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MOVEMENTS OF THE EARTH'S CRUST.*

(Bradyseisms, Earthquakes, Diurnal Waves, Tremors.)

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When we reflect upon the attention devoted to the observation of heavenly bodies, the changes that take place in the atmosphere, and the tides and currents in seas and oceans, it seems remarkable that so little has been done for the study of the movements of the so-called *terra firma* on which we live, and in building foundations to carry instruments which measure and record phenomena which take place above and round about.

If we except earthquakes, which from time to time attract a momentary attention by their violence, the reason that the more universal movements which are in operation beneath our feet receive so little attention is probably that they are not appreciable to our feelings. We cannot see a coast-line sink or rise, but that such things have happened, and that they may yet be in operation, is a deduction based upon a variety of evidence. We do not see tall buildings slowly moving to and fro with a diurnal period; nor do we perceive by any of our senses that there are times when certain instruments behave as if the foundations on which they rested were spasmodically breathing. Although a physicist may tell us that on a certain day a change in level is in operation, our knowledge of this, like that of many other earth-movements, depends upon hearsay.

While saying something about earthquakes, the object of the following short paper is to indicate the character of movements which are either too slow or too small to note with our unaided senses, and to

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point out the scientific and practical advantages which result from their study. We shall refer to them in the order of their relationship to each other—those with an orogenic origin, like bradyseisms and earthquakes, being taken first; whilst those like diurnal waves and tremors, the existence of which may depend upon meteorological conditions, are taken last.

BRADYSEISMS.

The first movements to be touched upon are the *slow* or bradyseismic motions of the Earth's crust which, relatively to sea-level, result in elevation or depression. The manner in which these are produced by a shell too weak to be self-supporting, folding and crumpling as it accommodates itself to a shrinking nucleus, is a theory sufficient to explain the existence of mountain ranges, the uplifting of strata which were once beneath the sea, and many of the more important features of the lithosphere. We shall treat of these movements in relation to continental development, geological history, and to what has been observed to occur during historical periods and a lifetime.

When studying the evidences we have of bradyseismal changes, the first inquiries relate to their magnitude, their form, and the rapidity with which they are performed.

As it is usually only the vertical component of these movements which is measured, and this relatively to ocean-level, a debatable point that presents itself relates to the fixity of this datum. We know that the oceans form a body of water always moving towards a position of equilibrium; that much has been written to show that by absorption a general lowering of level is in operation; that by sedimentation and the release of aqueous vapour from heated rocks, a movement in the opposite direction may be taking place; that by alterations in the position of the Earth's centre of attraction, and by other causes, there may have been changes in the configuration of the oceanic envelope. But putting these considerations on one side—partly because they result in long-continued uniformity of change in one direction—we may ask ourselves how far secular movements have resulted in real elevation or depression, and how far they are only apparent movements due to the rising or falling of the water.

The phenomenon we wish to consider is similar to that which takes place when we raise or lower an apple in a basin of water. When it is lowered the water rises, when it is raised the water falls, and the volume of the immersed solid is equal to the volume of the displaced fluid. If the rim of the basin was flat or saucer-like, over such a surface there would be large horizontal displacements of the water-line.

Applying such ideas to what may possibly have taken place during the growth of the habitable portion of the world—a great part of which we know was at one time beneath the surface of the waters—and assuming that there was at some particular period in the world's history a universal, or nearly universal, ocean, if we have not considered the matter before, we reach a somewhat unexpected result. Because we know the volumes and areas of the oceans, and the areas and average heights of the continents above sea-level, if the latter have been raised above the former when it extended over the whole globe, then out of an apparent average elevation, usually estimated at about 1000 feet, 250 feet of this would be due to the recession of the waters. A measure of the *area* of existing land surface which may in this manner have been only apparently elevated, may be realized by noticing the area which would be covered if the present sea-level could be raised 250 feet.

Regarding the evolution of continental areas in this manner, we see that the retreat of the ocean may have played a part quite commensurable with that attributed to bradyseismical movement. Should we dismiss, as being improbable, the idea that at any period of continental development there has ever been an almost universal ocean, and only consider the elevations which have taken place within the range of geological history, we still see that secular movements have been accompanied by considerable changes in ocean-level, and still greater in ocean-area. In discussing this question, the *à priori* assumption is, that when any area rose above the waters, it is unlikely that there should have been at the same time, in some other region, a subsidence beneath the water, and even if such contemporaneous but opposite movements had an existence, it is still more unlikely that they should have been volumetrically equal. Granting this hypothesis, as an example of what is likely to have occurred at times well within the limits of the geological horizon, we may take the two great mountain-forming epochs in the world's history. About the close of the Palæozoic era, the Urals in Europe, the Himalayian-like Appelachians, and other mountain ranges were formed.

In early Tertiary times, the Alps, the Pyrenees, the Carpathians, and the Himalayas were growing upwards, whilst a series of islands were slowly uniting to form the Apennines and the Italian peninsula. The complexity of the folds and the faulted and contorted strata out of which these masses are built, taken in conjunction with the step-like arrangement of sea beaches and terraces, suggest the idea that processes of elevation are intermittent. As these mountains were slowly growing, relatively to the Earth's centre, certain tracts of the rocky crust would rise, others would fall, and there would be a universal contraction in oceanic areas.

With a ratio of land and water areas in early Tertiary times of 1 : 4, and a mean height of the mountains then elevated of 4000 feet, which is the present mean height of Switzerland, the vertical fall in ocean-level would be about 26 feet. This calculation of the fall in the waters does not include that which would accompany the rise of the extended fold or dome on which the mountains stand, and the figure given is therefore probably much below the truth. The meaning of this retreat of the waters in relation to the history of oceanography is, that at the times when mountains were formed, an intermittent withdrawal of waters, which would be marked on shallow coasts, took place throughout the world, and extensive tracts would be bared, not by the rising of the land, but by the falling of the waters.

Although mountain ranges might, as they rose upwards, increase their distance from the Earth's centre, flanking regions, by approaching this centre at a greater rate than that at which the waters were falling, might be submerged. Accepting the idea of rapid sedimentation at certain points along these intermittently sinking coast-lines, and we have conditions for the accumulation of a series of strata such as are found in many coal-fields, and it is certainly remarkable that these two periods of mountain growth were closely coincident with two great periods of coal-formation. If we do not accept this suggested connection between mountain growth and the deposition of strata accompanying the occurrence of the formation of coal, not only have we to fall back upon the usual explanation that the coal-measures were formed when there was a nicely adjusted balance between the rate at which an area was depressed and the rate at which the same was rising by sedimentation, but we are left to explain why such an adjustment was only marked at two particular epochs in the Earth's history, and why it simultaneously occurred at so many points upon the surface of the globe, which, for carboniferous coal at least, lie between 20° and 60° N. lat. and 20° and 40° S. lat., in regions of bradyseismical action.

Although a relationship might be traced between pronounced secular movements, volcanic action, and other geological phenomena, the main object of the present section of these notes is to show that, although we may take sea-level as a datum relatively to which changes taking place within the historical period may be approximately measured, directly we attempt to measure the magnitude of upheavals with which the geologist has to deal, unless we make our datum at the Earth's centre, or at some fixed distance from the same, it seems possible that some of our results may exceed the truth by 25 per cent.

In Japan, as in many other countries, notably perhaps Italy, bradyseismical movements have, during historical times, taken place with great rapidity. With the assistance of Prof. D. Kikuchi, of the Imperial University of Japan, a few years ago, circulars were sent to officials at all the important seaports and villages round the coast, asking them to collect evidences whether, during recent times or within the memory of the oldest inhabitants, any changes had taken place in the relative positions of the sea or land. The replies, which were numerous, showed that, along the eastern side of the country in particular, elevation had been in progress, this being particularly noticeable in districts where earthquakes were frequent.

Information gathered in this manner, taken in conjunction with the writer's repeated observations on sea-terraces, shell-borings in rocks well above high-water mark, and other appearances, it may be concluded that movements along certain portions of the Japanese coast have been exceedingly rapid—possibly an inch per year. At one point, where soft tuff rock projects into Yedo bay beneath the bluff at Yokohama, lines of shell-borings can be seen fully 10 feet above high-water mark. Because the rock is so friable and soft, it is difficult to suppose that these markings have been exposed to atmospheric influences for a length of time very much greater than fifty years. Be this as it may, the writer well remembers that nineteen years ago it was impossible to pass round a neighbouring point of similar rock, whilst now at low water it may be walked round, not over drift or shingle, but over a rocky surface. There are no reasons for supposing that these changes in the relative positions of land and water on the coast of Japan which are here referred to, have taken place suddenly. It must, therefore, be supposed that they have either been brought about by the gradual elevation of the land, or by the gradual withdrawal of the water. The Japan current, or Kuro Siwo, which flows up the eastern coast of Japan, and which is comparable with the Gulf Stream in the Atlantic, has, like other oceanic circulating systems, marked seasonal changes in its velocity, and the distance to which it is appreciable.

Although we have no definite data respecting the greater changes which may take place in the magnitude and velocity of ocean currents, owing to gradual but periodic changes in climatic conditions, possibly culminating in periods of glaciation, it is highly probable that such changes have an existence, and if they do exist, it is difficult to suppose that they have been unaccompanied by alterations in the general level of the oceans, and to some degree in variations in the amount of water held back in bays and estuaries, especially those which have narrow entrances.

Notwithstanding these considerations, the occurrence of earthquakes, the crumpled rocks intersected by numerous faults, and other phenomena, incline us to the belief that the eastern coast of Japan is growing in consequence of the movements of the rocky crust rather than by a recession of the waters.

Whether the measurement of these movements which bring about such rapid changes are phenomena of sufficient importance to attract the attention of harbour and other engineers, is a question that cannot be answered until they have been more accurately determined. The importance of forming a trustworthy estimate of the rate at which the larger folds of the Earth's surface have been formed, or by which mountains may be increased or decreased in height, is evident to all students of Earth physics.

The only experimental determinations of these changes are the well-known measurements which were made between marks cut upon certain rocks and sea-level in Sweden. The method, although apparently certain, has its objections. The fact that differences in level can only be observed after long intervals of time is in itself an objection, while the fact that during such intervals, by variations in ocean currents, by sedimentation, elevation or depression at the entrance to a closed sea, like the Baltic, along the shores of which the marks referred to were made, the play of the tides, relative to a phase of which the measurements must be taken, may have suffered alteration.

Results might possibly be obtained in a quicker and more certain manner by differential measurements of the records of three or more tide-gauges referred to neighbouring bench-marks. In a nearly closed bay, like that of Yedo, where the tide is small, it would be possible to choose times when tides had similar phases, and when we could assure ourselves that the surface of the water had the same configuration. Taking observations made at these times, although total rise relatively to a certain phase of tide may be measured, the chief determination would be the relative rising or falling of one point of land as compared with another, together with the axis of such a movement.

