidence, but the chalk did not. The great contraction in thickness had developed a sort of imperfect cleavage in the clay, but not in the chalk—most likely because the clay contains many flat particles, and the chalk few.

The Deposition, etc. of Fine-Grained Material.

Possibly many may think that the deposition and consolidation of fine-grained mud must be a very simple matter, and the results of little interest. However, when carefully studied experimentally, it is soon found to be so complex a question, and the results dependent on so many variable conditions, that one might feel inclined to abandon the enquiry, were it not that so much of the history of our rocks appears to be written in this language.

The method employed in my experiments was to break up thoroughly in water some fine-grained yellow clay from my garden, due to the decomposition of a Coal-Measure shale, and, allowing the coarse matter to subside, to pour the finer into beakers of various sizes, with nearly perpendicular sides. After the contents of these had been well stirred up, the extent of the subsidence was carefully marked and measured at equal intervals, at first of a quarter of an hour, and later of one and two days.

The laws regulating the deposition and consolidation of clay in still water are very complex, and differ completely according to the relative amount of the solid material. When this is less than about 1 per cent., the particles remain separate, and appear to subside with a final velocity depending on their size and density. The result is that, in the earlier stages, the coarser grains collect at the bottom, and the supernatant water is not clear near the surface, but is increasingly muddy downwards. On further standing, finer and finer-grained mud reaches the bottom, and the water becomes less and less muddy. The upper part remains more or less turbid for a considerable time, since the final velocity of the finest grains is only about 1 inch per day. The result is that the subsided mud is moderately firm, but far from homogeneous, being possibly a sort of very fine sand at the bottom, and the finest possible clay at the top. This may be looked upon as a normal layer for one period of mud. On the contrary, if the amount of solid material is decidedly more than 1 per cent. of the volume, the grains collect together into small compound masses, which subside with a small velocity, quite unlike that of the larger or smaller constituent grains, forming an almost liquid mud, and leaving the supernatant water almost clear from the beginning. This mud slowly decreases in volume by forcing out the entangled water, but may remain in a semi-liquid state for a long time, which explains a number of interesting facts. The grains of varying size may thus be very little separated, and an almost homogeneous deposit formed, with a mere trace of division into layers; though, on final consolidation, it may show fissility analogous to imperfect slaty cleavage.

The following tables (IV & V, pp. 192 & 193) show the character and rate of deposition in the case of the less and the more muddy water during the first two hours and the first eight days. The intermediate state is best shown by an experiment in which the proportion of mud was larger, described later.

It will be seen from Table IV that, when the mud collects at the bottom as separate grains, it is at first in a somewhat loose condition and afterwards settles down; but, if the amount were considerable, it might remain for many weeks in a semi-fluid condition.

In an experiment in which the percentage-volume of the dry mud was 15½, semi-liquid mud continued to subside for about six weeks, and then, although soft, was not liquid. The water was removed and the whole left several months to dry, when it greatly contracted in volume; but, though looking quite solid, it was found by the oil-method (described later) to contain only two-thirds of its volume of solid material, the rest being invisible cavities. Table V. (p. 193) shows the amount of subsidence in hundredths of an inch, smoothed down, for every two days until the volume became permanent, and in the lower part the constant volume when wet and when dry.

TABLE IV.

Depth of water 2·30 inches.		2·90 inches.	
Hours.	*Mud at bottom.*	*Clear water.*	*Liquid mud.*
¼	·12	·05	2·85
½	·23	·14	2·76
¾	·35	·20	2·70
1	·40	·30	2·60
1¼	·35	·50	2·40
1½	·30	·60	2·30
1¾	·28	·70	2·20
2	·27	·80	2·10
Days.		*Days.*	
1	·20	1	1·33
2	·18	2	1·08
3	·16	3	1·02
4	·15	4	·99
5	·14	5	·94
6	·13	6	·90
7	·12	7	·88
8	·12	8	·87
Dry	·03	Dry	·16
Solid	·02	Solid	·11

As shown in Table V, the successive differences prove that the predominant influences vary greatly as subsidence goes on. During the last few weeks the first differences are nearly equal, as though the rate of subsidence were nearly uniform; whereas during the first few days, it is not until we arrive at the fourth order of differences that they become nearly equal. Towards the end, when no further subsidence occurs, although there was five times as much water as solid material, it is as though the gravity of the minute particles were just balanced by the cohesion of a film of water. For about four weeks before this, the rate of subsidence varied nearly as the amount of space through which the rising water had to escape upwards, but in the first few days the rate approaches the fourth power of the time, as in the case of water passing through small pipes. It will thus be seen that we have to deal with a very complex subject.

TABLE V.

Days.	*Depth of clay.*	*Differences.*			
0	186				
		40			
2	146		14		
		26		4	
4	120		10		1
		16		3	
6	104		7		0
		9		3	
8	95		4		1
		5		2	
10	90		2		0
		3		2	
12	87		0		
		3			
14	84		1		
		2			
16	82		0		
		2			
18	80		0		
		2			
20	78				
		2			
22	76				
		2			
24	74				
		2			
26	72				
		2			
28	70				
		2			
30	68				
		2			
32	66				
		2			
34	64				
		1			
36	63				
		1			
38	62				
		1			
40	61				
		1			
42	60				
		0			
44	60				
		0			
46	60				
Dry	15				
Solid	10				

To investigate fully what occurs under natural conditions would be a difficult undertaking, because some of the most important facts could be learned only in rough weather when it would be impossible to collect material. The result is that some of the most striking peculiarities of many of our rocks cannot be satisfactorily explained experimentally, or by appeal to what now takes place.

When dry hydrous pipe-clay was pressed together by hand it contained 43·4 per cent. of interspaces. On applying more pressure they amounted to 24·4 per cent., or closely as in the case of spheres arranged to occupy the least volume; but on further pressure they were reduced to 21·4. These results were very different in the case of ignited pipe-clay, for even after great pressure the interspaces were 48 per cent. Hence it seems clear that when the clay is hydrous, the grains are sufficiently soft and mobile to yield and partly fill up the interspaces, which is an important fact to bear in mind in studying the structure of many rocks.

The change produced by varying amounts of water in clay is of much interest. When only 40 per cent. of the volume of water is present, hydrous pipe-clay breaks easily and is not plastic. With 45 per cent. it is just plastic and soft, and completely plastic with 50 per cent. It thus appears that complete plasticity depends on the presence of rather more water than is sufficient to fill the interspaces. When less than this, its action is probably that of suction, but when more, it acts as a lubricant. This must also be important in connexion with the consolidation of many natural rocks.

The Effects of Currents.

Although the laws of the deposition of clays in still water had first to be learned, yet it seems quite certain that in many cases the results must have been greatly modified by gentle currents. Suitable experiments would require a considerable stream of clear water and special arrangements. However, judging from the rate of subsidence, the velocity of current necessary to produce decided effects must vary much according to the nature of the mud, and must be of some such order of magnitude as from 1 inch to 1 foot per hour for very fine mud, and 1 foot per minute for coarser.

It is easy to understand how irregularities in velocity can be produced. I have paid a good deal of attention to the movements of currents in tidal estuaries. The velocity varies much vertically, and although there is often a strong current in mid-stream, there is none (or else an eddy) towards the shore. Moreover, vortices are often formed with horizontal axes more or less perpendicular to the direction of the current, which on one side may bring up mud and carry it down the other. The result of all such actions would be to produce more or less variations in the suspended mud; and, when the movement of the water becomes sufficiently slow, layers of different texture may be deposited. It would be extremely difficult to verify this by collecting mud from deep water and studying its microscopical structure; at present, therefore, it is necessary to make inferences from experiments. At the same time, the subject is so complex that unknown conditions may vitiate some of the conclusions.

So far as I can judge, a gentle current of varying velocity in muddy water would explain the structure of many rocks which have alternating layers of different character, some very fine-grained and others more sandy, the thickness of merely an inch having a complex history of deposition and microscopical denudation. However, at all events in the older rocks, cases are fairly common which seem to require much more rapid slight alterations in the current than seem likely to have affected the general mass of water. Since the bottom of a current is retarded by friction and moves more slowly than that higher up, it seems probable that eddies are formed causing the water at the bottom to move with sufficiently-varying velocity to modify the deposition, and to give rise to very thin laminæ. I am sorry that I have not been able to verify this experimentally; but the structure of the ripple-drift in the green slates of Langdale, described later (Pl. XV), seems to prove the existence of such pulsations, whatever may be the true cause. In the ripple-drift on the side where the material was thrown down on a slope, in what represents the 'ripple-period,' may be counted eighty layers of slightly-varying material; and, since the most probable length of this period is half a minute, they would indicate about 160 pulsations in a minute. Similar thin layers are seen in a part where the current was not sufficiently strong to produce ripples, but they are thicker, as though the pulsations were slower. Since the calculated period depends on so many factors, all subject to error, it would perhaps be best to assume that when the current is too slow to produce ripples or drift-sand, the pulsations were, roughly speaking, of about the magnitude of one per second, which would indicate that the thickness of a single layer was deposited in that time. This must be looked upon as only an approximation; but, at all events, it is an interesting and plausible conclusion, and agrees fairly well with the probable period of the pulsations which must accompany the bottom-ripples.

Application to Particular Rocks.

Although I possess a fairly-large collection of thin sections of slate-rocks, yet the variations due to chemical and mechanical changes are so numerous that I have but few throwing light on this particular question. I have, however, some excellent examples. In one from the Skiddaw Slate of Portinscale there are darker and paler layers of different-sized grain, evidently due to water depositing different material; but these layers are divided into more or less well-marked laminæ, varying from ·002 to ·010 inch in thickness, and therefore, perhaps, indicating that the rate of deposition varied from about 7 inches to 3 feet per hour. A second specimen from another locality indicates a rate of from 9 inches to 3 feet per hour. Different specimens of the slate-rocks near Moffat indicate deposition of from 12 to 18 inches per hour. Various specimens from near Bangor indicate from 9 to 18 inches per hour. On the whole, my specimens give from 7 inches to 3 feet, but the usual rate is from 9 to 18 inches per hour. These may seem quick rates, but of course they refer to periods of decided current bringing deposit, which may have been separated by long intervals without deposition, as in the case of the green slates described later.

I have made thin sections of fine-grained rocks which break up at once when wetted, by hardening them with Canada balsam; but, until quite recently, I never suspected that anything of special interest could be learned from sections of soft clays, and it is now quite out of my power to collect and prepare such material. Judging from my experiments, much could be learned respecting the conditions under which such rocks were formed. It would be a wide branch of study, and would necessitate microscopical work on a large scale. The examination of the rocks in a natural condition is enough to show that the structure of clays differs enormously, and indicates formation under very different conditions; but there is always some doubt as to their true structure, when not made into thin sections.

A puzzling question is the origin of thick beds of almost homogeneous fine grain, like the slates of Penrhyn and Llanberis, since they may have been formed in two different ways, either by a gentle and uniform current, continuously drifting very uniform material, or by the deposit of cohered mud from very tranquil water. It is, however, possible that the above-named slates may originally have had a thin laminar structure, which was obliterated when cleavage was developed. So far as can be judged from the rocks in a natural condition, a homogeneous structure is common in our later deposits, and seems to indicate drifting by a very gentle current to spots where there was scarcely any at all. As examples, I may name deposits in old lakes, much of the Gault and Speeton Clay, and some Coal-Measure shales. The structure of much of the Boulder-Clay must be explained in a different manner. The Gault of Aylesford, the Kimmeridge Clay near Filey, and the Lias near Whitby, show laminar structure due to currents.

We thus see that the history of fine-grained rock is written in a well-defined special language, still imperfectly understood, for want of adequate experimental study, both in the field and microscopically.

X. THE GREEN SLATES OF LANGDALE. (Pls. XIV–XVI.)

Probably no better example could be found of the effects of currents acting for a short time, than specimens that I procured from the quarries in Langdale. These structures would probably have been overlooked, if the slates had not been wetted by a shower of rain; but, in order to see them to perfection, the surface of the slate should be ground flat and smooth, and coated over with a thin layer of so-called 'negative' varnish.

In my specimens the plane of cleavage on which the structures are seen is inclined at about 45° to the stratification, and in calculating out the results allowance has been made for this, and also for the change of dimensions when the cleavage was developed. The existence of this constitutes, however, a great advantage, since the rock has been compressed so as to be very nearly solid, the cavities amounting to only 0·5 per cent.; whereas in analogous rocks from the Coal-Measures they amount to over 13 per cent. Moreover, the character of the rock is eminently favourable for exhibiting the structure, since the fine-grained material is a pale green, and the coarser a dark green. The microscopical structure shows clearly that practically the whole material is a volcanic ash; and the structure in many cases is as though this had been deposited from above, with little or no drifting along the bottom or sorting by a current. In the following account of my specimens, I adopt for calculation, etc. the general conclusions already explained. Pl. XIV is a reproduction of a photograph of a case where the current was so gentle that only very fine-grained green material was deposited in just the same creamy semi-liquid condition as recently-deposited clay in which the amount of included water is about 80 per cent., so that it can be easily washed up by a gentle current. Then must have come a fresh volcanic disturbance and deposit of ashes, with a current moving from left to right, which broke up this semi-liquid material into what might be compared with breaking waves, some of which were permanently entangled in the ash, and others carried away. This not only shows the original character of the deposits, but also roughly the time that elapsed between the disturbances. Very fine-grained material does not remain in this semi-fluid condition for more than a few weeks; and therefore we have permanent evidence that, in some cases, the volcanic disturbances were separated by only a short interval. Other specimens indicate much longer periods, the breaking-up in similar cases being comparatively small.