Several other methods of obtaining similar results suggest themselves, the simplest of all being the installation underground of several levels parallel and at right angles to the dip of strata. Under these conditions, where changes of temperature would be inappreciable, and where readings would only be required at long intervals, the ordinary spirit-level would be sufficient for the suggested purpose, but, to avoid effects due to the creeping of the soil, the levels should be placed upon the solid rock. With such an installation in Japan, where we know that movements are taking place with comparative rapidity, it is not unlikely that definite measurements of tilting could be obtained in a very few years.

From the observations of levels placed in a cellar, Plantamour has shown that there are long period displacements in the position of their bubbles. Astronomers observe slow changes taking place in solid masonry piers which carry telescopes, and von Rebeur-Paschwitz has recorded the wandering of the zero-point of his horizontal pendulums. In Japan it has been observed that two of these latter instruments, placed 1000 feet apart, have synchronized in the directions of their displacements, slowly creeping for ten or many more days in one direction, and then returning to travel past their starting-point in an opposite direction. Although these instruments may only show changes of 1 or 2 inches of elevation per mile, it is premature to assume that what has been recorded is due to secular changes in the inclination of the Earth's crust. Movements of this nature may be due to the gradual creeping of a layer of soil upon a slope, to the differential loading or removal of moisture on two sides of an observing-station, and possibly to other causes.

Therefore, until levels or horizontal pendulums have been established in duplicate in the suggested manner upon rock, we shall be unable to say that secular movement has been instrumentally measured or recorded.

EARTHQUAKES.

To pass from movements which are accomplished so gently and so slowly that they are neither felt nor seen, to violent disturbances called earthquakes, which in a few seconds may alter the superficial aspect of a country, although the transition is rapid, it is one that is natural.

The greater number of earthquakes may be regarded as announcements that a resistance to secular movement has been overcome, and if such an explanation of earthquake origin is sufficient, then the relationship of the former to the latter is that of a child to its parent. Wherever we find mountains which are geologically young, where the process of rock-folding may yet be in progress, there we find earthquakes. Should these regions of rock-movement be near a sea or ocean, we also find volcanoes. Volcanic eruptions accompany the generation of steam and gaseous pressure beneath lines of yielding; while earthquakes, if we except a few explosive efforts at volcanic foci, tell us that rocky strata, bending under the influence of terrestrial contraction, have exceeded their elastic limit. Although both may occur in the same country, it is seldom that their origins are close together. In Japan it is seen that active volcanic vents chiefly occur along the backbone of the country which forms the upper edge of a huge monocline, whilst the earthquakes are most frequent on the flanks of this fold, or where it sweeps steeply downwards beneath the deep Pacific. The home of the majority of earthquakes is that of the majority of faults, which is a region of monoclinal folding. That a volcano by its eruption acts as a safety-valve for the surrounding district, does not seem to be borne out by observation. Earthquakes and volcanoes are independent, excepting in their parentage, and if between them there is a closer union, we should expect that a relief of pressure following an eruption would result in farther bending of the strata, and an increase in seismic frequency. If it is, therefore, admitted that the majority of earthquakes are the result of fracture resulting from excessive bending, rather than attempting to predict their occurrence by our sensations, the behaviour of lower animals, or the assumption of tides acting on lines of weakness beneath a hardened crust, a more reasonable method of procedure would be to determine whether the earthquakes of a given district are preceded by measurable changes in inclination of the yielding surface. During the last few years of the writer's residence in Japan, with the aid of delicately adjusted horizontal pendulums, some attempt was made towards the solution of such a problem. The results showed that in a number of instances there was an indication of a connection between a uniform and steady change in level and the occurrence of many disturbances.

Because the observations were incomplete, and because there were difficulties in separating earthquakes of a local origin from those which had originated at a distance, the definite solution of this problem remains for future investigations.

From the records obtained in Japan from nearly one thousand observing-stations, we learn that during a period of eight years about nine thousand distinct shocks were recorded, and for each of these the approximate origin and the area that was sensibly shaken have been determined. The analysis of a catalogue of these disturbances shows that Japan may be divided into at least fifteen distinct seismic districts, the records from each of which may be examined separately, the different shocks being taken as numerically equal or have values given to them bearing a relationship to the area which each disturbed. The records from each of these districts give relative measures of the rate at which rock-folding is in progress in such districts, and we are enabled to examine how far these changes are influenced by tidal loads, variations in barometrical pressure, and other phenomena exogenous to the crust of our Earth.

From the study of a series of *after-shocks* which have followed several large disturbances, Mr. Ōmori has shown that, in particular cases considered, the rate at which the broken-up strata settled to a state of equilibrium practically followed the same law, from which it may be concluded that the character of the disjointed materials had been on the average fairly similar. One result of these investigations is that, having observed for a month or so the number of *after-shocks* occurring in a given district, and in this way determined the first part of a curve of frequency, this curve may be completed, and from it we can see how many shocks we may expect in a given number of months, or how many years it will be before the district has returned to its normal state of seismic sensibility.

If we take the records *en bloc*, the only advantage which the Japanese catalogue of earthquakes presents over other lists is its completeness, and the fact that the different records can have " weights " assigned to them. By examining these records, we find that for Japan, as for other countries and for the earthquakes of the world, there is an annual and semi-annual periodicity, the former of which, as Dr. C. G. Knott and Mr. C. Davison point out, may be the result of differences in barometrical stress at two seasons of the year.

For very many of the shocks recorded in Japan, seismographs, writing on stationary or moving surfaces, have given diagrams from which we learn how many vibrations have constituted a given disturbance, the rapidity with which they have occurred, and the extent and direction of each successive movement. The study of these diagrams has led to the conclusion that earthquake-motion, which may be *felt* for periods varying between a few seconds and several minutes, is of different types, and that each type has a signification. If, for example, we feel a long, easy, rolling motion, at which time the diagram is that of a series of slowly recurring waves, the inference is that the origin of the disturbance causing this movement is at a considerable distance. If, however, an earthquake commences with a series of small but rapid vibrations, followed by a shock or shocks, and irregular sharp motions which die away gradually, then the origin is near, and it appears that the difference in time between the commencement of the tremors and occurrence of the first decided motion is proportional to the distance of the origin from the observing station. The interval of time between the commencement of preliminary tremors and the more violent movements, as shown by an ordinary seismograph, when the distance between the observing-station and the origin of the disturbance is not more than 200 miles, seldom exceeds ten seconds; but if the movement has been propagated to a very long distance, the interval may be many minutes. Should, for example, an earthquake originating in Japan be sufficiently intense that, by means of specially contrived instruments of great sensibility to elastic vibrations and angular displacement, its movements can be recorded at places distant one quarter or more of the Earth's circumference, then it is seen that the tremors appear upon the diagram half an hour before the maximum phases of motion. These latter motions are propagated to these distant places at a rate of two or three kilometres per second, or at about the same rate as they are propagated to places relatively close to an origin. The former, however, assuming that they actually originated at the same time from the same origin as their larger followers, have been transmitted at rates three or four times as quickly, and it becomes difficult to suppose that they have passed through the heterogeneous materials constituting the Earth's crust. To make careful observations on the rate of propagation of these tremors, and to determine the paths they have followed, are two of the most important problems which the seismologist is now asked to solve. Their velocity is somewhat reduced if it is assumed that, instead of passing round our Earth, they pass in straight or curvilinear lines *through* the same; but even with this assumption, we find that the rate of transmission has been greater than it would be had our globe the same constitution as glass or steel.

Our knowledge of the interior of the Earth is at present chiefly based upon the revelations of the thermometer and the plumb-line. Whatever information these may have given us, it does not seem improbable that after a seismic survey has been established for our world, the records of seismographs may add definite information about the effective rigidity of our planet, which, from the little that has already been accomplished, seems to be very much higher than has usually been supposed.

Although many of the investigations relating to earthquakes have apparently only yielded results of value to those engaged in researches of a purely scientific nature, yet a certain number of them have led to results of practical utility.

From the diagrams of earthquakes, the maximum velocity of a particle and the acceleration or suddenness of motions which are the destructive elements of earthquake movement resulting in projection, overturning, and shattering, have been calculated. That these quantities are real has been verified by numerous experiments, in which columns of brickwork and other bodies have been moved back and forth by a recorded motion, until, by the reaction of their own inertia, they have either been fractured or overturned. The result of these experiments and observations in the field has established the truth of calculations based upon diagrams, and if, after a destructive earthquake, we determine the amount of motion that has been experienced, either from the dimensions and nature of bodies which have been overturned, or from the records obtained from seismographs, we arrive at what are practically identical results. In the great earthquake which, on October 26, 1891, devastated Central Japan, at which time nearly ten thousand people lost their lives, the greatest accelerations recorded varied between 3000 and 4000 millimetres per second per second. Earthquakes which shatter chimneys, partially unroof houses, cause plaster to fall, and which give to an ordinarily built town the appearance that it might present after a bombardment, have a suddenness in their back and forth motions of about one-tenth of the above quantities. From these and other measurements, an engineer, having assumed that a certain quantity of motion may be expected, is now in a position not simply to make a structure strong because an earthquake is strong, but he is able to proportion and distribute his materials to exactly fulfil certain possible conditions. For example, in the building of piers to carry a railway bridge, we know from experience that, when they are sufficiently shaken, fracture first takes place at their base. This form of destruction is identical with that which occurs when a column of brickwork is placed upon a truck which can be moved back and forth at a continually increasing rate. At the same time it is in accordance with what we should expect, which expectations are borne out by the results of theoretical investigations. From observations and considerations like these, we see that walls, columns, and structures like piers and chimneys, require greater dimensions or strength near their base than those given to them in ordinary practice. Forms having equal strength at all their horizontal sections against the effects of horizontally applied motion have been designed; and in Japan, as illustration of these, we have the parabolic brick piers, some of which are 110 feet in height, designed by Mr. C. A. W. Pownall, M.INST.C.E., to carry the Usui railway.

One remarkable observation which seismographic records have confirmed and measured is the fact that, outside an epifocal area, earthquake waves like those of the ocean are somewhat greater on the surface than

carefully analyzed by the late Dr. E. von Rebeur-Paschwitz. In Japan, photographic records of these displacements have been obtained at many stations.

The most pronounced movements correspond to a slow tilting of the instruments for ten or twelve hours towards the east, followed by a retrograde motion towards the west. Accompanying this there is a north and south component of motion, which is definite but relatively small.

Von Rebeur found, after a careful analysis of his records, that whatever may have been the chief cause of these displacements, which amount to three or four seconds of arc, but at certain stations in Japan to several times this amount, a slight superimposed lunar effect may be detected.