The next illustration (Pl. XV) is of a case where the current set in and soon increased to probably about 9 inches per second, being able to develop ripples, yet not strong enough to drift along any but the finer material. The rate of deposition can be learned from the central portion when the current was at its maximum. The ripples were about $3\frac{1}{2}$ inches long, of normal height, and the thickness of the tails of the drift, corrected for bedding and cleavage, is ·86 inch. Then, in accordance with what I have explained in connexion with ripple-drift, the period of the ripples would be $3{\cdot}0 \times \frac{2{\cdot}52}{2{\cdot}52 \times 7 \times {\cdot}86} = {\cdot}49$ minute, in which time ·86 inch was deposited. This is equal to about $1\frac{3}{4}$ inches per minute of the rock in its present state, or to about 2¾ inches when deposited; but, since the current was probably less than 12 inches per second, the rate of deposition may be looked upon as about 2 inches per minute. This may seem rather a large amount, but the facts appear to justify the conclusion, which is not unreasonable in the case of a volcanic eruption. The current would seem to have set in somewhat suddenly, and to have continued for about 2 minutes, after which fine semi-liquid mud was deposited, somewhat broken up by a fresh eruption of ashes, which, judging from my experiments, probably took place in the course of a few weeks or months.

Another specimen is very different, and shows that the current suddenly increased from under 6 inches per second to 12 inches for a doubtful period, but probably for about 2 minutes, during which deposition took place at the rate of only ·10 inch per minute.

The third illustration (Pl. XVI) is of great interest. In the lower part is level bedded deposit, indicating a current too small to develop ripples. This gradually increased until it was strong enough to wash along the sandy ashes, and produce a thin bed of true ripple-drift, deposition taking place at the rate of ·4 inch per minute. The current then gradually decreased, and could no longer drift along the coarser part of the material, and the fine-grained semi-liquid clay was deposited, and was partly in this state when a fresh disturbance brought coarser ashes and broke up and entangled some of the creamy material.

The maximum angle at which stratula dip is of much interest, as indicating the character of the material when it was deposited. Making all the necessary corrections, and allowing for the compression from the condition of a newly-deposited rock to one almost solid, I find that the angle of rest of the material must have been closely the same as that for a fine-grained volcanic sand. Hence, although the slate is now of almost exactly the same hardness throughout, we have good physical evidence that one part was originally fine loose sand, and the other a semi-liquid clay.

All these results must be looked upon as only approximate, but they are as likely to err on one side of the truth as on the other. As will be seen, the rate of deposition varied greatly, but all my specimens agree in showing that currents set in more or less suddenly, and, after continuing for a few minutes, died out. It seems to me, therefore, much less likely that they were currents of the kind so common during the deposition of many rocks, than due to volcanic disturbances accompanying the throwing-out of the ashes of which the rock is composed. It is possible that further study in the quarry would show cases in which the current acted for a longer period. I describe only some of the specimens that I collected and prepared in a suitable manner. At all events, there seems, to be here good evidence of the rate of deposition, and indication of the intervals between different volcanic disturbances.

Ripple-Drift in other Rocks.

The detailed study of ripple-drift in other rocks is far more difficult than in the Langdale Slates, because the colour of the material does not vary so much, and my collected specimens do not enable me to give such precise results. One, however, which is fairly characteristic of Coal-Measure sandstones, indicates a current of about 1 foot per second, and deposition at the rate of about two-thirds of an inch per minute, as the rock now is, or 1 inch per minute in its original condition. Beds of varying ripple-drift of considerable thickness are common, and they differ widely from the Langdale Slates just described. The careful study of the rocks *in situ* would probably yield interesting results, and furnish information respecting the exact nature of the currents. There certainly appears good evidence to show that deposition up to 1¼ inches per minute was common, but associated with possibly long intervals during which there was little deposition. It must also be borne in mind that there may have been currents of over 18 inches per second, which would wash up the sand and leave little or no evidence of their existence, if no coarse material existed in the district.

I possess a specimen from the Lower Coal-Measures at Ringinglow near Sheffield, which, like the Langdale Slates, shows a current of about 1 foot per second, acting for a short time, so as to produce ripples, and not merely graining of the surface (like in the rest of the rock), as if due to currents varying frequently up to somewhat less than 6 inches per second.

XI. Washing-up, etc. of Clays.

The velocity of current needed to just wash up very fine-grained deposits must necessarily depend so much on the length of time that has elapsed since they were deposited and their state of consolidation, as well as on the effect of the associated small animals and plants, that in many cases all calculation is impossible. Judging from my experiments, recently-deposited fine clay, unmodified by minute organisms, would be washed up by a very gentle current, since its density differs so little from that of water; and we may safely conclude that it would not be permanently accumulated, except in more or less still water. On the contrary, I have dredged up fine-grained mud, made so tenacious by *Jassa pulchella* and other small organisms, that it was almost impossible to wash it out of the dredge. Banks of such mud would resist a much stronger current than one that would wash away sand, so that calculation from the size of the grains is out of the question. However, it is more important to consider the velocity of current that would allow of deposition; but, even then, very much would depend on the quantity held in suspension and on the extent to which the particles collected into compound groups. Assuming that no deposit would be formed when the current was stronger than the final velocity, it seems probable that for granules ·001 inch in diameter the velocity of the current could not be above ·01 foot per second, equal to about 36 feet per hour, and for those ·0001 inch in diameter about $3\frac{1}{2}$ feet per hour or less. At all events, for fine-grained clays these velocities are probably of the true order of magnitude, though possibly too great.

XII. ON THE INTERSPACES BETWEEN THE CONSTITUENT GRAINS OF DEPOSITED MATERIAL.

A knowledge of the relative volume between the constituent grains of rocks, as originally deposited, or as modified by subsequent mechanical or chemical changes, throws much light on many interesting questions. In studying this subject the foundation is to a large extent mathematical; and, in order to facilitate calculation, it was desirable to assume that the grains are spheres of equal size, uniformly arranged in various ways, so as to occupy as much or as little space as possible or some intermediate amount. Possibly this problem has never before been treated from its geological side. For the sake of simplicity, I have made my calculations as though there were only eight spheres, but so treated the question that the results would be the same as if the numbers were so great that the effects of the outer surfaces could be neglected.

(1) The first case to consider is when four spheres are arranged as a square, and the other four placed directly over them, so that each sphere rests upon only one, and the bounding surface of the whole is a cube. The radius of each sphere is taken as unity, and therefore the length of each side of this cube is 4, and the volume 64. The united volume of the spheres themselves is then $\frac{4}{3}\pi \times 8 = 33{\cdot}51$. Hence their relative volume is $\frac{33{\cdot}51}{64} = 52{\cdot}36$ per cent., and of the interspaces 47·64 per cent.

(2) The next case is when four spheres are arranged as a square, and the other four tilted over, so that each rests upon two, and the bounding surface is a parallelepiped, four sides of which are squares and the others parallelograms having angles of 60° and 120°, so that the height above the square base is $2\sqrt{3}$, and the volume $2\sqrt{3} \times 4 \times 4 = 55{\cdot}42$, whereas that of the spheres alone is 33·51. Hence the relative volume is $\frac{33{\cdot}51}{55{\cdot}42} = 60{\cdot}46$ per cent., and the relative volume of the interspaces is 39·54 per cent.

(3) The third case is when four spheres are arranged as a square, and the other four tilted over in the line of one diagonal, so that, if the number were indefinitely great, each would rest upon four. This would give a parallelepiped having a square base, and two edges inclined at 45° to the base, so that the height would be $2\sqrt{2}$, and the volume $2\sqrt{2} \times 4 \times 4 = 45{\cdot}25$. Hence the relative volume would be $\frac{33{\cdot}51}{45{\cdot}25} = 74{\cdot}05$ per cent., and that of the interspaces 25·95 per cent.

(4) The last case that we need consider is when the base itself is not square, but the spheres so shifted that each one touches two, and the base is a parallelogram, having angles of 60° and 120°; and the other spheres are arranged as much as possible in the same manner. It is, however, impossible to have each one resting upon three when the number is indefinitely large, but there are alternate rows with 1 on 3 and 3 on 3 cross ways. We thus get for the axes of the bounding parallelepiped $2\sqrt{4-\sec^2 30^\circ}$, $2\sqrt{3}$, and 4: so that the volume is $2\sqrt{4-\sec^2 30^\circ} \times 2\sqrt{3} \times 4 = 45{\cdot}25$, or exactly the same as in the last case considered, and the relative amount of interspaces 25·95 per cent. This is a very interesting result, since it shows that, when occupying the least volume, the spheres could be moved about considerably, without altering the volume. I may say that, in order to test my calculations, I made very careful measured drawings, and obtained practically the same results.

Experiments with Spherical Shot.

My experiments were made in a glass bulb holding a known weight of water, and the amount of interspaces was ascertained from the weight of water between the grains, when full of water; and, in other cases, from the weight of the material used, compared with that of an equal volume, if it had been solid lead. When the small shot was filled into the bulb without shaking, the volume of the interspaces was 47·2 per cent., which thus agrees closely with 47·64 per cent., calculated for spheres occupying as much space as they can when each touches six. When the glass bulb was turned about and well shaken, so as to cause the shot to occupy as small a space as it would, the interspaces were reduced to 40 per cent. This agrees closely with 39·54, found by calculation in case No. 2, for spheres arranged rectangularly in two directions, but one over two in another. It is scarcely probable that such an arrangement is brought about by shaking, but the above agreement is remarkable.

I then hammered the same shot, thus obtaining disks with a diameter about three times their thickness. On filling these gently into the bulb and afterwards well shaking them, the amount of interspaces was found to be practically the same in both cases as if they had been spheres. This is somewhat remarkable, but of much interest in connexion with sand built up of grains of irregular shape; for it shows that, if these are of fairly-uniform size, in the long run they occupy nearly the same volume as if they were spheres.

Experiments with Quartz-Sand.

As might be expected, the results differ materially with sand of different character. In the case of the somewhat coarse and angular sand of the Millstone Grit, having grains on an average ·05 inch in diameter, when filled in variously, but not shaken, the average volume of the interspaces was 49·4 per cent., and when well shaken 43·5 per cent. In the case of Calais sand, the grains of which had a mean diameter of about ·01 inch, the interspaces as above were respectively 45·8 and 38·7 per cent. The means of all my experiments with both kinds of sand were, for not shaken 47·0 per cent., and for well shaken 40·0, which agree remarkably well with 47·2 and 40·0 found in the case of shot. In no case with sand of fairly-uniform grains did shaking reduce the volume of the interspaces to anything like the theoretical minimum, namely 25·95 per cent. In the case of finely-powdered sand, having grains on an average $\frac{1}{300}$ inch in diameter, when filled in quickly the interspaces were 47 per cent., and when well shaken 34: this much smaller amount being probably due to the greater relative range in the size of the grains.

Sand with Grains of Extremely Variable Size.

Theoretically, the admixture of fine sand with coarse ought greatly to reduce the amount of the interspaces. For example, if the spaces between the coarse grains were filled with fine sand, the interspaces should be 40 per cent. of 40 per cent.: that is, only 16 per cent.; but in no case have I obtained so low a result by experiment. Thus, on mixing the coarse Millstone-Grit sand with an equal volume of Calais sand, I found the interspaces, when filled in quickly, to amount to 38 per cent., and when well shaken, 32 per cent. When equal quantities of these two sands and of the powdered sand were mixed, the interspaces when shaken were further reduced to 28·9 per cent. This, however, is far greater than it would be theoretically, if arranged with intelligent design; it might probably be the result, if the sands were exposed to pressure and vibration for an indefinitely-long period. The facts, nevertheless, clearly show that, as might have been expected, the proportion of interspaces in such sand as would be deposited from deep water, and not sorted by bottom-currents, would be much less than in well-washed sand. Hence, other things being equal, the amount of interspaces gives some indication of the relative depth of the water, which agrees well with many general facts. Calais sand mixed with half its weight of pipe-clay, when gently compressed contained 36·7 per cent. of interspaces, which was reduced to about 18 per cent. by a pressure roughly estimated at about equal to 60,000 feet of superimposed rock. In the case of clay alone it was only 14·6 per cent. We thus see that the effect of clay is great.

Small Flakes of Mica.

When fine particles of mica were put into a brass tube, the amount of interspaces was about 74 per cent., and by moderate pressure this was reduced to 55 per cent. When the mica was well shaken before pressure the amount was about 57 per cent., and when afterwards compressed it was reduced to 52. These facts show clearly how very far small flakes are from arranging themselves in the smallest space, since, even when somewhat compressed, they occupy more than the maximum for spheres arranged rectangularly.

XIII. SEGREGATION.

Before further considering the cavities in sedimentary rocks, it is desirable to point out the difference between two extreme forms of alteration subsequent to deposition. One, which may be called segregation from the outside, is when the cavities originally existing between the ultimate particles have been more or less completely filled, usually by carbonate of lime, introduced from without from contiguous water or deposits, the original grains being unchanged. The other may be called internal segregation, and is when original material migrates from one place to another, leaving empty spaces, and making other parts more or less completely solid. As examples of the former, I may cite some calcareous sandstones, and a case from St. Helena, where the original rounded grains of corallines and shells are quite unchanged, but the spaces between them filled with crystalline calcite; and as an example of the latter, a limestone from Binstead (Isle of Wight), where the original fragments of shell have been completely removed, leaving empty cavities, and the carbonate of lime has been transferred to the intermediate spaces. A combination of both these changes is seen in many limestones.