The records of these movements, like those of tremors and unfelt earthquakes, are obtained by continuously photographing on a moving film the varying position of a horizontal pendulum. When the pendulum remains at rest the diagram is a straight line, but if, during a period of twenty-four hours, it wanders slowly to the right and then back to the left of its normal position, the photogram is a wave-like curve, the amplitude of which varies according to the sensibility of the instrument and the locality where it is installed, from 1 or 2 up to 20 millimetres. In Japan, the localities where pendulums were installed were selected for the purpose of studying these movements under varying conditions. Five sets of diagrams were obtained from instruments placed upon the solid rock in caves, where the daily change in temperature and hygrometric conditions was barely appreciable. At these stations it was observed that the movements due to earthquakes were most pronounced in a direction parallel to the dip, suggesting the idea that this direction is that of least resistance, parallel to which there is a concertina-like yielding in the folded strata. Together with the displacements due to the diurnal wave, the pendulums showed that they slowly wandered, moving several days in one direction, and then returning towards their starting-point. Tremors and daily waves were not visible. Had the sensibility of the pendulums, which, in the experiments in Japan, never exceeded a displacement on the film of 1 millimetre for an angular tilting of 0·1″, been greater, it is possible that the latter phenomena might have been observed. In Europe similar or corresponding instruments have had sensibilities given to them of 0·01″ to 0·003″. The latter degree of sensibility means that changes in inclination of 1 inch in about 1000 miles would be measurable.

Another station was in an underground chamber in the alluvium, where the daily changes in temperature, as in the caves, was extremely small, but where, in consequence of fairly good ventilation, there may have been considerable changes in hygrometric conditions. The depth of this chamber was 12 feet. The records showed wandering of the pendulums, tremors, and daily waves, all of which were at times quite as pronounced as they were at stations on the surface.

These latter records lead us to the conclusion that diurnal waves are not to be attributed to the immediate effects of any change in temperature close to the instruments.

The remaining stations, where the instruments stood on brick columns with concrete beds, were on the alluvium, which near Tokio forms a layer of reddish stiff soil 50 to 100 feet in thickness, above a soft grey clay-like tuff rock. A locality at which the diurnal wave was hardly appreciable was in a wood. At this place there were high trees for some distance all round the hut in which the pendulum was placed, which protected it from the direct effects of the sun. On an open plain, where on one side of the station there was ploughed ground and on the others green corn, the movements were slight but fairly regular. They apparently indicated that during each day the ground covered with corn rose relatively to that which was bare. At all the other stations where the ground was covered with trees or buildings more upon one side than upon the other, the daily waves were large, and often differed in phase. For example, on two plateaux on the two sides of an open swampy valley, the instrument on one side of this having a protected area on its east side, and those on the other side a somewhat similar area on the west, moved at the same time in opposite directions—that is to say, if the trees on these two opposite scarps followed the movements of the instruments, it may be concluded that on each fine day they bowed towards each other.

A very important observation made at many stations was that *on wet and cloudy days diurnal waves were absent.*

The general conclusions to which the various observations point, is that the movements which take place during the day are due to the removal of a load from the side of the station most exposed to the effects of radiation, and in the alluvium this effect is quite pronounced to a depth of at least 12 feet. This load may be represented by aqueous vapour carried upwards and then dissipated.

On a fine day experiment shows that from moist open ground as much as one ton of moisture may be removed from an area 20 yards square. If an action of this sort is more marked upon one side of an instrument than upon another, then the ground by its resilience rises on the former side, and the pendulum swings away from the area which has been most relieved. In a similar manner, a load composed of men and boys marched up to one side of the station causes the ground on that side to sink, and the movement of the pendulum is towards the loaded side; when they retire, the pendulum returns to its normal position. For reasons like these, the instruments on the two opposite plateaux during the day move in opposite directions, or away from the intervening open valley, where unloading is most marked; while an instrument on uniformly exposed ground or in a wood shows but little or no motion. That there is little or no motion during rainy or cloudy weather, is evidently due to the fact that at such times there is but little evaporation.

A desirable experiment would be to record the movements of a horizontal pendulum on a uniform and equally exposed prairie-like plain. In this case, because during the morning the sun would take away more moisture from the east than from the west side of the pendulum, until noon or about two in the afternoon, we should expect to find the motion westwards. After a pause, some time in the afternoon, a retrograde movement would set in and continue until some time after sunset, but both movements would be small.

From this we see that a pendulum which has been moving westwards may have the direction of its motion reversed; but the difficulty which presents itself is to explain the manner in which this retrograde motion, having been established, continues during the night, until it nearly returns to the position it had on the previous morning. The explanation of this phenomenon apparently rests on the fact that during the night the area which during the day has lost the greatest load may be the one which gains the greatest load. The establishment of this suggestion depends upon facts and experiments relating to the precipitation and condensation of aqueous vapour. On open ground where radiation is marked, there is a greater precipitation of moisture in the form of dew than there is upon ground which is protected by trees, buildings, or other coverings. The quantity of moisture which is in this way drawn from the atmosphere, or, as Aitken has shown, is trapped just as it rises from the ground, is however small, and cannot be taken as more than one-eighth of that which during the day has been moved by evaporation. Although it may assist in causing the reloading for which we seek, by itself it is insufficient to explain all that is observed; and, further, the retrograde motion of a pendulum may have taken place on nights when dew is not observable. On such occasions, however, a differential loading of two neighbouring areas may have been brought about either by the condensation of moisture on the immediate surface of the ground, beneath leaves and stones, or actually beneath but near to the surface.

If we look beneath a board which has been lying all night upon the grass, it does not unfrequently happen that its under side is wet with moisture, although the grass around may be dry. This observation suggested the idea that just as moisture is condensed beneath a board, a leaf, or a stone, so it may be condensed *in* the ground, within one or two inches of the actual surface. On a hot day moisture is evaporated from soil, and this is perceptibly heated to a depth of about a foot. Shortly after sunset the surface to a depth of one or two inches is chilled, or in winter it may be frozen. The result of this is that moisture rising as vapour, and by capillarity from water-bearing strata below, is condensed on the under side of the chilled surface. To determine how far superficial soils gain in weight by an action of this description, independently of moisture precipitated from the atmosphere, or condensed as it rises from the ground, the following experiments were made.

Two boxes, each 1 foot 6 inches square, and about 2 inches in depth, were balanced on the extremities of beams carried upon knife-edges. One box had a bottom made of tin, and the other of fine wire netting, and each was filled with earth. Excepting when these were weighed by hanging weights at the other extremities of the beams, they were allowed to rest upon a bed of soft earth. Sometimes it was found that during a night both of the boxes would lose weight, but at other times it was found that the weight of the box with the tin bottom had not changed, whilst the one with the wire netting had gained from 2 to 2·5 ounces, which apparently showed that there had been a condensation of moisture coming up from beneath of 10 ounces per square yard, or about one-eighth of that which had been removed during the day by evaporation.*

Although the retrograde motion of a pendulum during the night is usually less than that which had taken place during the day, thereby causing a creeping of the zero point away from an area of rapid evaporation, the question is whether the precipitation and the forms of condensation which take place between sunset and sunrise are sufficient to produce the observed effects. Our data respecting sub-surface condensation are unfortunately confined to the few observations made by the writer, and many observations have yet to be made before its quantitative analysis has been completed. The greatest effect would be produced when each of these forms of condensation was at a maximum and acted in conjunction.

The difference in the weight of moisture evaporated from two neighbouring areas is apparently sufficient to cause the movements observed during the daytime, whilst the difference in the weights added to such areas from a water-supply common to each, is a phenomenon which may influence the movements observed at night.

These explanations, although they may be sufficient to account for many of the movements observed in Japan, because instances occur where their application is not clear, until they have received more careful attention, can only be received as provisional. At Potsdam, for example,

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* In consequence of a fire which occurred at the writer's house, not only were the notes relating to these experiments destroyed, but the experiments themselves had to be terminated. Sub-surface precipitation is evidently closely related to a fact noticed by farmers upon the chalk downs in the Isle of Wight, who find that if the ground is cleared of flints, which means the depriving it of its radiators and condensers, its fertility is impaired.

where Dr. E. von Rebeur-Paschwitz tells me that the ground is equally covered with trees towards the east and west, the motions are quite pronounced, and the same authority has observed extraordinarily large motions on a strongly founded pillar, when there was apparently no motion in its neighbourhood.

An experiment which has yet to be made is to place an instrument upon a pillar, the two sides of which are unequally porous, with the object of determining whether any of the observed movements, especially tremors, which are described in the next section, can be traced to changes taking place in the supporting column.

NOTE.—From the researches of S. H. Miller, F.R.A.S. (Prize Essay on Evaporation, published by the Utrecht Society of Arts and Sciences, 1878), it would appear that the average daily quantities of evaporation for *particular* months are approximately as follows:—

Soil (humus; July)	4,243 lbs. per sq. yd.
Water (July, 1869)	8,613 " "
Forest (a spruce)	12,528 " "
Grass (red clover; May)	15,613 " "

These loads, which are removed during twelve hours of daylight, may be either greater or less than the quantities given. They are practically in the ratio of 1 : 2 : 3 : 4.

The greatest daylight displacement of a horizontal pendulum ought, therefore, to be expected when such an instrument was placed on the boundaries of two areas respectively covered with soil and grass. On a fine day the differential evaporation effect on the two sides of the instrument would be equivalent to removing a load of about 2½ tons per 20 yards square from the ground covered with grass, which is quite sufficient to produce many of the observed effects.

EARTH-TREMORS.

The last class of earth-movements to which we have to refer are even more perplexing, and more frequently observed than the diurnal waves.

These are the earth-tremors, earth-pulsations, or microseismic motions, which were first systematically observed by Father Timoteo Bertelli of Florence. The method that Bertelli adopted prior to 1870 —which was to observe, by means of a microscope, the movements of the style of a pendulum carefully sheltered from air-currents—is practically identical with the methods yet employed at very many stations throughout the Italian peninsula.

Amongst the more important results obtained by these observers, and confirmed by observations made in other countries, is that tremors were more frequent in winter than in summer, and when the barometer is low rather than when it is high.

In Japan during the last nineteen years these movements have been studied by the use of a large variety of instruments, whilst for some years past the records have, by photographic or other means, been made continuous.

The analyses of a long series of these latter records have shown that tremors were usually at a maximum when the barometric gradient was steep, no matter whether, at the place where they were observed, the barometer was high or whether it was low. From this it may be concluded that when tremors are pronounced, somewhere or other, either at or within 100 or 200 miles distant from the point of observation, a strong wind may be blowing. This conclusion, which has been verified by an appeal to weather charts, led the writer at one time to regard local or distant winds as the immediate cause of microseismic storms. Although the mechanical action of wind upon buildings, trees, and the surface of the country may produce slight tremors and influence the character of a record, the fact that tremors are sometimes observed at the time of a dead calm around the station, and that they may not be visible when the building in which the instrument which renders them visible is being violently shaken during a heavy storm, has led observers to think that tremors are not the immediate effects of wind. The reason for the existence of tremors was next sought for in fluctuations in barometric pressure. During a typhoon the needle of an aneroid may be observed to pulsate back and forth, whilst the slow change in pressure over an area may, as von Rebeur has shown, produce sufficient alteration in level to be recorded by movements of a horizontal pendulum. When barometrical pressure is changing rapidly over a district, the parts of which offer different degrees of resistance to distortional effects due to varying pressure, the inference is, that such a district might be thrown into a state of irregular agitation sufficiently pronounced to cause the movements called tremors. Although tremors may perhaps be induced by causes such as these, and therefore be really the result of actual movement of the ground, a close inspection of photograms, and a knowledge of the localities where they were obtained and the character of the instrument which yielded the records, give rise to a suspicion that some of these intruders, which may mask effects due to distant earthquakes, and interfere with many delicate physical operations, are, in some cases at least, the result of movements of the atmosphere.