Coral-Reef Limestones.

An analogous transfer from one part to another may result in irregular cavities, and in a far more solid intermediate material than the original deposit, conditions which are very characteristic of those coral-reef rocks that I have been able to examine. Through the kindness of the Trustees of the British Museum and Dr. A. Smith Woodward, I have been able to study carefully four specimens from various depths in the boring through the reef at Funafuti. In all of these there are empty spaces, in no way like casts of decayed bodies, but of the most irregular shape: when small, some may represent bubbles of gas, and when larger they are of such irregular shape as to defy description. Their surface is generally covered with small rounded protuberances. I cannot explain the facts better than by internal segregation, causing the material to collect into certain parts, so as to make them nearly solid, and to leave other parts void, for lack of matter to make all solid.

These cavities are so unevenly distributed as to make it useless to determine their amount from a small piece, but Dr. W. M. Hicks kindly ascertained the specific gravity of the whole of each specimen with the large balances at the Sheffield University, from which I could calculate the amount of empty cavities completely enclosed in the solid rock. Then, by careful measurement and calculation, I was able to determine approximately the volume of the cavities open to the outside. Combining these volumes with those calculated from the specific gravity I obtained the following results:—

TABLE VI.

From	150 to 160 feet deep	18 per cent.
,,	643 to 646 ,, ,,	32 ,, ,,
,,	755 to 766 ,, ,,	20 ,, ,,
,,	1080 to 1090 ,, ,,	29 ,, ,,
	Mean = about	25

It will thus be seen that, from the four specimens studied, no law can be deduced connecting the empty spaces with the depth; but it might be apparent, if all the specimens were studied in the same manner. The mean value is, at all events, of the same order of magnitude as would occur in deposits that were consolidated by internal segregation without being first subjected to any considerable pressure, and it affords no certain evidence of material segregation from the outside. The hardness and structure completely prove the great extent of the internal segregation.

The limestones from Bermuda and Bahama contain analogous cavities; but their total volume seems much smaller, and some look as if they were spaces originally occupied by bubbles. At all events, both these and the Funafuti specimens differ in a marked manner from nearly, if not quite, all the 500 thin sections of British limestone in my collection.

Determination of the Amount of Interspaces in Natural Rocks.

In studying the interspaces in rocks, two courses are open to us. We may determine their volume in the rock as it now exists, which may or may not have any connexion with its original condition; or, by studying a thin microscopical section, we may ascertain their relative volume in the early condition of the rock, before it was materially changed by subsequent deposition or solution of material. The former is much the most important when looking upon the rock as a building-stone, but the latter, when we wish to learn the history of its deposition.

To enter into full particulars would be almost equivalent to writing a treatise on the microscopical structure of rocks; and I shall therefore take it for granted that, in the cases here considered, there is no practical difficulty in distinguishing between the materials originally deposited and those subsequently introduced by infiltration. Having then selected a suitable portion of a suitable specimen, a photograph or a careful *camera-lucida* drawing is made on thin cardboard of uniform thickness, showing the original constituent fragments and the interspaces. These are then accurately cut out with scissors and their weight determined separately, from which the percentage of the interspaces can be at once calculated. The volume of the very minute interspaces existing in the grains themselves cannot thus be determined, but is not needed for the purpose in hand. The method which I have adopted in determining the interspaces in the whole rock as it now exists, is to boil thoroughly a portion in water in a flask, and tightly cork it when full of hot steam, so that on cooling there is a partial vacuum. After remaining for a few days the fragment is taken out, the loose water removed, and the weight of the fragment full of water determined. If thought necessary, the process may be repeated, so as to make sure that there is no increase in weight. The fragment is then dried until its weight is constant. The specific gravity of the rock being known, the volume of the open spaces can then be easily calculated. Those completely closed may be neglected for the purpose in hand, though in all cases they may cause the determinations to be somewhat too small. There are cases in which these two methods give nearly the same result, but others in which they are absolutely different, on account of changes since deposition.

In order to clear the way for subsequent descriptions, it may be well to state a few general conclusions applicable to the case when the grains may be treated as if they were spheres. Deposited quickly and shaken very slightly, the interspaces may amount to nearly half the volume, but the grains are then in a state of unstable equilibrium. On shaking, a sort of equilibrium is established, when the tendency to settle into smaller volume and the resisting friction are nearly equal, in which case the interspaces amount to about 40 per cent. of the volume. Another much more stable equilibrium is when the grains, probably under greater pressure, have arranged themselves so as to occupy the smallest space without their shape being altered, namely, where the interspaces amount to about 26 per cent. Another final condition occurs rarely and partially in limestones that have been subjected to the intense pressure which produced slaty cleavage, when the original interspaces have been almost or entirely obliterated. It will thus be seen that the detailed study of sandstones and some allied rocks is a very special, wide, and complex subject, which is likely to lead to a new class of results.

Application to Particular Rocks.

For some of the purposes of this paper limestones are by far the most suitable. Thin sections of coarse-grained sandstones are difficult to prepare, and teach so little that I have made very few. Considering how many thin sections of limestone I have made, very few are suitable for calculations, because the three axes of the original fragments are not sufficiently equal to enable us to treat them as spheres. In some cases, where the original deposit consisted of fragments of bivalve shells, more than one-half of the present solid rock must have been a subsequent chemical deposit from solution, brought in by percolating water. It is manifest that calculations in such cases are of little value. Deposits almost entirely composed of oolitic grains of nearly equal size are very suitable, and so are those composed of small joints of encrinites or fragments of shells or corals worn into fairly equi-axed grains of nearly equal size. Cases totally unfit for calculations are common, in which the grains are of very unequal size, and the spaces between the larger filled by smaller, as though not sorted by a bottom-current.

Sprudelstein, Carlsbad.—The amount of spaces between the almost spherical grains, as determined by the *camera-lucida* method, was found to be 44·3 per cent., which corresponds to what happens when deposited spheres are only slightly shaken. They must have been soon filled with infiltrated material in such a manner that all further settling was impossible.

Recent deposit, St. Helena.—What was given to me as such many years ago, is composed almost entirely of rounded grains of calcareous algæ, nearly all of one size. The amount of interspaces ascertained by the camera is 39·6 per cent. This corresponds very closely with 40 per cent. in the case of shot or sand well shaken. They are almost completely filled by infiltrated calcite, and probably this took place at an early period in the history of the rock, and its structure was thus made permanent. This specimen is extremely interesting, because the percentage of interspaces corresponds so closely with that observed in many older rocks, down to the Silurian.

Oolitic rocks.—It appears to me that a good deal remains to be learned respecting the exact conditions under which our Oolitic rocks were formed, since they differ so much from any deposits associated with recent coral-reefs that I have been able to examine, in which I have found only a very few oolitic grains of a kind rarely seen in British rocks. These generally appear to have been formed originally of calcite, a few of aragonite, and probably in certain districts of a mixture of the two. Over a considerable area in the Great Oolite, what was the original deposit is now a mere wreck. Properly to explain all these variations would require further researches of various kinds. For my present purpose, some of these rocks are of especial interest—because the oolitic grains can very fairly be looked upon as small spheres. In the first place, I will consider cases in which there has been little change since deposition.

Oolites of the Lincolnshire district, etc.—I have two excellent microscopical sections of a specimen given to me, and said to come from near Grantham (see Pl. XVII, fig. 1). The nearly spherical grains are quite hard, and the interspaces almost entirely empty, containing only a few small crystals of calcite. The volume of these interspaces, as determined by the *camera-lucida* method, was found to be 25·3 per cent., which, allowing for slight compression, corresponds remarkably well with 25·95, the theoretical minimum for spheres. It should appear therefore, that, although mere shaking will not cause grains of shot to arrange themselves in the least volume, pressure (and perhaps earthquake-shocks) acting through geological periods will produce this result; but of course not, if the interspaces had been filled by infiltrated calcite at an early part of the history of the rock.

I have examined some analogous rocks from the same part of England by the boiling-water method. The building-stone of Ketton was found to contain 29·7 per cent. of empty spaces, and another specimen from near Stamford 36·3 per cent. This latter was extremely soft when obtained in the quarry, and the grains themselves may not be solid. The excess over 26 per cent. may, therefore, not be due to the grains not having accommodated themselves to the least volume, but to the different manner in which the interspaces were determined, those in the grains themselves having no influence in the case of the Grantham specimen.

Portland oolite.—Determined by the *camera-lucida* method, the Portland building-stone was found to contain 23·6 per cent. of interspaces, only to a small extent filled with infiltrated calcite: but it can be seen that the grains have been to a slight extent pressed together, so as to explain why the interspaces are less than 26 per cent. When determined by the boiling-water method, those now empty were found to amount to 22 per cent., which smaller amount as thus determined is no doubt due to infiltrated calcite. It is, however, only particular portions of the Portland rocks that are at all suitable for the purpose in hand; for, in some, the oolitic grains have been changed since deposition in an unusual and remarkable manner, especially when near the Dirt-Bed.

Oolitic beds in the Carboniferous Limestone of Clifton, near Bristol.—In one excellent case the original interspaces must have amounted to 40 per cent., but these are now filled by infiltrated calcite. This agrees with the supposition that this infiltration occurred before the deposit was subjected to much pressure, though in another specimen belonging to my friend Mr. T. S. Cole, and photographed by him, the interspaces had been reduced to 32·6 per cent., or even less.

Oolitic bed in the Wenlock Limestone, near West Malvern.—Determined by the *camera-lucida* method, the original interspaces were found to have been 39·6 per cent.; and thus, like the above-described Carboniferous Limestone, they must have been filled by infiltrated calcite at an early period in the history of the rock. The mean of the two is 40·2, which is practically identical with what occurs in the case of shaken shot and sand, and in the recent rock from St. Helena. This seems to me a very interesting conclusion, since it shows that right down to the Silurian Period the infiltrated calcite was introduced while the rock was still what would now be called 'recent.' In other cases it must have been introduced when the rock was much older, or only partially up to the present time.

Modified oolites.—The freestones of Corsham (near Bath), Minchinhampton, and Cheltenham differ remarkably from those near Grantham and Ketton, inasmuch as the original oolitic grains are now mere residues, and the present solidity of the rock is due to the interspaces having been filled by infiltrated calcite. By the *camera-lucida* method, I found that in the Cheltenham rocks the interspaces originally amounted to 46 per cent., which does not materially differ from what is found in the Sprudelstein of Carlsbad (see p. 206). It seems to indicate a somewhat rapid deposition, and little disturbance before the grains were permanently fixed by the infiltration of calcite. As I pointed out in my address to the Geological Society in 1879,[1] there is good reason to believe that the grains in a few of our oolitic rocks were originally deposited as aragonite, but others as calcite; and that the former have since been changed, though not the latter (except in the case of the Portland rocks, as described above). It seems quite possible that some may originally have been a mixture of these two minerals, and that, in the same manner as in the case of some fossil-shells, the aragonite-portion has been removed and the calcite-portion left. At all events, this would explain why some parts of the oolitic grains have been removed, so as to give rise to their partly-decayed character. Much more experiment is needed to ascertain the exact conditions limiting the production of aragonite and calcite. Temperature certainly, but evidently other things also have great influence. So far, I have found very few oolitic grains in recent coral-reef rocks, and these seem to be aragonite. The conditions under which our characteristic oolitic rocks were formed appear to me to have involved shallow water, perhaps rather warm, and highly charged with carbonate of lime, which not only gave rise to the oolitic grains, but also crystallized out between them very soon after they were deposited. One specimen of the building-stone of Corsham gave, by the boiling-water method, 25·6 per cent. as the amount of cavities now existing; but the microscopical structure shows that this must have little to do with the original condition, although it may have some connexion with the amount of cavities when the oolitic grains were partly decomposed. I very much regret that I did not measure the angle of rest of the material, since a knowledge of the change in its value would probably have furnished valuable information. Judging from the facts at my disposal, the above-

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[1] Quart. Journ. Geol. Sol. vol. xxxv (1879) Proc. p. 84.

described differences in the oolitic grains vary in different beds and in different localities, but seem to extend over a considerable area.

Limestones composed of fragments of calcareous organisms.—When made up of joints of encrinites or of fragments of shells or corals so nearly equi-axed that they may be treated as imperfect spheres, their study leads to much the same results as in the case of oolites. All my best specimens of Oolitic age belong to the Coralline Oolite. That shown in Pl. XVII, fig. 2 was found to have originally contained 43·3 per cent. of interspaces, and another from Oxford 43·1. Yet another from Filey contained 53·1 per cent., this larger amount being probably due to the fragments having too flat a shape to be strictly comparable with spheres. A Devonian limestone from Hope's Nose (near Torquay), composed of small joints of encrinites, gave 45·6 per cent. Leaving out the specimens from Filey, the other three give as a mean 44 per cent., or almost exactly the same as the Sprudelstein of Carlsbad. Everything therefore agrees with the supposition that the material was deposited rather quickly, and not much shaken or pressed before the interspaces were filled with infiltrated calcite. A specimen of the lower part of the Carboniferous Limestone near Bristol, mainly composed of fragments of encrinites, seems to have contained only 25 per cent. of interspaces, as though they were not filled up with calcite until after the material had been exposed to considerable pressure; and a specimen from the Wenlock Limestone of Easthope (Pl. XVIII, fig. 1) contained only 17·7 per cent., which seems to have been due to the still closer pressing-together of the angular and irregular fragments of shells, corals, and encrinites.

Taking, then, all these facts into consideration, the study of the interspaces seems to indicate that, as might have been expected, the effects of pressure are more marked in the older than in the newer rocks. It reaches, however, its maximum in some Devonian limestones associated with well-developed slaty cleavage. This is notably the case in a portion of a thin bed composed of joints of encrinites at Ilfracombe, in some parts of which nearly all traces of the original interspaces have been obliterated.

Magnesian Limestone.—The empty spaces seen with the microscope in some specimens, as in that of Bulwell (near Nottingham), are really places left empty when the crystals of dolomite were formed. In this they amount to 13·3 per cent.; but the whole history of the rock is too complex and difficult of understanding to make it desirable to do more than allude to it, as an example differing entirely from most others that I have described.

Cavities determined by the Boiling-Water Method.

Although the percentage of cavities now existing in a rock may throw little light on its original formation, yet it is of great interest in connexion with the present condition of the rock. Many of my experiments were made with building-stone, in order, if possible, to obtain some clue to the cause of their variable durability; but I will mass them together, and give such results as are of geological interest.

Sandstones.—First of all, we may compare the sandstones of New Red, Carboniferous, and Old Red age, of which I have examined a fair number, but only from a few districts, so that my results cannot be looked upon as necessarily applying to every place. Those of New Red age were from Maer in Staffordshire, and Mansfield in Nottinghamshire. Leaving out a shaly bed, the mean for the different places near Maer is 25 per cent., which agrees closely with the minimum for spheres and for fairly-uniform grains of sand. The variation from 21·5 to 28·8 may be explained by variation in the grains and the presence of a small amount of infiltrated carbonate of lime. The very friable sandstone at Mansfield, in which the rock-houses were excavated, contains 33·5 per cent. of vacuities; which looks as though the grains had never completely accommodated themselves to the smallest volume. The very special and excellent building-stones contain on an average only 16·9 per cent. of vacuities, and thus approach closely to the building-stones from the Old Red of Herefordshire; like them, the greater solidity is due partly to infiltrated calcite, and partly to deposition in deeper water, with less sorting of the material by bottom-currents.

The sandstones of Carboniferous age were from South Yorkshire, Derbyshire, and the Forest of Dean. The vacuities vary from 8·7 to 14·8 per cent., the mean being 11·1. This small amount is sometimes due to the original cavities having been, to a considerable extent, filled up by decomposed felspar; but sometimes it may be ascribed to the feebleness of the bottom-current.

The Old Red specimens studied were building-stones from various quarries in Herefordshire; and, leaving out those containing an unusual amount of carbonate of lime, the vacuities vary from 16·1 to 21 per cent., the mean being 18·4. In those containing much carbonate of lime, some of which are building-stones of remarkable durability in pure country-air, the cavities have been to a large extent filled up, and the mean is only 6 per cent. This infiltration and the greater depth of water and less bottom-current, and also exposure to pressure for a longer time, will explain why, on an average, there is a marked difference between the sandstones of Old and those of New Red age.

The Gannister near Sheffield is, in some cases, an excellent example of the almost complete filling-up by infiltrated quartz of the interspaces in a deposit of very fine sand. When fractured surfaces are examined with a microscope, many small crystals may be seen. The boiling-water method shows that, in some specimens, free from the rootlets of *Stigmaria*, the empty cavities amount to less than 1 per cent., which is far below what occurs in recently-deposited sand of fine grain. It seems to me probable that slight deposition of quartz may play an important part in causing the grains to cohere, in many sandstones which are not disintegrated by long exposure to strong acids, and thus resist exposure to the weather remarkably long.

Limestones of Oolitic age.—I have made many determinations of the open interspaces by the hot-water method; but their present structure is due to so many complex conditions, that the results are of little value in connexion with my present purpose. In the case of building-stones, the maximum was 37 per cent. and the mean of the maximum values about 31. The minimum for the more solid was 13 per cent. and the average about 15. This is as though such limestones were more or less solidified by infiltrated calcite not long after deposition, and as though the process had not been carried out so completely as in the case of many older rocks.

Kentish Rag.—By the boiling-water method I found that, in its natural condition, this rock contains only about 3·5 per cent. of open interspaces; whereas the much-weathered rock contains 26·1 per cent., owing to the removal of soluble material, which left the rocks with about the same amount of interspaces as in the case of the most closely-packed fragments.

The cavities in many other rocks will be considered, when dealing with the pressure to which they have been subjected.

Determination of the Cavities in Fine-Grained Rocks.

The physical characters of different fine-grained rocks vary so much that the fairly-accurate determination of the cavities is in some cases quite easy, but in others difficult. This is due to the combination of a number of curious relations between a certain class of rocks and water, which they seem to contain in four or five different conditions:—(1) The water is combined chemically, but lost on strong heating; (2) it is absorbed, somewhat like an occluded gas; (3) it is condensed from a damp atmosphere among the very minute particles, but not in such quantity as to fill the cavities; (4) when the rock is placed in water, the latter fills up the cavities; and (5) it may not only fill them, but interpenetrate the material with such force as to swell it to a considerable extent, or even break it up completely and cause it to occupy about twice and a third the volume of the solid material. One result of the absorption of water from a damp atmosphere is that it may be almost impossible to decide what ought to be looked upon as the correct weight. This is certainly not due to mere fine division, since very finely-powdered calcite does not increase in weight in an atmosphere saturated with water, whereas clays largely composed of decomposed felspar vary considerably in weight from day to day, according to the state of the weather, the amount varying up to at least 5½ per cent. Very much remains to be learned respecting the connexion of these facts with the history of the rocks; but they are evidently of fundamental importance, and in some way are connected with the age of the rocks. One striking fact is, that specimens of different geological periods may look almost alike, and yet differ enormously in their reaction with water.

Then, again, we have to consider the amount of soluble salts dissolved out by water, as shown by the loss of weight. The amount and the nature of these form a subject worthy of careful study. My results are but imperfect, because deduced from experiments made specially to learn other facts. So far as they go, they make it appear possible that in fine-grained shales there are to some extent the salts originally contained in the water from which the shales were deposited; this was slowly lost by evaporation, since the amount of salts is sometimes far greater than in the volume of sea-water that would fill the cavities. There is also in the specimens examined more salt in the shales deposited in sea-water than in corresponding rocks from the freshwater Coal-Measures.

When fine-grained rocks are treated with water, there is generally a certain amount of swelling, which may be so small as to be invisible, and there is no breaking-up; but sometimes the tumescence is so great and energetic, that the specimen quickly breaks up from a fairly-hard rock into a soft clay. In such cases, it is of course impossible to determine the amount of natural cavities by means of water, and almond-oil or benzol must be used. In this event allowance must be made for what may be dissolved by these liquids. When the rock does not break up, the volume of absorbed water may be considerable, and due almost entirely to swelling, the amount of oil or benzol absorbed being comparatively small. After being soaked with water, the specimen may, or may not, contract to its original volume, on drying. Then, again, after the specimen has been kept in water for several days, its weight when dried may be considerably less than it was, owing to the removal of soluble salts, which may amount to several per cent.

The Structure of Fine-Grained Deposits.

The results of my experiments with clay and chalk explain the structure of many deposits of nearly all ages. It appears as though the comparatively-recent mud of our estuaries may remain for a very long time in a sort of semi-fluid condition. That in the wide mud-flat in the Deben, opposite Waldringfield, which may have been there for ages, is so soft that an oar can be pushed down into it for a good many feet with scarcely any resistance, the mud being in much the same condition as the clay in my experiments, which did not subside more on keeping. In other cases, the upper part is very soft; but the material becomes firmer at a lower level, where it may have existed for ages under some little pressure. The much older clays and shales agree with what would happen if more and more included water had been squeezed out by the pressure of superincumbent material; and their fissility is like that produced in my experiments, being due, not to any alternation of different materials in the plane of bedding, but to the necessary change in ultimate structure brought about by the great decrease in thickness. As shown by these experiments, the interspaces in clay which had settled so as to become somewhat stable amounted in extreme cases to no less than 86 per cent. of the volume, whereas in the fine-grained shales of the Coal-Measures they amount only to 13 per cent., which indicates a probable reduction to about a sixth of the original volume. Considering the nature of the material, this would be quite adequate to develop an imperfect cleavage in the plane of bedding, in accordance with the principles which I have described in my papers on slaty cleavage. The experiments also explain the change consequent on the weathering of the shale, which has become soft, and now contains 39·2 per cent. of interspaces; this corresponds to what happens when the dried clay swells up by water forcing itself into it, such an effect being no doubt increased in the shale by frost.

As bearing on the production of schistosity in shales and of cleavage in slates, I may mention an experiment that I made with what was given to me as 'Brodie's graphite.' This, when filled into a brass tube about 2 inches long, was loose and bulky, but could be compressed down to about a quarter of an inch by a tightly-fitting brass rod, when it was found to be fairly solid, and to possess a more perfect cleavage than any slate, in the plane perpendicular to the direction of compression.

Chalk.—My experiments with fine-grained chalk show that, when deposited so as to arrive at a kind of temporary stability, it is a sort of imperfect liquid; and this was probably the case with the natural deposit. The amount of interspaces might well have been more than 70 per cent. My impression is that, many years ago, I examined a soft chalk in which they were still about 50 per cent. Artificially deposited, very fine-grained chalk, when contracted by drying without any pressure, contains 41 per cent. of interspaces, or sensibly the same as shaken shot or sand. The clunch used for internal artistic work was found, by the boiling-water method, still to contain 34 per cent. The Chalk of the Yorkshire coast has been much hardened by infiltrated calcite, and contains only about 15 per cent. Probably, then, the reduction in thickness of natural chalk has been to about 45 per cent. of the original thickness, which is a much smaller reduction than in shales; and this, combined with the difference in the character of the material, will explain why, in the case of the natural rocks, and in my experiments, no fissility was developed approaching that of shales.

Cavities in slate-rocks.—The determination of the exact amount of cavities in rocks possessing cleavage is rendered difficult by a peculiarity which, in most cases, is of no importance. After boiling in water, there can be little doubt that, as a rule, nearly all the loss of weight on drying is due to liquid water which existed in the cavities. In the case of slate-rocks, which are so nearly solid, what otherwise would be small effects become of importance. Thus, in the case of the Penrhyn slate, after being boiled in water, and kept in the water until the weight was constant, the specimen on being dried in the atmosphere at the natural temperature indicated (by loss) the existence of about ·24 per cent. of interspaces; but, when it was dried at a very moderate heat near a fire, there was a further small loss, which, if due to liquid water which had been in cavities, would indicate that these amounted to nearly a half per cent. This should make a very considerable difference in the calculated pressure, discussed later (p. 227). It seems, however, improbable that liquid water would remain in the rocks when dried for several days in the air of a warm sitting-room, and more probable that the further loss in weight represents water occluded in, or loosely combined in some way with, the solid material. If so, the question arises whether part or even all of the water lost after boiling is not also thus combined, and does not represent cavities. On the whole, in the present state of the question, it is probably the best to adopt a mean result, and to conclude that the empty spaces in the Penrhyn slate amount to ·24 per cent.

XIV. CONTRACTION OF ROCKS AFTER DEPOSITION.

This may be very little, or as much as 90 per cent. of the original thickness, even when nothing has been removed chemically. In the case of sandstones and allied rocks, having at first about 46 per cent. of cavities, the contraction when the grains had accommodated themselves to the least volume would be about 25 per cent., but much less if the sand consisted of grains of extremely-variable size. Much depends, however, on whether the cavities had or had not been filled with calcite at an early period; and a rock may now be almost quite solid, and yet may have contracted very little. Manifestly, then, we must rely on the evidence furnished by each particular case. As shown already, the best is that afforded by the change in the angle of rest of well-developed drift-bedding or ripple-drift. Information of a less reliable character may be furnished by concretions, as will be shown when dealing with them.

The contraction in the case of very fine-grained rocks may have been very much greater, since, even after standing for a year without further subsidence, such material may contain 90 per cent. of water; whereas consolidated older rocks of analogous character may have been made almost solid by the squeezing-out of this water—so that, in extreme cases, shales and slates may occupy only a ninth of the volume which they possessed when originally deposited. At all events, it is important to bear in mind that the thickness of many of our rocks may now be very much less than the original.

XV. Concretions.

The accurate, detailed study of concretions is a promising enquiry, likely to yield interesting and unexpected results. Except in the case of the green spots in some slates, which I studied carefully many years ago, I have unfortunately made but few accurate measurements in the field; and I must now rely on what can be learned from such specimens as are in my collection, which, however, include those of nearly all geological periods. It is desirable to divide them into those formed before they were finally deposited, and those formed in the rock after its deposition. The former division includes oolitic grains, pisolites, and a few analogous objects; and the latter, more or less rounded bodies of very diverse character.

Pisolites.

The mean ratios of the three principal axes of the examples in my collection of Oolitic age are 0·4, 1, and 1·33, no two being equal, and the shortest being unusually short. This is largely due to the flat nucleus. In a sandstone at Barlborough, near Sheffield, probably of Lower New-Red-Sandstone age, the ratios of the axes are 1, 1, and 1·41, and the well-developed grains are remarkably uniform in size. They are composed of fine sand held together by carbonate of lime, and their peculiar form was probably due to rolling along the bottom.