The instruments in the caves, where it was uniformly damp, and many of those situated in huts upon the surface, where the ventilation was so good that it was difficult to understand why the long booms of the horizontal pendulum had not been set in motion by currents of air, either never showed a trace of these movements, or, if they were shown, they were insignificantly small. With other instruments, some of which were well protected inside substantial buildings, and the possibility of their being affected by currents of air from the outside was inconceivable, day after day tremors were pronounced. The difference between these two sets of installations was, that the pendulums showing tremors were either longer in proportion to their weight, or actually very much lighter than those which remained steady; and experiments showed that the smaller and lighter a pendulum was, the more closely it approached in character to the imaginary instrument endowed with perpetual motion. The conclusion derived from this observation was, that the so-called Earth-tremors might in some way or other be due to air-currents produced within the containing cases; but there was nothing making it likely that such currents were due to any marked difference in temperature between the parts of an instrument and its enclosing walls.

An experiment which tended to confirm this view was to place a tray of calcium chloride inside the case containing a horizontal pendulum, when it was observed that the pendulum commenced to swing. The meaning of this is that air-currents are produced by rapid desiccation, which is a cause of atmospheric circulation, which has a wide application, but which is seldom emphasized.

Assuming this explanation for the origin of *certain* so-called Earth-tremors to be correct, great difficulties occur when we attempt to explain the peculiarities they exhibit in the times of their occurrence.

If a tremor-storm extends for several days, it is noticed that maxima of movement occur during the night, or from 9 p.m. until 6 a.m. As the disturbance dies out, tremors will only be observed at these hours, until finally the disturbance will end with a little movement about 6 a.m. Some instruments have only shown movements about daybreak, whilst one showing a maximum motion about this time was always on the swing at night (Fig. 9). It will be observed, therefore, that tremors are frequent during the time that the portion of the diurnal wave is most gentle, and that the maximum of tremors occurs about the time when the reversal in the direction of the diurnal swing of a pendulum takes place. Although experiment has shown that the appearance of tremors can be produced by the removal of a heavy load at the distance of 100 feet from a pendulum, at which time it slowly heels over towards the relieved area, because they are often frequent when no daily wave is observable, it does not seem likely that there is any direct connection between these two phenomena.

The conclusion at which we therefore arrive is, that during the night, and especially about daybreak, in consequence of the absorption or giving-off of moisture within more or less closed cases, and possibly to a greater extent outside them, air-currents are produced which are sufficient to move light pendulums, and that some of these movements have been supposed to be caused by an actual movement of the ground.

By no means can all tremors be explained in this simple manner. For example, the horizontal pendulum, now working at Shide, which is usually adjusted to have a period of seventeen seconds, often shows tremors extending over many hours, which have periods of from one to three minutes. It may be possible that air-currents have produced these movements, the character of which indicates a slow tilting of the instrument; but it seems unlikely, especially when it is seen that for several hours there has been no variation in the period or amplitude of the displacements (Fig. 7). The bulk of tremors seem to have their origin in causes which are meteorological; but we are yet waiting for a more complete explanation of the manner in which they originate.

Although we see in diurnal waves and tremors the results of meteorological phenomena which affect agriculture and forestry, the movements they represent usually present themselves as intruders which have not simply interfered with, but which have hopelessly destroyed certain physical operations.

Tremors brought investigations relating to lunar gravitation at Cambridge to an end; tremors may have been the cause of unsteadiness of images from long focal lenses, rendering accurate astronomical photography uncertain. Times have occurred, when making determinations of standard weights, that balances have been unsteady; and the writer has repeatedly observed delicate forms of these instruments, used in assay work, on the swing and often changing their zero-point. How far tremors may have accelerated or retarded the swing of pendulums in gravitational observations, we do not know. What we immediately wish to do in regard to tremors and diurnal waves is to find means to isolate ourselves from, or at least to minimize, their effects. We can entrench ourselves against the mechanical vibrations of a passing train by means of trenches, and the surveyor in a city can isolate his instrument against the same intruders by a suspension of rubber bands.

How far the study of the tremors which have here been described will enable us to avoid their effects or to destroy them, or to what extent a closer examination of these ill-understood phenomena will prove beneficial, are matters only to be decided after further investigation.

Before the reading of the paper, the PRESIDENT made the following remarks: The paper this evening, which we may expect to be of an extremely interesting and novel character, is by Professor Milne, on the movements of the Earth's crust. I will now call upon Professor Milne to read his paper.

After the reading of the paper, the following discussion took place:—

Sir ARCHIBALD GEIKIE: I think the Geographical Society is to be congratulated on having had from our great authority on earthquakes so luminous and so humorous a description of the subject as he has given to-night. I don't know whether it is a fortunate thing to live in an earthquake country, but Professor Milne's residence of many years in Japan has given him experience of more earthquakes than any other man of science in the world has had, and he has certainly devoted more time to the study of them. But not only has he given attention to earthquakes, he has, in recent years, been studying the minute and almost imperceptible movements which Mother Earth is continually suffering. From the point of view of Geography, we ought specially to support the idea of having these movements carefully measured. The day for the kind of observations which were all that Mallet and his predecessors had to rely on, is past; we can get no more information as to the internal economy of the Earth from that source. What we want now is careful and critical measurement to explain the movements of the Earth's crust. The fitting up of ten or fifteen observatories would probably not cost very much, and even if, in two years, we could get only a part of what he has promised us, I think the expense would be well bestowed. There are various features of physical geography where such measurements might help us. "Old as the hills" is a familiar phrase, but the hills differ vastly in age, and many of them are growing still. We do not know the rate of their growth, but if we had such observatories fixed up, we might be able to watch from year to year the gradual movement, either upwards or downwards, of a mountain chain. Then, again, we know almost nothing of the source of earthquakes. Professor Milne, though familiar with many of their phenomena, has not yet been able to throw much light on their actual source. No doubt they arise from many causes, and possibly the instrument to which he has referred may enable him to classify the various earthquakes and say that they originated, some from one, some from another cause. These are some of the geographical aspects of the subject which he has brought before us. I rejoice very much to have listened to him. He and I have corresponded for many years, but it is not often I have had an opportunity of seeing him in the flesh. He has set up an observatory in the Isle of Wight, where he sits watching for earthquakes in all parts of the globe, and recording the tremors that affect the ground. I think it is incumbent on geographers to do everything they can to further the institution of similar carefully equipped observatories for the detection and recording of these earth-movements. We can never tell to what practical account the most abstract scientific research may be turned, and I think we can even now see, apart altogether from the scientific results, the practical bearing of some of these observations. I hope the idea will be taken up and developed.

Dr. H. WOODWARD: I don't think, after we have heard Sir Archibald Geikie, that it would be proper that I should add any remarks, except to congratulate my friend, Professor Milne, on his return to England after twenty years' residence in Japan. When a man has been working steadily in a foreign country for twenty years, he must feel it a great pleasure to come back and meet with a large and appreciative audience like the present one, to take an interest in the work he has been carrying on, when one considers the very small encouragement one receives in a foreign country; and although we have not as many earthquakes to supply him with, yet, if what he has told us to-night is to be relied upon, that he can receive through the Earth, from all parts of the Earth, earth-tremors and indications of earthquakes, and can measure them, it is hardly necessary he should go further away. At the same time I hope, for the peace and safety of our island, as Sir Archibald said, that he will not promote the production of too many earthquakes here. I hope the Society will encourage Professor Milne in every possible way, and help him in setting up proper stations to make observations, and when these are accumulated we may look for most valuable results. Nothing can be more striking than what he has pointed out regarding the rapidity with which earthquake-tremors are transmitted through the centre and different portions of the Earth's interior, because it would seem to point out an enormous density of the Earth's interior as compared with what we had reason to suppose would be the case, and if that is so, it may lead to most important results with regard to a knowledge of the interior of the Earth, which we can never hope to reach by any other means. I heartily congratulate Professor Milne on his interesting paper.

Professor **John Perry**: I am afraid I am not well able to respond to a sudden call like this. Indeed, as Professor Milne hinted in his lecture, I have not anything to say even about the instrument which I invented. I may further say that even in my three and a half years' stay in Japan, I was not able to develop any greater love for earthquakes or for the study of earth-movements than exists in the average inhabitant. But I can admire the enthusiasm, and I can welcome results of the work, of a man like Professor Milne. I confess that I have puzzled very uselessly over that fact so wonderful to the mathematician, that earth-ripples travel sometimes from Japan to Europe at the rate of 12 to 20 kilomètres per second. As a practical engineer, I take it that the most important outcome of Professor Milne's work during twenty years is that he really has discovered how to protect houses from earthquakes. He has not said anything to us about this to-night, but it is a very wonderful thing. To me, living as an engineer in London, the interest in this matter greatly depends upon the possibility of mutual reversibility of the problem and its solution. He isolates a house so that it cannot be affected by motions of the outside earth. Can we employ Milne's method to so isolate an engine that it cannot give motion to the outside earth? I feel sure that we can, and if this is the case, one great trouble will be removed from the minds of electric light engineers. It was in connection with this great trouble that I recently began to study the subject of earth-movement. The instrument which Professor Milne has shown you was hurriedly designed to allow me to make observations of motions in a block of houses built near an electric light station. I have been able to discriminate between up and down, north and south, east and west motions, and to observe their amounts. Professor Milne applies it, I think, to indicate more particularly, changes of inclination. He has assisted me by using one of his horizontal pendulums to make actual records of the motions in my block of houses. I congratulate the members of this Society on hearing such an excellent lecture on the subject of earth-movements from the man who has so specially made it the study of his life, and I hope that he will be successful in establishing his seismic observatories at many places over the world.

The **President**: It is from Sir Henry Howorth that we must hope for the advocacy of the cause of establishing observatories in England. As there are 928 in Japan, our Government might respond to our request for one here, and we must look to Sir Henry Howorth, one of the scientific members of the House of Commons, to help us in advocating this measure.