One very important fact connected with pisolites is that their flatter surfaces are often inclined at various angles to the stratification, as though deposited irregularly from a rather strong current. Concretions formed after deposition usually have their longer axes all nearly in the plane of stratification.

Oolitic Grains.

These are often remarkably uniform in size in each part of the rocks, as though more or less sorted by a current of water.

The Plane of Symmetry.

So far as I can judge from the specimens in my collection, the plane of symmetry of the rocks in three cases out of four coincides with the plane of stratification, but in one-fourth of the cases it is inclined at an angle varying from 5° to 10°. This is apparently in no way due to chance, being uniform in all the specimens from the same place. Those in my collection are not adequate to clear up all the difficulties, which would require extensive work in the field. The main question is whether the inclined plane of symmetry was produced at the time of deposition, or long after. If produced at the time, it seems to indicate that in that particular place the bottom was not quite horizontal, but inclined at a considerable angle, and that, although the layers of different quality lie in the plane of stratification, the unequi-axed particles were deposited as they fell horizontally. Subsequently the material settled in a perpendicular line, so that the unsymmetrical structure was increased. If these suppositions are allowable, all the facts seem explained. If the inclined plane of symmetry was produced long after deposition, it would indicate that the rock as a whole was somewhat compressed in a line perpendicular to this plane, so that it is analogous to one having a very imperfect slaty cleavage; but, considering all the circumstances, this seems extremely doubtful. Possibly, however, both explanations may be true in particular cases.

Concretions formed *in situ* in Stratified Rocks.

Their actual size is of secondary importance, and may vary greatly. The most important consideration is the relative length of their axes, which may be called a, b, and c, a being the shortest and c the longest. This may depend, to a considerable extent, on the length of the axes of the original nucleus, which we may call x, y, and z. Round this the material of the concretion has been collected; and the thickness in different directions depends, to a great extent, on the structure of the rock. When this is the same in all directions in a particular plane, which we may call the plane of symmetry, the thickness of the deposit may be called T; but perpendicular to this it may differ, and may be called T'', the water penetrating equally well in all directions along the plane of symmetry, though with greater difficulty perpendicular to it, probably because the flat particles of the rock lie mainly in the plane of symmetry. This is assuming that, when the nucleus is elongated, its thickness is constant. If this varies much, there may be a corresponding variation in the thickness of the deposit; in extreme cases, therefore, the concretion may taper off to a sharp point, or show other abnormalities. We may, then, generalize the axes as follows:—$x+2T'$, $y+2T$, $z+2T$. If, as in some cases, x, y, and z are small or nearly equal, and T and T' are also equal, the concretion is a sphere. In many cases, however, though x, y, and z are small or nearly equal, T' is less than T, owing to the structure of the rock, and the result is that two axes are equal and the third less. In other cases x, y, and z, and perhaps also T and T', are unequal, and consequently all the axes differ. In a few cases, two axes are equal and the other much longer, owing to an elongated nucleus. Of course, the effect of the nucleus becomes relatively less as the concretion increases in size. In some cases, owing to variations in the nucleus, examples of several of the above-named groups are found in the same rock, but sometimes all belong to one of them.

It seems probable that, in most cases, the concretions were formed at an early period in the history of the rock, while the nucleus was still chemically active.

The relative length of the axis perpendicular to the plane of symmetry seems to have depended chiefly on the nature of the material in which the concretion was formed, and on the amount of contraction which had previously taken place. If the original deposit consisted of particles more or less completely symmetrical in all directions, a considerable amount of contraction would produce little or no effect, and the growth of the concretion would take place equally in all directions. This would explain why the concretions in the Magnesian Limestone are almost perfect spheres. In the case of those in fine-grained sandstone at Arborthorne (near Sheffield), the axis perpendicular to bedding is 90 per cent. of those in its plane. The thickness of sand quickly deposited is reduced to 89 per cent. when well shaken, or to about the relative length of the shorter axis in the Arborthorne concretions, and is exactly the same as the mean for them and the green spots in the Old Red Sandstone. The shortest axis in one from shale at Woodbourne is 49 per cent. of the mean of those in the plane of symmetry. When fine clay which has subsided for a day is allowed to subside for a year, the contraction is 54 per cent. of the original, and would have been close on 49, if the measurement had been taken earlier in the day, or if the clay had been subjected to the pressure of superincumbent deposit. The explanation that I would suggest is that, as may be seen with the microscope, many of the fine particles of clay are flat flakes, and these to a great extent subsiding as compound granules would originally be inclined at all angles to the horizon. Then, when the deposit contracted vertically, they would become less and less inclined, and the permeability for water would be increased in the plane of symmetry and decreased in a line perpendicular to it. In such a case, the relative length of the short axes of the concretion would indicate the approximate amount of contraction. This conclusion would, however, apply to a particular class of rock alone, and then only approximately.

Special Examples.

The concretions in the Magnesian Limestone seen along the coast north of Sunderland vary much in size, but when not interfered with by others are almost true spheres, the axes being 100 : 100 : 100, and when they leave impressions upon one another these are almost perfect circles. In my best specimens no axis differs from the mean by more than a hundredth of an inch. The nuclei must, therefore, have been small, and the surrounding material of very uniform structure in all directions.

The concretions in a fine-grained sandstone at Arborthorne (near Sheffield) are a good example of those which have the axes in the plane of stratification nearly equal, and that perpendicular to it shorter. The means for the bigger specimens are 92 : 100 : 100, and for the smaller 87 : 100 : 103—thus showing the greater effect of the nucleus on the smaller, and in both cases the effect of the different structure of the rock in a direction perpendicular to the bedding.

A red sandstone used in Gloucester, which looks very like some of the Old Red Sandstone of the Black Mountains, contains very perfect green spots, varying in size from very small up to 1 inch or more in diameter, which can easily be measured to a hundredth of an inch. The material is of fine grain, the particles varying in size from a hundredth of an inch down to very small. It is very compact, and by the boiling-water method was found to contain only 7·2 per cent. of cavities. There is no indication of a bottom-current of as much as 6 inches per second; and the whole structure is that of a deposit formed in fairly-deep water, the chief current being some distance from the bottom. In the plane of symmetry the ratios of the axes are 100 : 101, the variations from this being only a hundredth of an inch. Perpendicular to the plane of symmetry the axes are 88 : 100, or closely the same as in the concretions at Arborthorne. Unlike them, they cannot be obtained separate, and are merely the red colour discharged by the deoxidizing action of some nucleus. It is important to observe that the plane of symmetry in all my specimens is inclined at about 10° to the stratification, probably because the bottom was not horizontal.

A small concretion from the Coal-Measures at Woodbourne (near Sheffield) is an excellent illustration of the laws of formation. It is split open through the centre, showing the exact shape of the nucleus, which is very much like the seed of a plant. The dimensions of this are: perpendicular to the plane of symmetry ·2 inch, and in the plane ·33 and ·50. In this plane the deposit is on two sides ·55, and on the other two ·60; so that, including the nucleus, we have for the entire concretion $\cdot33+2\times55=1\cdot43$ and $\cdot50+2\times60=1\cdot70$. Hence there is not equality, but rather more deposit in the line of the long axis of the nucleus, probably due to a slight difference in structure in the direction of the gentle current, which caused the long axis of the nucleus to lie in that direction. Perpendicular to the plane of symmetry, we have for the shortest axis $\cdot20+2\times\cdot28=\cdot76$; so that the three axes are ·76, 1·43, and 1·70. It is also important to notice that the plane of approximate symmetry is inclined to the stratification at an angle of 5° in the line of the longest axis of both nucleus and concretion, as though the bottom dipped in the line of current.

In the case of a concretion from the Coal-Measures at Broomfield (Sheffield), in a fine-grained sandstone, the plane of symmetry appeared to be inclined at about 20° to the stratification, which may be partly due to compression near a fault.

In driving a drift through the Lower Coal-Measures at Stannington (near Sheffield), some very large and perfect concretions were found in a sandy shale. One of these, over 7 inches in diameter, gives as the ratio of the axes 78 : 100 : 103. Besides this, I collected a considerable number of much smaller size, down to less than an inch in diameter. The variation in their shape is so great, that at first sight it might seem impossible to deduce from them any general conclusions. A few differ completely from the rest, being broad and flat, imperfectly consolidated, and break up in the plane of bedding. In my specimens the ratios of the axes are closely the same as 40 : 100 : 101. By far the greater number are, however, hard and solid, but vary in general shape, one tapering off to a point at both ends. Taking, as usual, the shortest axis in the plane of bedding as 100, the longest axis varies from 207 to 100 and the shortest from 94 to 55, the means of all my twenty specimens being 81 : 100 : 127. Those which are almost circular in the plane of bedding, and therefore have a nearly-symmetrical nucleus, give for the axes 79 : 100 : 101; which differ from those in the case of the large specimen by about a hundredth of an inch. Hence, at the time when the concretions were formed, the relative permeability of the rock in a line perpendicular to the stratification was about 79 per cent. of that in its plane. The prevailing greater length of one axis in the plane of bedding was probably due to the nucleus being more or less elongated, combined with a slight want of symmetry in the rock, due to current. I have entered into all these details, in order to show that the proper discussion of very unpromising material yields nevertheless extremely satisfactory and concordant results.

Fortunately, I possess a few specimens of coarse shale with included concretions. These show that the layers of the rock do not pass through the concretions, but curve round them, as if the shale had been compressed to about half its thickness since they were formed, unless they (to some extent) pushed it aside in growing. Of this there seems to be no good evidence, whereas experiment shows that there must have been a great vertical contraction in shales and clays. This is, however, a question which needs further investigations, differing from any that I have been able to carry out.

The following (Table VII) is a list of all the concretions which I have studied, with the ratios of their axes, the shortest axis in the plane of bedding being taken as 100:—

TABLE VII.

All axes nearly equal.			
1. Magnesian Limestone, Sunderland	100	100	100
2. Post-Glacial, Bridlington	103	100	106
Two axes nearly equal.			
3. In fine sand, Arborthorne	90	100	101
4. Green spots, Old Red Sandstone	88	100	101
5. Lower Coal-Measures, Stannington (small)	79	100	101
6. Do. do. do. (large)	78	100	103
7. Fish-nodule, Old Red Sandstone	53	100	103
8. Imperfect, Stannington	40	100	101
Elongated nuclei.			
9. Post-Glacial, Bridlington	100	100	222
10. Pyrites in Chalk	100	100	444
All axes unequal.			
11. Gault, Lidhurst	88	100	153
12. Post-Glacial, Bridlington	86	100	136
13. Lower Coal-Measures, Stannington	81	100	127
14. Oxford Clay	77	100	143
15. Coal-shale, Woodbourne	54	100	190

Nos. 1, 3, 4, 5, 8, & 13 are means of a number of specimens, and the rest single good cases.

In the case of pyrites in chalk, it seems clear that this pushed aside the chalk and did not include it. On the contrary, when concretions of carbonate of lime or iron were formed in sandstone, they appear to have filled up the interspaces between the grains, and did not displace them. In some rocks, both kinds of action may have combined.

XVI. SPOTS IN WELSH SLATES.

Many years ago I made nearly 200 measurements of these spots, determining the length of greatest and least axes to about a hundredth of an inch, excluding those manifestly much influenced by stratification.

At that time I little thought what interesting conclusions could be drawn from them, and my object was merely to ascertain the amount of compression to which the rock had been subjected when the cleavage was developed. I even then saw that the original spots were not spheres, and endeavoured to make my observations so that the stratification was inclined at all angles to the cleavage, and the spots could be treated as spheres in making my calculations. In some cases this was approximately correct, but not in others; and my results were not sufficiently accurate for the purpose now in hand. I devoted therefore some weeks to the discussion of the observations, my aim being to learn what was the condition of the material when first deposited; the period in its history when the spots were formed; their exact shape; the extent of the consolidation to which the rock had been exposed, before cleavage was developed; the amount of lateral compression caused by the elevation of the Welsh mountains; and the change in dimensions and structure of the rock, caused by the compression. Since this may appear an impossible programme, it will be necessary to enter into much detail, in order to show how the facts enable us to learn all these particulars approximately.

In many cases, when measuring the axes of the spots, I also determined the direction of the stratification; but this could not always be done, and in some cases there is so much contortion that the conclusions are doubtful. I invariably kept separate measurements made in the plane of cleavage and perpendicular to it. They were made in a quarry at Bethesda, in one at the bottom of Nant Ffrancon, in the great Penrhyn quarry, and in sundry quarries near Llanberis. The first step was to calculate the relative length of the longest axis, that of the shortest being taken as unity. In some places, where the stratification was fairly uniform, this ratio did not differ materially in as many as 26 cases; but in others there was a wide difference, because of the stratification cutting the cleavage at various angles. This was visible at once by marking the value of each measurement along a straight line. It was then seen that, for certain calculations, the measurements in the quarry at Bethesda were extremely good, since the long axis of the spots seems to be inclined at all angles to the cleavage. Those in the quarry at Penrhyn in a plane perpendicular to the cleavage were also well distributed at all angles, but in the plane of cleavage were inclined on an average at 27° to the horizontal. In the quarries in Nant Ffrancon and at Llanberis, the data, being intermediate, are not good for calculation.

Although having much in common with concretions, the spots were due to the deoxidization of the peroxide of iron. They are really ellipsoids; but, since the calculations require the ratios of the axes in particular planes, we need not consider the third dimension.

In studying the rocks *in situ*, it was easy to see that the axis of the original spots was longest in the plane of stratification; and the question to be first considered is, what was the exact ratio of the axes? It was assumed, in the first place, that in the plane perpendicular to the cleavage, when the rock was compressed, the sectional area of the spot remained the same; so that, for example, if the axis in the line of the pressure was reduced to one-half, that at right-angles was increased to double. If then the longest axis of the spot was inclined at all angles to the cleavage, the maximum value of the ratio between the longest and the shortest axis must be when the stratification was nearly in the plane of cleavage, and the smallest ratio when it was nearly perpendicular to the cleavage. It can thus be easily shown that the ratio of the shortest axis of the original spot, compared with its longest, was the square root of the smallest ratio after cleavage was developed, divided by the largest. In the quarry at Bethesda, perpendicular to the cleavage, the smallest ratio is 1:4 and the largest 1:10. Hence we have $\sqrt{\frac{4}{10}} = {\cdot}632$, that is the axes of the original spots were 63:100:100. In the quarry at Penrhyn the extreme ratios are 1:4·78 and 1:11·96, which give $\sqrt{\frac{4{\cdot}78}{11{\cdot}96}} = {\cdot}632$, or the same as Bethesda. However, the probability is, that in both cases the result is somewhat too large. Combining the measurements made at both quarries, and taking the means of the maxima and minima, we get 3·66 and 10·37; and therefore $\sqrt{\frac{3{\cdot}66}{10{\cdot}37}} = {\cdot}597$. Consequently, I conclude that the axes of the original spots were about 60:100:100. In the quarry at Bethesda, in the plane of cleavage we get $\sqrt{\frac{1{\cdot}26}{3{\cdot}01}} = {\cdot}645$, whereas at Penrhyn we have $\sqrt{\frac{1{\cdot}21}{1{\cdot}86}} = {\cdot}836$. These last two results agree with what would be expected from the manner in which the axes of the original spots were inclined to the cleavage. At all events, it seems reasonable in further calculations to adopt the conclusion that, before cleavage was developed, the axes of the spots were about 60:100:100.

The next thing to consider is, what change was produced by the compression of the rocks. For the quarry at Bethesda, the mean ratios of all my observations are, in the plane of cleavage 1:2·01, and perpendicular to the cleavage 1:7·10; and for the quarry at Penrhyn, 1:1·60 and 1:6·99. The great discrepancy between the results in the plane of cleavage is mainly due to the fact, that at

Bethesda the spots had their axes distributed at nearly all angles to the cleavage, but at Penrhyn at angles varying from 17° to 41°, the means of all my measurements being 27°. Calculating from this, and correcting accordingly, we obtain as the corrected ratio $1{\cdot}60 \times \frac{865}{648} = 1{\cdot}86$. The mean for both quarries would thus be 1:1·94 and 1:7, and the length of the shortest axis $\frac{1{\cdot}94}{7{\cdot}04} = {\cdot}275$. The length of that in the line of strike being taken as unity, we obtain the following results:—

Before cleavage was developed .... $\cdot600 \times 1{\cdot}00 \times 1 = \cdot60$
After cleavage had been developed ... $\cdot275 \times 1{\cdot}94 \times 1 = \cdot53$

Hence, the reduction in volume due to pressure was from 60 to 53, or about 11 per cent. The most satisfactory explanation of this is that, before compression, the rocks contained about 11 per cent. of cavities, which were almost completely squeezed up, since the slate now contains only about ·24 per cent. This 11 per cent. must be looked upon as only approximate, since it depends on four quantities all subject to error. I consider, however, the result very satisfactory in showing that, before cleavage was developed, the rock was, if anything, rather more consolidated than the fine-grained beds of the Coal-Measures, an inference which agrees admirably with the microscopical structure described in other parts of this paper.

These various data seem to lead to the following conclusions:—

(1) The material when deposited was not exactly a clay, since the axes of the spots were originally 60:100:100, instead of more nearly 52:100:100.

(2) The spots were formed early in the history of the rock, before the deoxidizing power of some nucleus was lost.

(3) The rock was fairly hard and consolidated.

(4) It was subjected to enormous pressure, and so changed in dimensions in different directions that cleavage was developed.

(5) The facts appear to establish conclusively the mechanical origin of cleavage.

XVII. Slip-Surfaces.

On carefully examining some varieties of much-contorted mica-schist, it is very easy to see that, in many cases, there has been an irregular giving-way of the rock in a plane perpendicular to the pressure, and one bent portion has slipped over another, so as to give rise to a surface of partial or complete discontinuity. A similar structure can be seen with the microscope in thin sections of some imperfectly-cleaved slates; and that these slip-surfaces are, as it were, microscopic faults, often from a hundredth to a thousandth of an inch apart, is proved by the upthrow or downthrow of very thin beds, or by broken flakes of mica, the movement being a hundredth of an inch or much less.

As seen under the microscope, these slip-surfaces in section look like very thin black lines, on the whole perpendicular to the line of pressure, as shown by small contortions of the bedding; yet they are seldom straight, being, as it were, drawn by a shaking hand. They are sometimes parallel for a short distance, but usually unite with or branch into each other. Although small in size, these slips are sometimes so numerous that, if those in a square inch of surface were united end to end, they would extend to a length of fully 50 feet; and, if the surfaces of those in a cubic inch were united, they would give an area of 5 or 6 square feet. It need not be wondered, then, that they play so important a part in the structure of the rock; and they prove most conclusively that, when it was compressed by the force which developed the cleavage, the rock did not give way as a truly-plastic substance, but to a large extent yielded as though fairly hard. These slip-surfaces are in fact analogous to the slip-planes in metals described by Prof. J. A. Ewing & Mr. W. Rosenhain.[1]

The detailed study of these slips in different rocks is very instructive. I have examined carefully all my numerous thin sections of rocks allied to slates, and find that in those without cleavage comparatively-long slips are absent, and even short ones occur only when there is a very sudden change in the nature of the rock, as, for example, from fine sand to clay. Highly-cleaved thickly-bedded slates, like those of Penrhyn, show very few, if any long slips. On close examination, however, of a section perpendicular to the cleavage in the line of dip, vast numbers of those a hundredth of an inch in length or less can be seen; but they are not so much developed in the line of strike.

I have an excellent section of a purple slate from Birnam (near Dunkeld), containing a band composed of a mixture of a green and a colourless mineral, bent into contortions which show that there has been great compression in a line perpendicular to the cleavage. The above-mentioned minerals must have been formed before the cleavage, since they are broken up in the most remarkable manner, and the structure of the fine-grained purple slate near the junction is very instructive. At the round ends of the contortions, where the green band has protected the purple slate from compression, its structure is almost exactly like that of an uncleaved rock; whereas alongside the contortions, where there must have been great differential movement, the purple slate looks like a complete mass of slip-surfaces, which are only $\frac{1}{2000}$ or $\frac{1}{3000}$ inch apart. In passing away into the purple slate the longer slips rapidly decrease in number, and there are only slips of a hundredth of an inch or so in length between the constituent particles of the rock. It will thus be seen that there is good evidence to prove that, when the rock was greatly altered in dimensions by pressure, it was so hard that the crystalline mineral was broken up, and the purple part yielded by slip-surfaces. These conclusions are well borne out by similar

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[1] Phil. Trans. Roy. Soc. ser. A, vol. cxciii (1900) p. 353.

facts. In a considerable number of different localities I have met with fine-grained, spotted slate-rocks; and, on careful examination with the microscope, it was easy to see that the spots are small imperfectly-developed crystals of chiastolite, somewhat broken up and compressed, with their long axes in the plane of cleavage. The relation between these and the surrounding rock clearly shows that, when slaty cleavage was developed, the rock was nearly as hard as chiastolite, yielding little more to compression.

As already shown, the best slates of Penrhyn prove that, in the line of dip, portions of rock which before cleavage were 100 : 100 are now on an average 100 : 705. The small flakes of mica, or a mineral of similar character, which constitute so large a portion of the rock, may be assumed not to have changed their dimensions, and thus the entire yielding of the rock must have been due to the giving-way of the surrounding finer particles. There must consequently have been a great relative movement; and, assuming as a fair average that the flakes of mica form one half of the rock, the surrounding material must have been elongated along the line of dip from 1 to 1 to 1 to 14, whereas the flakes were not elongated at all. It is thus easy to understand why there is so vast a number of minute slip-surfaces perpendicular to the pressure among the ultimate constituent grains in the line of dip, and why such surfaces are much less developed in the line of strike, where little elongation could occur. A number of these facts were overlooked in my early papers, and they show that the compression of the rock would alter its structure much more than I suspected. The change in the position of the flakes would be twice as great as I calculated. Instead of half of them being as before cleavage spread over 90°, they would after compression be spread over only 8°; and this, combined with the numerous small slip-surfaces, easily explains the fissility of the rock.

Slate near Penrhyn, which has been much changed in appearance by a trap-dyke, still shows well-marked traces of the previously-existing slip-surfaces, so that the rock cannot have been softened by heat. In the mica-schist between Aberdeen and Stonehaven, with cleavage-foliation and therefore metamorphosed after compression, all traces of slip-surfaces have been obliterated.

XVIII. SURFACES OF PRESSURE-SOLUTION.

In my Bakerian lecture to the Royal Society[1] I showed that pressure increases the solubility of the greater number of soluble salts. Also in my paper on the impressed limestone-pebbles of the Nagelfluh[2] in Switzerland, I showed that the impressions could not have been produced by simple mechanical action, since it is

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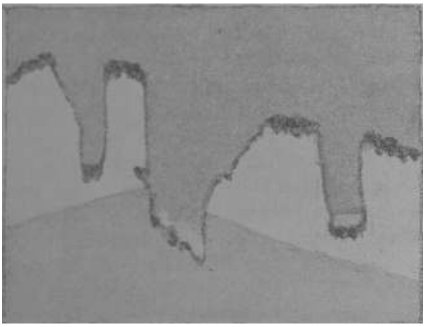
[1] Proc. Roy. Soc. vol. xii (1863) p. 538.
[2] Neues Jahrb. 1863, p. 801.

only the carbonate of lime that has been removed by solution, and the whole of the insoluble material of the stone has been left, as carefully determined by suitable means.

An equally striking example of pressure-solution was described in my address to the Geological Society,[1] in which I showed that in a Devonian limestone at Ilfracombe the joints of encrinites have been partly dissolved where the pressure which produced the cleavage was at a maximum, and the dissolved calcite has crystallized out where the pressure was at a minimum. Until quite recently I did not recognize fully how important a part this action plays in many other cases.

What I propose to call surfaces of pressure-solution are often numerous and small; but some years ago I obtained from the Carboniferous Limestone of Stoney Middleton (Derbyshire) specimens in which they are unusually large. The normal characters are layers of dark, apparently bituminous, material extending over a considerable area, passing up and down like larger and smaller interlocking teeth. Sometimes one of these layers branches into two, which may again unite, and they cannot have been due to stratification. The most probable origin of the bitumen is that it is a residue of the solution of the limestone; and that solution of carbonate of lime has occurred is clearly proved, by the manner in which the layers pass into the shells of brachiopoda and into encrinites. This is shown by the accompanying text-figure. In the centre, not shaded, is a portion of a shell with well-preserved structure, and, as will be seen, the zigzag bituminous layer (shown black) passes quite through the centre, and partly on each side into two depressions filled with the limestone (shown shaded throughout). The laminar structure of the shell seems to have slightly influenced the direction of solution. Taking all the facts into consideration, it seems as though both pressure and solution have acted, and in some cases their combination will explain the facts. However, when we come to examine the detail of

Tooth-like structure penetrating, by removal, a fossil shell and the surrounding limestone, most probably by pressure-solution. (Magnified 4 diameters.)



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[1] Quart. Journ. Geol. Soc. vol. xxxv (1879) Proc. p. 89.

the Stoney-Middleton specimens, of which I have ten excellent microscopical sections of unusual size, it seems almost impossible to explain why the pressure should have been at a maximum along so complex and tooth-like a surface; and sometimes I feel tempted to conclude that a cause may have acted, about which we know little or nothing. The quantity of carbonate of lime dissolved and transferred to where the pressure was less must have been considerable, as shown in the figure by the amount of shell removed.

It is chiefly in those Devonian limestones in which slaty cleavage has been developed that surfaces of pressure-solution on a small scale are common, and play an important part in modifying the structure, since they have often conspired in altering the form of joints of encrinites and other organisms. In a few cases, however, notably in a specimen from Kingskerswell, the rock has been greatly changed as a whole, and joints of encrinites altered from about 1 : 1 to 1 : 4. There is also another interesting fact which for a long time puzzled me—that is, the alteration in crystalline calcite in which pressure has sometimes given rise to curved cleavage, or broken up the crystal into thin layers of twinning, similar to what can be produced by a knife, as shown by H. Baumhauer.[1]

Oolites with Pressure-Solution.