Sir **Henry H. Howorth**: Whatever courage it may require to face these awful earthquakes in Japan, you must attribute to me a considerable amount of courage in daring to get up on the eve of Parliament, to make a suggestion that the money which is wanted for guns and ironclads should be invaded by a claim for observatories to record various methods of earthquake action. You will be pleased to know that, so far as I am concerned, it will be a great delight to press on this kind of expenditure; for, in addition to believing that we cannot spend too much in insuring what we have, I want to make what we have as good as we can, so that we may be at the head of all nations, if possible, in science as well as material prosperity, and not have a rival in our Japanese friends in investigating earthquakes. But I must remind you, sir, I belong to a class not popular in the House of Commons—the independent member, and the independent member is described as "a rascal, whom no party would trust," and therefore all I can do is of very little value and importance. May I refer to one kind of earthquake that happens in England, to which my friend did not refer. We have to welcome here to-night, not only Mr. Milne himself, but the lady who has shared all these perils with him, and I am pleased to believe that there are certain tremors and earthquakes that occur in a good many houses in this country, even in the houses of members of Parliament, which do not reach Japan at all, and these he will hardly be able to measure by the instruments on the table. I refer to domestic tremors. I believe I have one curious distinction in this room. I was born in the house which was situated in the very middle of where the great chasm took place in Lisbon, in the year 1755, in the corner house of Blackhorse Square, built by the Portuguese king over the spot where the collapse took place. This I have often seen as a boy; for the remains were not removed until three-parts of a century afterwards. One moral that we should draw is this, that we never know when these paroxysmal movements take place, or in what particular quarter. People do not realize that the destruction of the Eastern empire was more by earthquakes, that destroyed nearly every town in Asia Minor, than any other thing in the world, and those earthquakes that came in a great rush in a certain period of Byzantine history, might come, it appears to me, with a rush in any latitude, because we do not know the laws which govern the distribution of these movements. It would be a great gain to us if we could analyze how these forces are moving, and in which direction, for we may ourselves have to face some of these perils presently; but, apart from this there is the greater question which really underlies a great deal of the reasonings in the sciences of geology and terrestrial physics. It is important, as my friend on my left—who has written the most delightful book on the physiography of the Earth I know anywhere, the 'Scenery of Scotland'—has said, that when we study these sciences, the causes of earthquakes and their effects form the very elementary knowledge we must have, if we are to understand these problems. The whole of science is continuous; we know now that all the gases, liquids, and solids are perfectly continuous forms of matter. These waves of motion, which were supposed formerly to be limited to air and water, pass through solid ground as easily as through liquid masses. We shall now be able, because we can apply our tests more easily through solid matter, to test the character and extent and work of this kind of earth-movement. I must ask your indulgence for detaining you so long, but the lecture has been most delightful in every way, and I cannot say how much we all appreciate the difficulty of introducing a little humour into a dry subject, which seismology at first sight seems to be. We hope we may be able to secure one or two at least of these most necessary observatories in this country.

The PRESIDENT: I cannot help recurring to the time, many years ago, when this study of seismology was confined almost entirely to earthquakes. I was looking the other day over one of the addresses of my illustrious predecessor, Sir Roderick Murchison, delivered in 1859, and I found that he referred to the subject of the lecture to which we have been listening this evening. He said that "the theory of earthquakes could only be regarded as a subordinate part of a more general theory which will deal with all earth-movements, great or small, to which the Earth's crust is subject." He looked forward to the time when an attempt would be made to unite into a whole the mass of facts, and to account for them by the application of one consistent theory. Now, I think this shows rare prescience in Sir Roderick. He almost seems to have foretold that we should have such a meeting, and listen to such an address as we have heard this evening, looking forward for a period of thirty-seven years. I think I may assure Professor Milne, on the part of the Council of the Royal Geographical Society, that he will receive all possible help, and that the Council will use all the influence it possesses in order to further the admirable work of establishing these stations in proper places over the globe. It now only remains for us to perform the very agreeable task of thanking Professor Milne for combining, with amusement and humour, an address which is likely to be an important starting-point in a new direction, and in a new line of investigation, or, at all events, a very great enlargement of a former line of investigation. It is quite clear that, however dull the subject—and seismology is certainly not a dull subject—it would never be dull in the hands of Professor Milne. Now I ask you to return Professor Milne our warmest thanks for the very interesting evening he has given us. Our next meeting is on February 24, when Mr. Littledale will read an account of his recent very interesting journey through Tibet. I cannot help, in anticipation, expressing my admiration of the way in which Mr. Littledale performed his journey. Mr. Scharbau has put into my hands a note, by which I see that, after going over 860 miles by dead reckoning, he was out only half a mile when he came to fix his position by the observation.

APPENDIX.

For the purpose of making the references to the records obtained from seismographs and horizontal pendulums more clear, it has been thought advisable to add the following diagrams.

A. *Records from Seismographs.*

Fig. 1.—This figure shows the vibrations of an earthquake (September 3, 1887) as recorded upon a *stationary* smoked glass plate. From this we see that, although the greatest movements have been in a north-east and south-west direction, movement has taken place in many azimuths. The direction followed by an earth-particle has not been in straight lines, but in ellipses and along paths showing a complexity of curves.

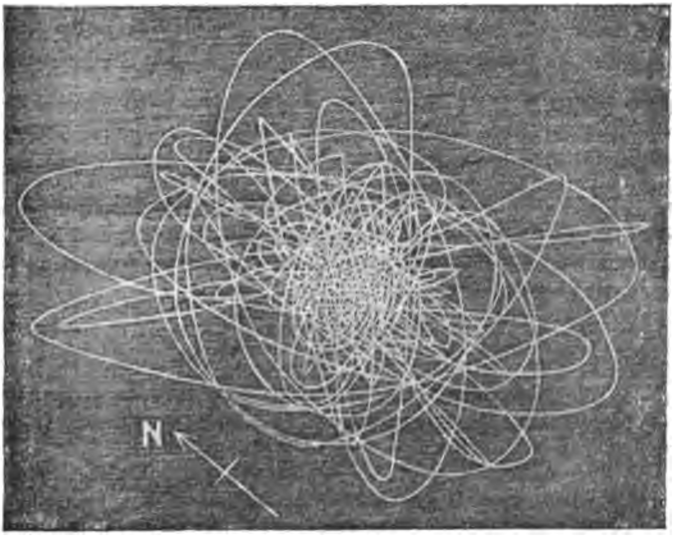
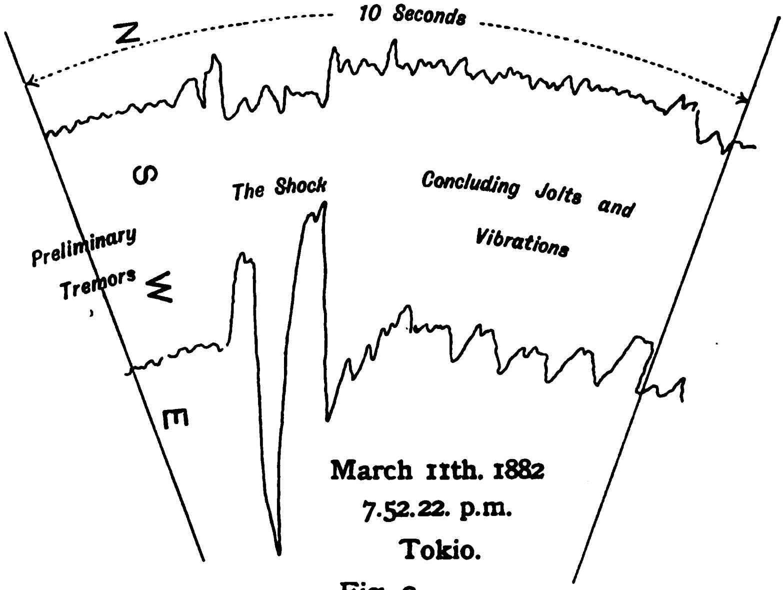


Fig. I.

Fig. 2.—The diagrams shown in this figure are those of the north-south and the east-west components of an earthquake recorded upon a *moving* smoked glass surface. The vertical component is not shown. A portion of the preliminary

tremors are shown preceding the large vibration or shock of the disturbance, which is followed by irregular vibrations and jolts, which in some earthquakes may continue for several minutes. Small vibrations corresponding in period to the preliminary motions are often superimposed upon the latter portion of a diagram, which usually dies out as long-period smooth undulations. Although this diagram illustrates various types of earthquake motion, it is only occasionally that all occur together. Instead of one shock there may be several shocks, or a movement consisting only of rapid vibrations or of gentle undulations. The duration of the disturbance as recorded by a free horizontal pendulum may be several hours.



March 11th. 1882
7.52.22. p.m.
Tokio.

Fig. 2.

From the open diagrams, it is evident that the amplitude and period of any vibration may be measured, and from these quantities maximum velocities or or accelerations may be calculated.

B. *Horizontal Pendulums intended to record Earthquakes, including those which cannot be felt, Diurnal Waves, Tremors, and other Earth-Movements.*

Fig. 3.—This figure shows the chief features of a horizontal pendulum standing on a masonry column, with the photographic recording apparatus on a table at a lower level, which is an arrangement found to be convenient. The boom of the pendulum, which is about 2 feet 6 inches in length, is held in a horizontal position by a wire tie, which brings the agate pivot at the inner end of the boom against a needle projecting from the base of an iron stand. The weight of the boom, which is made of aluminium, is balanced by two small weights at the extremities of a small bar pivoted between the tie and the stand. The stand has three levelling-screws. The front one of these tilts the boom in a fore and aft direction, thus varying the sensibility of the instrument. Another of these screws has a pointer moving over a scale of degrees. By turning this screw the plane of the stand and boom may be tilted through a known angle. This results in the outer end of the boom being deflected through a certain number of millimetres. The instrument

is usually adjusted so that the period of its swing is about seventeen seconds. When this is done, a movement of the outer end of the boom of one millimetre indicates that the stand has been tilted 0·5″ of arc.

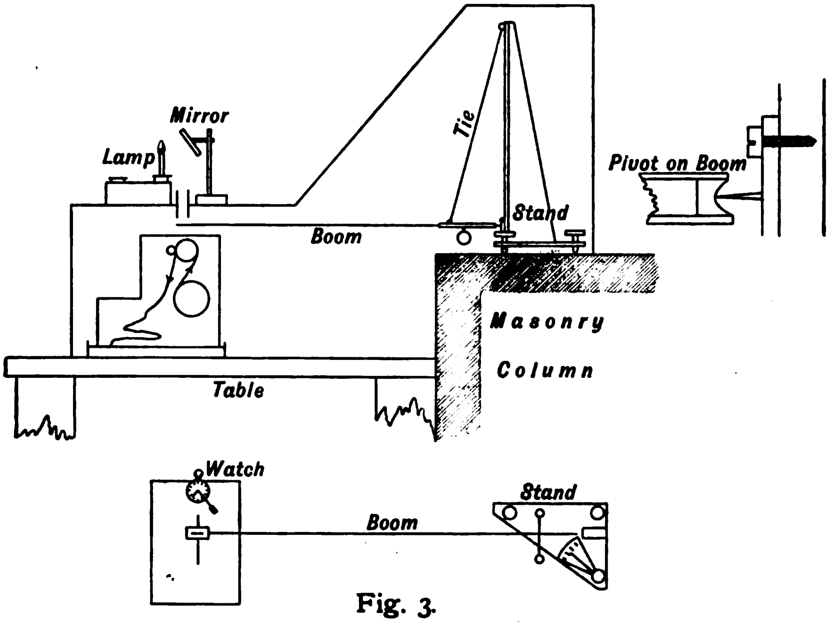


Fig. 3.

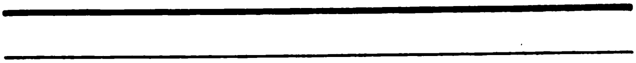


Fig. 4.

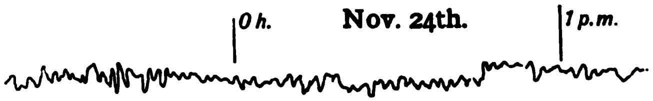


Fig. 5.

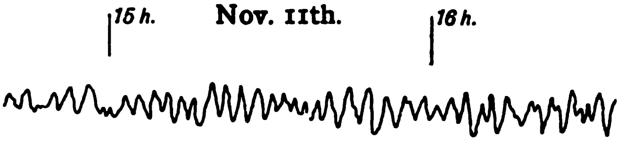


Fig. 6.

The outer end of the boom carries a small slip of blackened mica, which has a slit in it. This floats over a slit in the lid of a box, which is at right angles to the slit in the mica. In the box there is clockwork driving a 2-inch band of bromide paper. The light from a small benzine lamp is reflected by means of a mirror downwards through the two slits, and reaches the paper as a point. If the floating plate has a broad slit and a narrow slit, the image on the moving photographic surface is that of a broad line and a fine line (Figs. 4 and 7). The fine line gives beautiful definition for slow movements, whilst the broad line is visible for rapid motions when sufficient light may not have passed the fine slit to make a photographic impression.

The rate at which the paper moves is controlled by a watch with a long minute-hand tipped with a piece of blackened mica, which every hour eclipses the light entering one end of the slit. These hour-marks, with paper moving at about 45 mm. *per hour* (see Fig. 7), enable an observer to determine the time of any movements to within ten seconds. For studying diurnal waves and the times when tremors are frequent, paper moving at the rate of 75 mm. *per day* is sufficient.

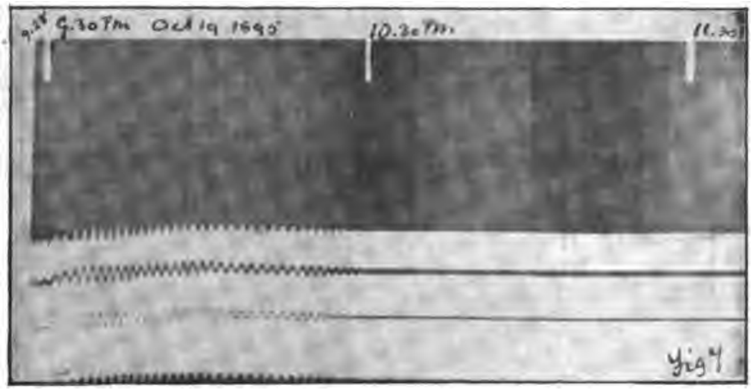


Fig. 7.

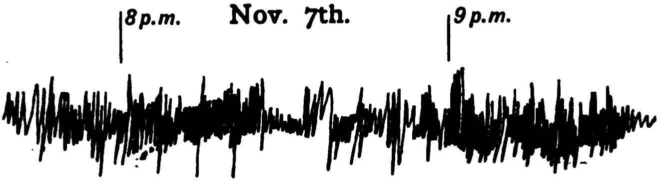


Fig. 8.

With the writer's installation at Shide, in the Isle of Wight, every morning the watch is wound, and the lamp attended to; whilst once a week a new 25-foot roll of bromide is put into the clock-box. The attention which the instrument requires is therefore very small. The cost for photographic materials and benzine is about two shillings per week.

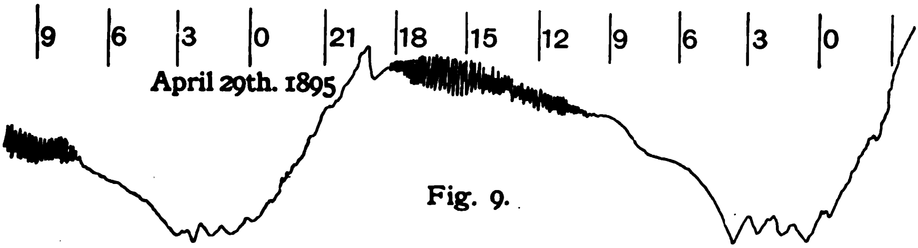


Fig. 9.

one year, but in successive years. The network of almost simultaneous observations which alone can make it possible to draw such synoptic charts, involves the combined operation of a larger number of ships than any one government can reasonably be expected to send to sea at one time; but the evidence brought out by the Swedish researches, amounting almost to actual proof that increased physical knowledge must lead to increased acquaintance with the habits and migrations of some of our food fishes, opens a way towards an international scheme of observations in which all the countries interested in the fisheries of the North Sea and the Baltic may take part, each exploring the areas more immediately concerning it, and at the same time contributing observations to the general fund.

Professor Pettersson and Dr. G. Ekman submitted a proposal for systematic co-operation to the Congress of Scandinavian scientists in Copenhagen in 1892, suggesting that a preliminary series of such surveys should be made in May, August, and November, 1893, and in February and May, 1894. The results were, under the circumstances, fairly satisfactory. In May, 1893, Danish and Swedish ships were at work, and in August these were joined by ships from the Kiel Commission and the Fishery Board for Scotland, while Norway took part in some of the later terminal cruises. Work was unfortunately greatly hindered during the winter months by weather of quite exceptional severity, H.M.S. *Jackal* being peculiarly unlucky in this respect; but a very considerable amount of material was nevertheless collected upon each occasion. Special reports upon the work done have already been published by the directors of the Swedish, German, and British expeditions—Professor Pettersson, Professor Krümmel, and the present writer—and it is here proposed to summarize the joint results of all the observations, in so far as they refer to the *surface* waters of the North Sea and the entrance to the Skagerak. In discussing the relation of these results to the currents of the North Atlantic and Norwegian Sea, I have collected all the available observations made on board merchant vessels within the periods named. Professor Pettersson was kind enough to work up the records of Danish and Swedish vessels, and to send me the results, and with these I have incorporated observations extracted from logs deposited in the Meteorological Office, to which Mr. R. H. Scott, F.R.S., courteously gave me access. I may here be permitted to express my thanks to my colleagues for entrusting to me this part of the work, and for the cordial way in which they have rendered every assistance in carrying it out. I have specially to thank Professor Pettersson for many hints and suggestions.

In considering the circulation of waters in the North Sea, we have to deal with the various sources from which these waters are derived, the relative amounts obtained from each, and the distribution and mixture of the different waters over the North Sea itself. The inflowing waters are in the main of two kinds—oceanic waters entering from the north and west, and also to some extent from the Straits of Dover, and land waters entering from the Baltic and from various rivers; and the chief characteristics upon which we have to rely in identifying these are salinity and temperature. The oceanic water in its pure state always contains more than 35 parts by weight of salt in 1000, while the land waters under similar circumstances never rise above 34 parts per 1000: the range of temperature is in the former case much less than in the latter, the supply of fresher waters being to a large extent cut off by frost in winter, and rising in summer to a temperature much higher than is observed in the open sea. Between 34 and 35 per mille of salinity is found a water intermediate in its properties between the two just mentioned, and in nearly all cases formed from them by mixing. To this mixture has been given the name "North Sea water," although, as will be seen later, the actual mixture does not always occur within the North Sea itself. In a large and deep basin the amount of this intermediate water present at any time would be small compared with either of the others, for the fresh water would overlie the denser, and mixture would take place slowly; but in a shallow area like the North Sea, where the influences of wind and tide penetrate to the bottom, mixture sometimes takes place with amazing vigour and rapidity, so that the intermediate water sometimes occupies almost the entire basin.

In reviewing the distribution of temperature and salinity in the North Sea during the periods over which our observations extend, we are accordingly led to consider local influences which affect the mode and extent of mixture taking place, after which we may proceed to discuss the external influences regulating the supplies. These influences we may take to be, on the one hand, differences of specific gravity, themselves due to the variations of temperature and salinity; and on the other, the prevailing atmospheric conditions, the latter, in the case of surface waters under consideration, much more important than the former. The sketch-maps (Figs. 1 to 5), which are intended to accompany the charts of the salinity (Plates 1 to 5) and of the temperature (Plates 6 to 10) of the surface of the North Sea, need no further justification. They have been prepared from data extracted from the Daily Weather Reports published by the Meteorological Office, *i.e.* from land observations only; the average distribution of atmospheric pressure at 8 a.m.—from which the direction and force of the prevailing winds are inferred—is shown by solid lines, and the air-temperature by broken lines. Similar charts have been prepared for periods preceding those covered by the salinity charts (Plates 1 to 5); these are not reproduced, but any points of interest brought out by them are included in the following notes:—

SURFACE OF THE NORTH SEA.

May 1 *to* 10, 1893.—(Plate 1.) Oceanic water covers a very small area well to the north and north-east, and an isolated patch near the Straits of Dover. Water of 34 per mille salinity covers nearly the whole area, and extends to the west coast of Scotland. The fresher waters are restricted to a narrow strip on the eastern side. Temperature (Plate 6) tends to be highest near the coasts, but the isothermals run, in general, parallel to the lines of latitude, temperature rising to southward, but nevertheless highest in the open sea to north-west. The characteristic feature of this chart is, however, the remarkable uniformity in the distribution of temperature in and around the North Sea area, and this fact attains great significance from a consideration of the distribution of pressure. During the last ten days of April, a shallow anticyclone lay over the North Sea, and moderate anticyclonic winds from north-east continued during the whole period covered by the charts (Fig. 1). So far as the movements of the surface water are concerned, the conditions are probably those normally assumed when both oceanic and land streams are weak.

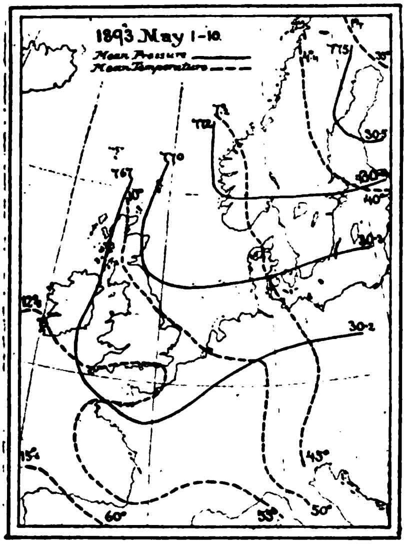


FIG. 1.

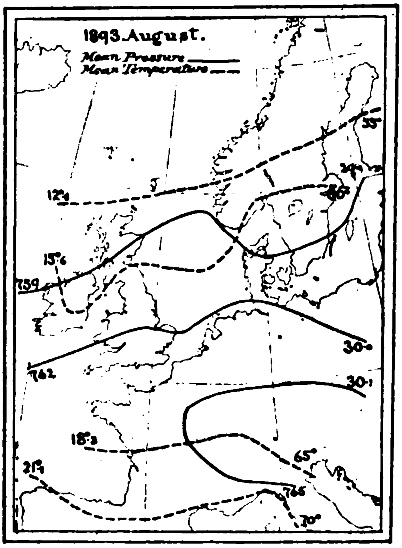


FIG. 2.