The most remarkable specimen in my possession is one from the Carboniferous Limestone on the south side of the gorge at Clifton. As will be seen from Pl. XVIII, fig. 2, instead of the grains being well separated or merely touching one another, as in Grantham Oolite, they (as it were) interpenetrate to a considerable extent by surfaces of pressure-solution, and yet their structure is not materially disturbed. This interpenetration could not have occurred, if the interspaces had been filled by infiltrated calcite before the rock was subjected to great pressure. This seems to have acted in a line perpendicular to the stratification, for there is little or no interpenetration in the plane of bedding. In the part figured the interspaces amount to only 11 per cent., which is a great reduction from 26 per cent., and closely corresponds with the change of dimensions calculated from the shape of the oolitic grains, which is from 100 to 83. The production of this exceptional specimen may be explained by supposing that the interspaces remained unfilled until after the rock had been exposed to great pressure, and that the cavities were then filled by material possibly transferred by pressure-solution from the altered oolitic grains.

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[1] Zeitschrift für Krystallographie, vol. iii (1879) p. 588.

In some places east of Sheffield similar surfaces of interlocking teeth, separated by a thin layer of earthy residue, are met with in the Magnesian Limestone, which well deserve further study, and might show that it is necessary in the case of dolomites to take into account other circumstances besides pressure and simple solution.

XIX. DETERMINATION OF THE PRESSURE TO WHICH ROCKS HAVE BEEN SUBJECTED.

That the pressure brought to bear when mountains were elevated and slaty cleavage developed was very great will readily be admitted, since it causes fairly-hard rocks to yield as if more or less plastic. It appeared to me, however, unsatisfactory to remain content with merely calling it a great pressure, and not to attempt to form some estimate of its value in tons to the square inch, or the weight of so many feet of superincumbent rocks. I therefore attacked this problem, both by experiment and by the discussion of my observations made with different rocks.

My first experiments were made in strong brass tubes, ·6 inch in internal diameter, into which a solid brass rod fitted. Finely-powdered dry pipe-clay was used, since the presence of water would have greatly increased the difficulties. The tube was filled with this clay and it was compressed by hand with the brass rod, and so much added as to make the length of the column of clay 2 inches. Then gradually increasing pressure was applied, and the reduction of volume determined by measurement. The weight of water filling the tube up to 2 inches and that of the clay were known, and also the specific gravity of the solid material of the clay, from which it was easy to calculate the percentage of empty spaces, when variously compressed. Taking the solid volume of the clay as 100, that of the interspaces was, when pressed by hand, 76·7; when the brass rod was driven in by moderate blows of a 1 lb. hammer, it was 34 per cent.; and with hard blows, 21·4 per cent. This method, however, did not enable me to estimate properly the pressure. This was attempted by forcing in the rod by a screw, with a pressure estimated at not much short of 10 tons, or about 30 tons to the square inch, which is equal to the pressure of about 75,000 feet of superincumbent rock. In this case the amount of interspaces was 29·2 per cent. It is, however, probable that it would have been a good deal less, if the pressure had been continued for a long time. These experiments, therefore, merely show that, in some way or other, the interspaces vary inversely as the pressure.

The Table (VIII) on the following page is a list in descending order of all my determinations of the interspaces in clays, shales, and slates, adopting what seem to be the most probable results. The amounts are percentages of volume.

TABLE VIII.

Clay from an old lake at Sewerby ...	49·5	
After Boulder-Clay, Bridlington ...	33·9	32·0
Alluvial clay, Orgreave	30·2	
New Red Marl, Leamington	34·4	32·2
” ” ” ”	30·0	
” ” ” from a deep boring .	10·0	7·5
” ” ” ” ” ” .	9·0	
” ” ” ” ” ” .	6·2	
” ” ” ” ” ” .	4·8	
Tertiary Clay, Watch Point	29·8	28·8
” ” Whitecliff	28·4	
” ” Barton Cliff	28·3	
Boulder-Clay, Bridlington	25·5	24·8
” ” ”	25·5	
” ” ”	23·4	
” ” Balby (nr. Doncaster).	24·1	24·0
” ” ” ” .	23·9	
Kimmeridge Clay, Oxford	30·7	24·8
” ” Filey	19·0	
Liassic clay, Bath	27·7	24·4
” ” ”	23·0	
” ” Robin Hood's Bay ...	22·5	
Gault, Aylesford	28·1	24·0
” Folkestone	25·0	
” ” bottom	18·9	
Speeton Clay, weathered	18·4	13·6
” ” unweathered	8·8	
Coal-Meas. nr. Nottingham, 510 ft. .	13·4	12·9
” ” 829 ft. .	14·6	
” ” 1190 ft. .	10·7	
” Brightside	14·3	12·5
” Darnall	12·8	
Wenlock Shale, Malvern, weathered.	14·1	10·0
” ” unweathered.	5·8	
Slate, Mawnan (Cornwall)	5·9	
Slates, etc., Moffat, black	5·2	3·6
” ” ”	3·8	
” ” red	3·4	
” ” green	2·1	
Slate, Hele, Ilfracombe	3·5	2·4
” ” ”	1·3	
Westmorland slate, black	0·55	0·49
” ” Coniston	0·52	
” ” Langdale	0·40	
Purple slate, Penrhyn	0·24	

A few remarks on these results may be apposite. The amount of invisible cavities in the apparently-solid deposit from an old lake at Sewerby is so unusual, that I thought I had made a mistake—until I found that a second experiment gave exactly the same result. The probable explanation is that the material is of abnormal character, and not comparable with clay. The percentage of cavities in the New Red Marl in some cases seems so abnormally great as to indicate the removal by weathering of some constituent, which is easily understood when we find that specimens from a considerable depth in a deep boring, given to me by Mr. J. A. Howe, contain so little of interspaces and also crystals of intercrystallized calcite, giving fractures of uniform reflection. The amount of cavities in a number of specimens of Boulder-Clay is so remarkably uniform, as to make me think that this deposit deserves further study. It is so much less than would agree with the small pressure of the material now lying over them, as to make me think that the clay has been somewhat compressed by the lateral pressure which forced it along, or by superincumbent ice, or by some other special cause not fully understood. The great variation in the Gault and Kimmeridge Clay is probably due to carbonate of lime. The much smaller amount in the Speeton Clay than in the Gault, as in the Yorkshire Chalk compared with that from many other localities, is certainly due to this cause. The Liassic clays were fairly satisfactory. Taking everything into consideration, the Coal-Measure shales give good results. The specimens of Wenlock Shale at my disposal were unsatisfactory, since one contains too much carbonate of lime, and the other is too much weathered. The variations in the slaty rocks of Mawnan, Moffat, and Hele are in some cases most certainly due to varying pressure connected with slaty cleavage. What is wanted is the careful study of more and better specimens, unaltered by infiltration, chemical action, or surface-weathering, collected so as to be specially suitable for this subject.

It will thus be seen that, in some cases, the results for different specimens agree sufficiently well, and therefore the mean may be relied upon as approximately correct. In other cases, there are great differences, sometimes certainly due to the presence of too much carbonate of lime, and sometimes to the specimens being too much swollen and weathered. Collecting together those cases which seem most trustworthy for calculation, we may compile the following list :—

Table IX.

Alluvial clay, etc.	32·0	Coal-Measure shales	13·2
Tertiary clay	28·8	Slate-rocks, Moffat	3·6
Boulder-Clay	24·4	Slate-rocks, Hele	2·4
Liassic clay	24·4	Slates, Westmorland	0·49
Gault	24·0	Slate, Penrhyn	0·24

It will thus be seen that there is a fairly-uniform decrease in the amount of cavities, in passing from clays which have been subjected to very little pressure, down to the oldest rocks; and a most marked decrease in those with well-developed slaty cleavage. The question then arises whether the compression is due to age or to pressure. Although quite prepared to believe that mere age may have some effect, when combined with pressure, yet taking all into consideration, and bearing in mind my experiments with clay, it seems to me more probable that the chief cause of the compression was the pressure of superincumbent rock, or of that which developed slaty cleavage.

Though it seems almost certain that the amount of interspaces varies in some way inversely as the pressure, their exact relation is unknown. Possibly it could be learned by experiments with a testing-machine, by means of which suitable pressures could be applied continuously for a long time. It seems, however, undesirable to delay the publication of this paper, and better to make use of the data now known. It is clear that the law must be of such a kind that the effect of a given increase in pressure is much greater on material which has been slightly compressed and contains a large amount of cavities, than on highly-compressed rocks. It is, of course, not the compression of the cavities, but the deformation of the solid particles involved in filling them up which is of prime importance; and this must be relatively greater and greater as their amount becomes smaller and smaller. This must have been brought about by the slow giving-way of minute grains during long geological periods, and not by the sudden fracture of large objects, as taken into account in studying the strength of materials for engineering purposes.

The percentage of cavities in fine-grained clays of great antiquity but never exposed to the pressure of more than a few feet of superincumbent material, is about 33 per cent.; and, after careful consideration, it seemed to me probable that the formula connecting cavities and pressure may be of the form:

$$\text{constant quantity} \times \frac{32-\text{cav.}}{32\times\text{cav.}}$$

In order to make the best use of the material at my disposal, I adopted the following plan to learn the value of the constant. The amount of cavities in clays of great antiquity, which have never been exposed to a pressure of more than a few feet of superincumbent material, is about 32 per cent.; and I endeavoured to ascertain from known geological data the thickness of superincumbent strata corresponding to a medium amount of cavities. The best cases at my disposal were from the Coal-Measures near Sheffield, and the calculated cavities in the Penrhyn slate before cleavage was developed. The former gave 13½ per cent. of cavities for a medium thickness of 2500 feet, and the latter 11 per cent. for a thickness of about 3400 feet of rock, as now compressed, of specific gravity estimated at 2·85, which equals 3976 feet of rock of specific gravity = 2·50. Combining these together, I obtained for 13½ per cent. of cavities a pressure of 2700 feet of rock, instead of 2500, which would agree with the supposition that 200 feet of the uppermost part of the Coal-Measures has been lost by denudation. As a first attempt, I therefore worked on the supposition that for no pressure the cavities are 32 per cent., and for 2700 feet 13½ per cent.

On the whole, it seems to me probable that the necessary force may vary as the ratio between the amount of solid material and that of the cavities, commencing at 32 per cent.; something like the strength of a beam of the same thickness with a long or a short span. From this it would follow that, when the cavities approach 32 per cent., a small pressure would produce considerable effect; whereas, when the percentage is small, a great increase in pressure would be necessary to produce much influence. On this supposition, the law connecting cavities and pressure would have the form

$$2700\text{ feet} \times \frac{\dfrac{100-\text{cav.}}{\text{cav.}} - \dfrac{100-32}{32}}{\dfrac{100-13{\cdot}5}{13{\cdot}5} - \dfrac{100-32}{32}}$$

So far as I can see, this agrees with all the facts of the case, and yet it must be looked upon as only a plausible approximation. Although this and the previous formula differ much, yet, strange to say, they yield so nearly the same results as to make me think that they may not be far wrong.

The Table (X) on the following page was calculated from the formula just given, and shows not only the ratio between the volume of the solid material and the cavities, but also the pressure in feet of rock of specific gravity 2·5, and that in tons per square inch.

TABLE X.

	$\frac{100-c}{c}$	*Feet of rock.*	*Tons per square inch.*
Little pressure	2·123	0	0
Tertiary clays	2·472	220	0·102
Boulder-Clay	3·032	573	0·265
Liassic clays	3·160	616	0·285
Gault	3·167	658	0·304
Coal-Measure shales	6·407	2·700	1·250
Penrhyn slate before cleavage	8·091	3·807	1·764
Slate, Mawnan	15·949	8·715	4·040
Moffat rocks	26·777	15·550	7·200
Hele, Ilfracombe	40·666	24·310	11·250
Slates, Westmorland	203·080	126·800	58·700
Slate, Penrhyn	416·700	261·500	121·000

Compared with clays, shales, and slates, limestones and sandstones are unsatisfactory for the determination of pressure, since those of different ages vary so much in important particulars. Still, the mean results of a considerable number of specimens are nevertheless of some interest. Thus the average amount of interspaces in fifty samples of Oolitic limestones is 32·2 per cent. of the solid material. In the Magnesian Limestone it is 19·2, and in the Carboniferous Limestone it is 11 per cent., whereas in the Carrara marble it is only 0·7 per cent. These numbers agree fairly well with the conclusions deduced from the clays.

In the case of sandstones my results are, for the cavities in the New Red Sandstone 66 per cent. of the solid material; for the sandstones of the lower part of the Coal-Measures and the Millstone Grit 25 per cent.; and for the Old Red Sandstone of Herefordshire 45 per cent. These results agree in a rough way with the others, but the differences in the depth of the water and in the mineral constitution make them valueless for calculating pressure.

In conclusion, I may say that a number of my numerical values must be looked upon as only approximate, though probably of the true order of magnitude. More accurate results would often require complicated experiments on a large scale, which could be carried out only in a specially-organized laboratory, or in a small clear river with artificial arrangements to control the current.

EXPLANATION OF PLATES XIV-XVIII.

[Plates XIV-XVI are slightly-reduced photographic reproductions from the slates, and the other plates are reproduced from my own drawings.]

PLATE XIV.

This shows the breaking-up of a fine-grained deposit soon after deposition, while still in a semi-liquid condition. (Green slate, Langdale.) See p. 197.

Plate XV.

This shows ripples with not much drifting forward, rapid deposition, and slight breaking-up of the fine-grained material, while still somewhat soft. (Green slate, Langdale.) See p. 197.

Plate XVI.

In this is a thin layer of ripple-drift, over which is a fine-grained deposit, very little broken up by the current bringing the coarser material, as if partly consolidated. (Green slate, Langdale.) See p. 198.

Plate XVII.

Fig. 1. Oolite from Grantham, showing an amount of empty spaces nearly equal to the theoretical minimum. (Magnified 50 diameters.) See pp. 206–207.

2. Coralline Oolite from Scarborough, in which the interspaces, some filled with calcite, are nearly of the same volume as when recently deposited with little shaking. (Magnified 50 diameters.) See p. 209.

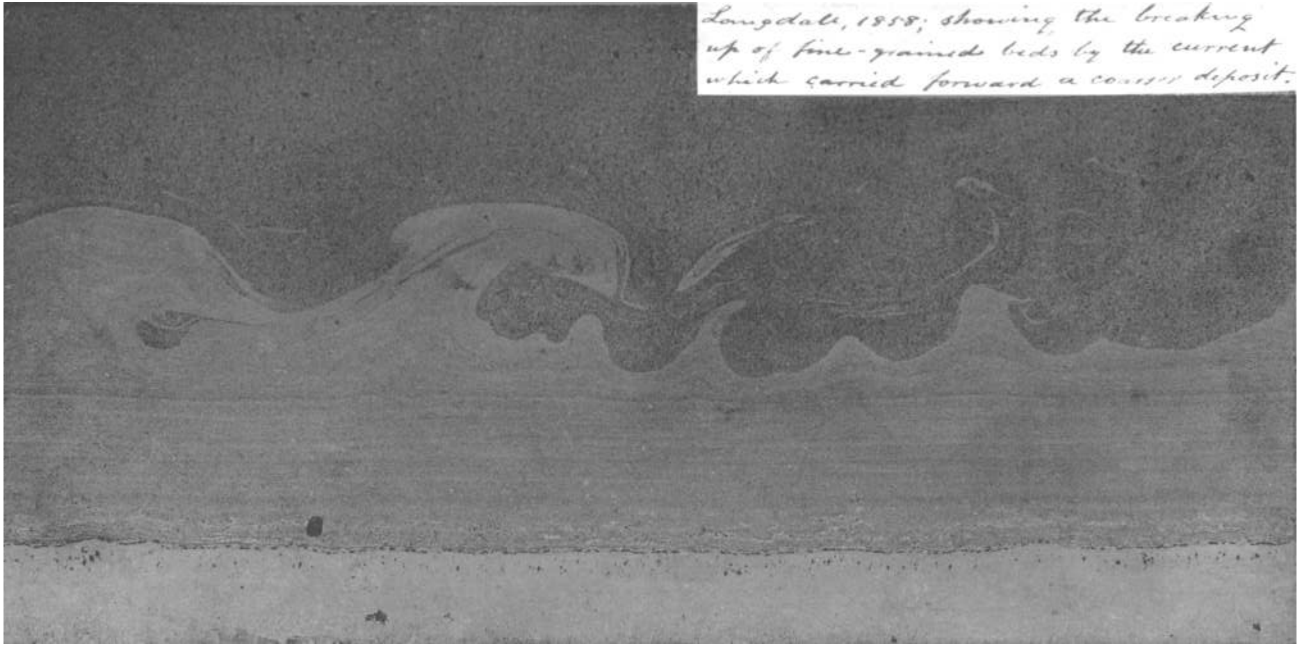
Plate XVIII.

Fig. 1. Wenlock Limestone from Easthope, showing angular fragments much squeezed together. (Magnified 25 diameters.) See p. 209.

2. Carboniferous Limestone from Bristol: an unusual example of oolitic grains considerably influenced by pressure-solution. (Magnified 50 diameters.) See p. 226.

Discussion.

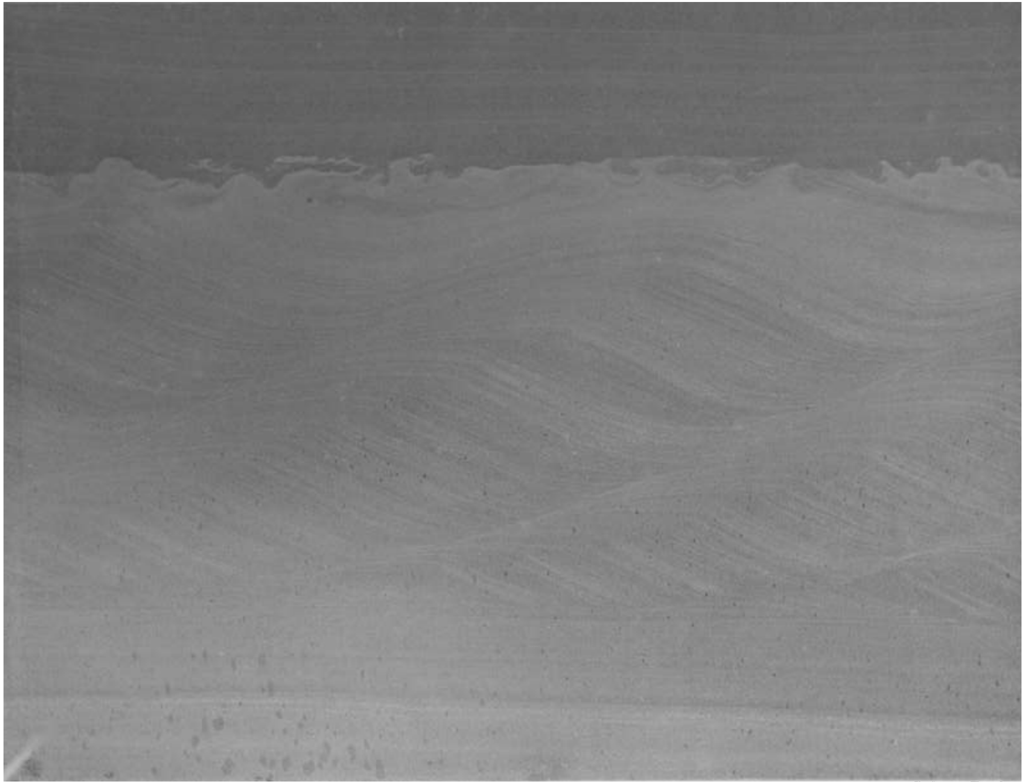
The President observed that any paper coming from their veteran comrade, Dr. Sorby, could not fail to be full of suggestion and to be marked by that wealth of experimental detail and sagacious calculation for which all his scientific writings had been distinguished. The present communication was the result of many years of experiment and reflection, although the Author was now confined to his room and unable longer to continue the prosecution of active research. It was probably safe to say that, although only his own brief summary of results had been read to the meeting, the paper when published would be found to mark a new starting-point for the investigation of the origin, history, and chronology of rocks. An interesting point of connexion could be noticed between the two papers communicated to the Society that evening. Dr. Wright, while lessening the length of the unit in the geological time-scale, still dealt with a period of several thousand years. It would be remembered that Baron Gerard de Geer, as the President had recently announced, had discovered, among the marine deposits that followed the retreat of the ice-sheet in Sweden, a remarkable repetition of distinct layers which he interpreted as indicating the succession of seasons in a series of years. And he was believed to have lately detected among the deposits of the inland Glacial lakes a similar series of apparently-seasonal deposits. But Dr. Sorby, in the paper of which they had heard an abstract, thought himself in a position to speak confidently of the number of minutes which



Langdale, 1858; showing the breaking up of fine-grained beds by the current which carried forward a coarser deposit.

Bemrose, Collo., Derby.

GREEN SLATE, LANGDALE, SHEWING THE BREAKING-UP OF A SEMI-LIQUID DEPOSIT.



Bemrose, Collo., Derby.

GREEN SLATE, LANGDALE, SHEWING RIPPLE-DRIFT WITH GENTLE CURRENT AND RAPID DEPOSITION.

QUART. JOURN. GEOL. SOC. VOL. LXIV, PL. XVI.

GREEN SLATE, LANGDALE, WITH MORE CURRENT AND SLOWER DEPOSITION.

Bemrose, Collo., Derby.

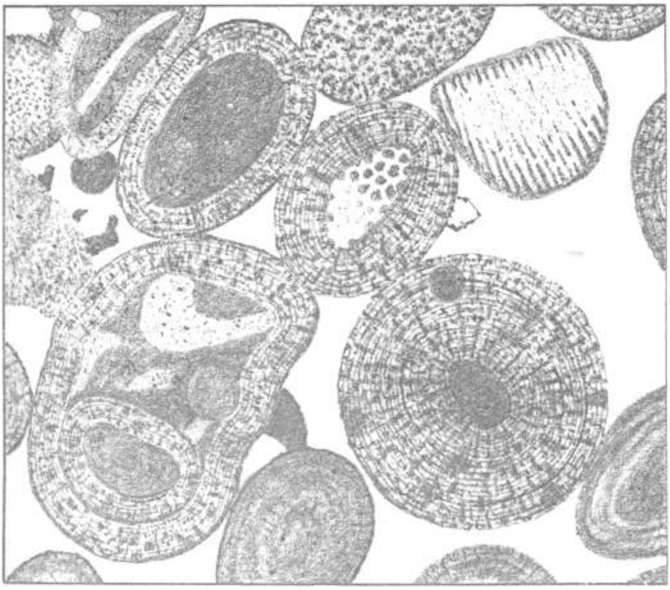


FIG. 1.—OOLITE, GRANTHAM, × 50 DIAMS.

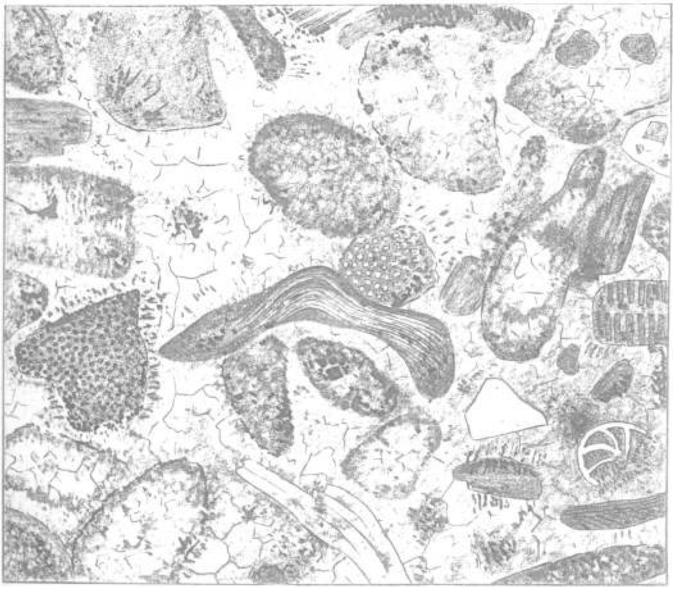


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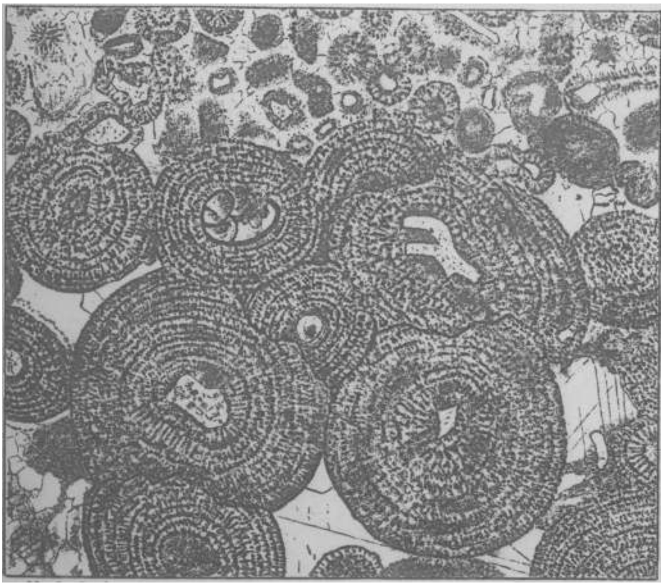
FIG. 2.—CORALLINE OOLITE, SCARBOROUGH, × 50 DIAMS.

WENLOCK LIMESTONE, EASTHOPE, × 25 DIAMS.



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CARBONIFEROUS LIMESTONE, BRISTOL, × 50 DIAMS.

certain layers of sediment had taken for their deposition. The geological world would await with impatience the publication of his full paper, and the Society would meanwhile return to him its hearty thanks for having honoured it by communicating so important a memoir, and would express the earnest wish that he might still live to complete the preparation of other papers embodying the result of his long years of quiet work in experimental geological dynamics.

Prof. JUDD, while pointing out that it was impossible to discuss (and much less to criticize) the paper from the abstract, directed attention to its highly-suggestive character. Like all the work of the Author, its value consisted not only in the conclusions arrived at by him, but still more in the indication of new methods of observation, and novel lines of reasoning, which could not fail to have an important influence on the future development of geological science. He was sure that all present united in sympathy with the Author in his enforced absence from them; in admiration for the energy with which, despite all difficulties, he continued his researches; and in the hope that they would receive still further contributions to science from his hand.

Mr. E. A. MARTIN wished to call especial attention to the allowance made by the Author for contraction in some of the older rocks. Estimates had been made of the Earth's age, according to the number of feet of total thickness of existing strata; but no allowance had in such estimates been made, as a rule, for compression. An attempt in this direction was made in the 'Geological Magazine' for August 1907, but few would have anticipated that the older rocks must be allowed to have contracted to 20 per cent. of their original thickness, or even in the case of the Chalk to 45 per cent. The publication of the paper would be awaited with very great interest.