August, 1893; *the whole month.*—Plate 2 shows oceanic water covering a much larger area than in the preceding case, the axis of this area being also more central, while there is a marked influx from the Channel. Water of 34 per mille salinity is now a good deal restricted; the bulk of it lies in the south part of the North Sea and along the east coast of Great Britain. Fresher waters cover a greatly increased area, an immense volume of water of extremely low salinity now issuing from the Baltic at a temperature higher than is observed anywhere except in two isolated patches near the British coasts. Temperature gradients are on the whole much steeper, the isothermals being generally tilted towards the continent, but tending to curve round an axis running north-westward between the Orkney and Shetland islands, obviously the effect of the strong tidal streams (see 'Report of the Fishery Board for Scotland,' 1894, p. 354).

The barometer charts show that in the latter part of July pressure was lowest over Southern Norway, giving fairly light gradients for westerly and north-westerly winds. During August the centre shifted westwards towards the Norwegian Sea, and then returned—on account of the passage of shallow depressions—giving south-westerly winds in the early part of the month, and then again westerly. The mean for the whole month (Fig. 2) gives light gradients for westerly winds, closely agreeing with the many-year average for August ('*Challenger* Reports,' *Ocean Circulation*).

In studying these maps, it is to be borne in mind that alike in Scandinavia and in the eastern parts of Great Britain August is a month of great local disturbances of temperature and rainfall, some of the eastern counties of England receiving their greatest monthly rainfall at that time. It would seem that in 1893 the wind influences over the North Sea were nearly normal, both the oceanic and continental inflowing streams being probably abnormally strong.

November 16 *to* 25.—Plate 3 shows 35 per mille water forming a blunt-ended tongue which extends over the greater part of the northern entrance to the North Sea; and the same water is intruded for a considerable distance from the Straits of Dover. The intermediate 34 to 35 per mille water occupies nearly all the rest of the area, except off the Norwegian coast, where the periodic influx of "bank water" from the Norwegian channel, first recognized by Ekman and Pettersson in 1890 (*Scottish Geographical Magazine*, 1894, p. 456), is again observed. Professor Pettersson informs me that the deep-sea observations of the Norwegian and Swedish expeditions, under the direction of Dr. Hjorth and himself, afford final and conclusive proof of this remarkable phenomenon. It is further noticeable that the supply of nearly fresh water from the Baltic is now wholly cut off.

The temperature of the surface water (Plate 8) is now lowest on the continental side, except off the east coast of Scotland, where the occurrence of up-welling under the conditions observed has been proved (*Journal of the Scottish Meteorological Society*, vol. viii. p. 332). There is a marked maximum at the entrance to the Skagerak.

The isobaric chart for the ten days preceding November 16 shows a strong minimum near the Færoe islands, the centre of which moved later (Fig. 3) to Northern Scandinavia, giving gradients for strong westerly and north-westerly winds. The period was marked by a succession of deep cyclones moving eastwards and north-eastwards, and included the great storm which travelled from south-west as far as the Shetlands, and then recurved southwards along our eastern coasts.

In this case local and oceanic influences were probably more energetic than usual, and the result is shown by the sharp contrasts.

February 19 *to* 28, 1894.—Plate 4 shows that oceanic water of 35 per mille salinity now extends over most of the area, and is continued along the central axis throughout. Water between 34 and 35 per mille forms a narrow strip on each side, and in the north reaches the Norwegian coast. Fresher waters are restricted to a band near the German and Danish coast and to the Skagerak, water below 33 per mille being entirely confined to the last.

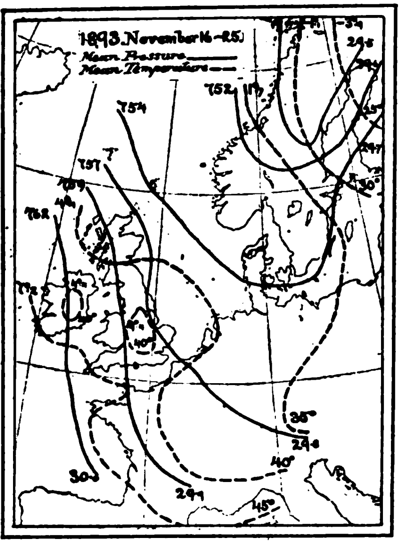


FIG. 3.

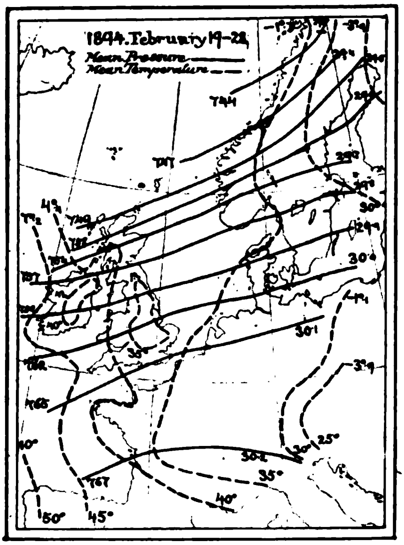


FIG. 4.

The distribution of temperature is altogether changed; the isothermals run north and south, temperature being highest to westward. In the oceanic area beyond, a large area of very uniform temperature is observed to the westward of the British Isles, to the north of which we may suppose, although observations are wanting, that the gradient becomes steep. The distribution gives additional interest to the barometer charts for the days preceding and during (Fig. 4) the period, which show steep gradients for westerly and southwesterly winds. The appearance of the charts would indicate that local and oceanic influences are strongly developed, while the land influence remains weak.

May 1 to 10, 1894.—Data are very incomplete, but it appears from Plate 5 that the 35 per mille water covers practically the same area as in the preceding February. Probably there is some freshening of the water round the continental coasts, but the relative conditions seem very much to resemble those of February. Temperature shows a tendency to resume the normal east and west distribution, with increased warmth towards the north-west. Barometric pressure (Fig. 5) shows a minimum north of the Shetlands, with fairly steep gradients for westerly winds over the North Sea, tending to north-west near the Orkneys and Shetlands, and south-west on the continental side.

In addition to the charts here reproduced, Professor Pettersson has been able to map the distribution of surface salinity over the greater part of the area at the end of November, 1894, and the middle of February, 1895, from observations to which this country did not contribute. Both cases exhibit some peculiar features. In November, 1894, the 35 per mille water is split into two parts, one extending south-east from the Orkneys, and another off the Norwegian coast, reaching as far south as lat. 59° N. The total area covered by it is but small, by far the greater part of the North Sea surface being occupied by the intermediate 34 per mille water. Very fresh water—below 32 per mille—extends far north along the Norwegian coast, and the 32 per mille to 34 per mille forms only narrow strips. A more complete contrast to the corresponding chart for 1893 could scarcely be imagined; oceanic and bank waters are enormously reduced, while the Baltic stream, instead of being cut off as in 1893, makes its way far to the northward.

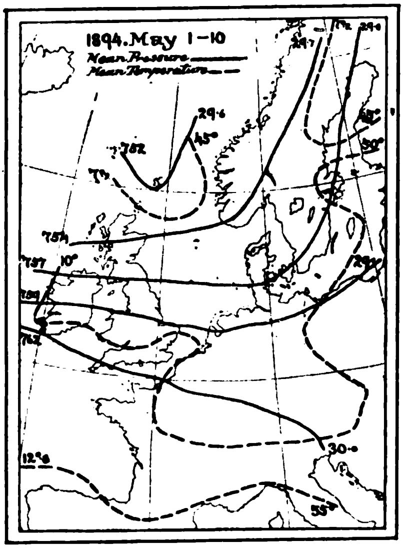


FIG. 5.

The daily weather charts show that anticyclonic weather persisted during most of the month from the Continent nearly across to Great Britain, while deep depressions moved north-eastwards, but kept well out in the Atlantic. This may account for the great weakness of the oceanic streams, including the "bank water;" and the greater strength of the Baltic stream is probably due to the mildness of the season.

In February, 1895, the 35 per mille water again formed a blunt-ended tongue, somewhat as in November, 1893, but it is remarkable for its westerly position, this being the only case where the 35 per mille touches the east coast of Scotland. Water below 32 per mille salinity has disappeared from the Norwegian coast. The daily weather charts show that a centre of high pressure was nearly stationary during the whole period over Scandinavia, while depressions moved northwards to the west of the British Isles. Hence strong north-easterly winds were experienced over the North Sea, westerly winds outside, and low temperature generally. The oceanic stream is therefore probably strong, but there is a tendency to cover it over at the surface with "bank water," of which the supply is, however, deficient. The cold has entirely cut off the supply of fresh waters from the Baltic.

Comparing the three existing charts of salinity for February, 1890 (see *Scot. Geog. Mag.*, 1894, plate iii. p. 392), 1894, and 1895, we find in each case a central area of 35 per mille water at a temperature of 6° to 7° C., surrounded by colder and fresher waters—North Sea water, temperature 4° to 5° C.; bank water, 3° to 4° C.; and coast waters, 0° to 2° C. In 1894 the strong influx of oceanic water both from the north-west and from the Channel caused it to occupy an unusually large area, and temperatures were everywhere high. February, 1895, on the other hand, is characterized by a much smaller area of oceanic water, and large quantities of remarkably cold, fresh waters in the southern part of the North Sea. We remark that 1894 was the warmest, and 1895 the coldest, of the three winters.

We gather from the foregoing that the distribution of water of all salinities over the surface of the North Sea varies within very wide limits, both as to the size and position of the areas covered, and it may be altogether different at the same season in different years, and practically the same at quite different seasons in the same year. Hence it appears that the forces which produce the changes act rapidly; a change of distribution at the surface may be wholly or chiefly due to forces which have come into action only a short time before. A study and comparison of the maps confirms our division of these forces into *local* and *external*, and shows that the chief influence at work locally is the wind. It will be seen later that the wind has also a profound effect on the surface supplies of oceanic water, a fact which greatly increases the difficulty of separating out the results of local action in mixing the inflowing waters. In so far as existing observations enable us to surmount these difficulties, however, it may be suggested that—

1. Calm weather favours the spread of a thin layer of water of 34 per mille salinity over a great part of the surface of the North Sea, the result of previous mixing.

2. Strong northerly (N.E., N., or N.W.) winds tend to broaden the area covered by 35 per mille (oceanic) water, and to blunt its extremity, and the surface salinity on the whole is increased, the fresher outflowing waters being driven back. One very important effect is to send water between 33 and 34 per mille southwards along the west coast of Norway; and further investigation may show that these conditions are responsible for the influx of "bank water" into the Skagerak already referred to, which has been found to coincide with the period of the Swedish herring fishing.

3. Westerly and south-westerly winds tend to form a continuous strip of 35 per mille water along the whole of the central axis of the North Sea, probably because the oceanic streams are strengthened, but at the same time mixing goes on rapidly, and there is strong upwelling from the British coasts.

4. Easterly and south-easterly winds reduce the salinity as a whole by spreading the fresher waters over the surface. The oceanic water is covered over, or shows a small area close to the coast of Scotland.

It would, of course, be premature, at this stage of the investigation, to insist on the invariableness of these results, or to found important conclusions upon them. The actions are in all cases complex, and their reactions are undoubtedly still more so. One point only can hardly pass unnoticed, and may be suggested as lending additional interest and importance to further inquiry—the indication that during the colder seasons the presence of a large body of warm oceanic water in the North Sea constitutes a line of weakness at the edge of the great continental system of high pressure, along which cyclones tend to make their way, while a surface of fresher, colder water favours the extension of high pressure over the British Isles. A comparison of the winters 1893–94 and 1894–95, in relation to the surface temperature and salinity of the North Sea, brings forward a number of questions of this nature which press for early settlement one way or another.

THE INFLOWING WATERS.

Turning now to the external influences controlling the volumes and velocities of the inflowing streams, it is evident that these must be considered in two divisions—the fresh or land waters, and the oceanic waters. With regard to the former, it may be said that the chief supplies come from the Baltic, while the British contributions are of small importance. The currents of the Baltic and Skagerak have been discussed by the Swedish hydrographers with a completeness which stands unrivalled in work of this kind, and we need, therefore, only again refer to Professor Pettersson's papers in the *Scottish Geographical Magazine* for 1894. For our present purpose it is to be remembered, in particular, that the supplies of these waters are much greater in summer and autumn than in winter and spring, the difference being in general greatest between a hard winter and a wet summer, and least between a mild winter and a dry, hot summer; that these waters are much the hottest in summer and the coldest in winter, and that therefore their relative lightness is greatest in the former season; that, as outflowing currents, they tend to split into two whenever from any cause their direction is altered, and that they tend to induce "reaction currents" of inflowing water under them.

The most important fact in connection with the supply of oceanic water to the North Sea is, that in the Færoe-Shetland channel, just at the north-western edge of the continental shelf, a mixture of waters from different sources is constantly taking place. The current of Atlantic water pouring over the Wyville Thomson ridge sucks up the cold bottom water of the Norwegian Sea, and, mixing with it, has its own temperature lowered, thereby causing it to sink and in great part lose its horizontal motion. It is absolutely essential that we should possess a detailed knowledge of how this process goes on from year to year, of the various sources from which the waters are derived, and of the proportions in which they enter into the mixture. Much information is to be derived from the work of the *Porcupine*, *Lightning*, *Triton*, and *Jackal* expeditions, and from the Norwegian North Atlantic expedition, but a great deal remains to be discovered, and for this deep-sea observations alone will suffice. It will appear presently, however, that surface observations are of very considerable value.

We have to consider (*a*) the movements of the great north-easterly drift of warm surface water from the Atlantic, and (*b*) obscure and variable currents from northern latitudes. The maps of surface temperature for dates corresponding to the charts of the North Sea (Plates 6 to 10) present the following features:—

May, 1893 (Plate 6).—An axis of high temperature runs from mid-Atlantic north-eastwards to about 15° west longitude in the latitude of Ireland. It then splits into three branches: one runs up to the Færoe-Shetland channel, the warm stream being very narrow; a second runs due north to the south-east coast of Iceland, where it is blocked; and a third in a north-westerly direction to the west coast of Iceland, where it spreads out and is lost. A centre of cold to the east of Iceland extends south-eastwards, and there are signs of another such centre to the east of Greenland.

August, 1893 (Plate 7).—The warm axis in the Atlantic has moved about 6° to the westward. It splits into three as before, but the Færoe-Shetland stream has become still narrower, the east-Iceland stream has considerably diminished, and that to the west of Iceland has also narrowed, especially towards the south. Two well-marked cold axes are now apparent, one stretching from the east of Iceland to the Færoes, and a second coming down the east coast of Greenland and beyond Cape Farewell. It is to be noted that the warm axis which appeared off the western coast of Greenland in May is no longer to be found, having probably moved westwards.

November, 1893 (Plate 8).—The Atlantic warm axis has moved still further west, but it now bends more rapidly to the north-east. The data are insufficient to show the subdivisions completely, but it is noticeable that the north-easterly stream seems wider, and that the stream to the east of Iceland is almost absent. The observations indicate a well-marked cold axis from the east of Iceland, but this now runs due south, allowing greater expansion of the warm stream in the Færoe-Shetland channel.

February, 1894 (Plate 9).—The Atlantic axis has again moved eastward, and, so far as the observations go, there seems to be a wide stream entering the Færoe-Shetland channel.

May, 1894 (Plate 10).—The Atlantic axis lies far to the westward; the north-easterly stream is probably weak, but is narrowed and confined by the cold stream from the east of Iceland. The cold stream to the east of Greenland, and the absence of warm surface water to the west of Greenland, are again noticeable.

From these maps it would seem that the Atlantic streams are on the whole strongest in summer, but that their common source is at that season far to the west. The currents flowing northward may then be weakened by an unusually large delivery towards the Spanish and African coasts (the "west wind drift" and "Canaries current"); but the great branchings of the north-east stream are nevertheless more clearly marked than in winter, and we may connect this with the fact that the cold streams moving southwards from the eastern coasts of Iceland and Greenland are also most strongly marked in summer. In winter these streams are weak or altogether absent, and the warm north-easterly drift tends to spread itself uniformly over the surface of the Norwegian Sea.

At the same time there is evidence that the whole distribution varies greatly from year to year. The charts of mean surface temperature between Davis Strait, Iceland, the Færoes, and Scotland, recently published by the Danish Meteorological Institute, give monthly averages for the warmer seasons, and, so far as it is possible to compare these with the charts accompanying this paper, it seems that the average conditions are of much the same type as those we have described, except that the contrasts between the different streams are less clearly marked. In some years the state of affairs may be altogether different, but there remains little doubt of the immense importance of the effects of the polar currents at certain seasons in modifying the warm drifts from the Atlantic. The problem, indeed, resolves itself into one very similar to that already stated for the North Sea—an analysis of the external and local influences acting in the region between the Færoe and Shetland islands. The former are now (*a*) variations in the Atlantic stream, and (*b*) variations in the polar streams; and the latter the action of the winds in mixing the waters derived from these two sources, and in driving the mixture into the North Sea.

The polar streams are evidently greatly diminished in volume during winter and increased in summer, to an amount depending chiefly on the extremes of temperature in each case in different years. The strength of the Atlantic streams and the conditions of mixture depend partly on temperature, but chiefly on the intensity of wind action, and with this in view it may be worth while to glance at the changes in the distribution of atmospheric pressure controlling the prevailing winds from month to month. The question is one of the varying relations between the "Atlantic anticyclone" and a belt of low pressure, really an immense cyclone track, which extends from the south-east of Greenland, round Iceland, and thence in a north-easterly direction. The Atlantic anticyclone is weakest in winter, consisting in January of a long belt of high pressure with two centres; the eastern centre increases in intensity and moves westward, attaining its maximum in mid-Atlantic in July, and thereafter extending east and west and diminishing. Hence what we may call the *propelled* surface currents are strongest in summer, but the north-easterly branches may be weakened by the greater area of north-westerly winds towards Spain and Western Africa. Towards autumn the source of these streams would move to the westward, as we have seen it did in 1893.

The *induced* streams, under which we include the continuation of the warm north-easterly drifts, the polar currents, and the mixed waters resulting from their meeting, are chiefly controlled at the surface by the Iceland depression and by the distribution of pressure over Western Europe. The low-pressure belt is most strongly marked in winter, and at the same season pressure is high over the Continent, air tending to flow outwards from the latter towards the former. In summer, on the other hand, pressure over Europe is diminished, and the Iceland depression greatly lessened in intensity. Hence we might expect Atlantic water to be most widely spread over the surface of the Norwegian Sea in winter, but that at that season it would be most difficult for it to gain admission to the North Sea. As the season progressed, the surface-water of the Norwegian Sea would be likely to contain an increasing proportion of water coming from the north by the east of Iceland, and it would become easier for the mixture to enter the North Sea at its eastern side, where it would be covered over by the outflowing Baltic waters. The changes in the prevailing winds seem also to account, in part, for the presence during summer of large bodies of the mixed waters, extending to a considerable depth near the north-western corner of the continental shelf, at the entrance to the Færoe-Shetland channel. I have elsewhere (Report of the Fishery Board for Scotland, 1894) discussed how these waters may find their way into the western part of the North Sea.

The great differences observed over the Norwegian Sea in different years point to a possible explanation of climatic phenomena similar to that hinted at on a smaller scale in the North Sea, inasmuch as a great influx of cold water from high latitudes—the result of exceptional warmth in the far north—must tend to keep atmospheric pressure above the average, while a wide distribution of warm drift water from the Atlantic must favour the development of the Iceland depression or its constituent cyclones.

Arrangements have been made by the Swedish and Norwegian

Governments for monthly expeditions during 1896, which will make deep-sea observations in the North Sea. Through the courtesy of the British Meteorological Office, the Danish Meteorological Institute, the United States Hydrographic Department, the Bureau Central Meteorologique de France, and the captains of a number of private vessels, the present writer is collecting material which it is hoped will be sufficient to allow of the construction of monthly charts showing the distribution of temperature and salinity over the whole of the Atlantic north of 40° N. lat. during the two years 1896–97. The combined investigations will undoubtedly do much to clear up many of the questions suggested by the preliminary work during 1893–94; but it is equally certain that they cannot attain to their full value, nor can the matter be finally set at rest, unless deep-sea observations are made simultaneously in the Færoe-Shetland channel, and at the north-western entrances to the North Sea.

THE GUINEA AND EQUATORIAL CURRENTS.*

By J. Y. BUCHANAN, F.R.S.

This is an atlas compiled at the Royal Meteorological Institute of the Netherlands from 2900 logs of Dutch ships, and it gives, besides the currents, the wind, rain, temperatures of the air and the sea surface, and some other less important details. The results are given for each month of the year, and the area included lies between the parallels of 2° N. and 24° N., and the meridians of 2° W. and 29° W. From the grouping of the observations, it is obvious that the great majority of the ships whose logs have been used have been bound round the Cape of Good Hope. The observations are most numerous in the squares from 400 to 500 miles from the African coast, where the conditions are subject to much less rapid and violent change than in the immediate neighbourhood of the shore. This, however, is unavoidable where the logs of ships making ocean voyages are the source from which the data are taken.

The currents logged and entered on the charts are obtained by the time-honoured plan of measuring the distance and bearing of the position of the ship by astronomical observation from that given by dead reckoning. The value of the observations so obtained depends mainly on two particulars—the kind of ship on which they have been made, and the skill of the officer who makes them. The logs used have no doubt been selected after careful criticism of the way in which they have been kept, and elimination of those not reaching the required standard. No information is given as to the character of the ships,

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* 'De Guinea en Equatorial Stroomen.' Utrecht, 1895.