"List of Fellows of the Geological Society," 1874; from that Society.

"Royal Cornwall Polytechnic Society—Report 1873;" from that Society.

"Journal of the Quekett Microscopical Club," No. 27; from that Club.

"Abstract of Proceedings of the Liverpool Geological Society," 1873-4; from that Society.

"Taylor's Calendar of the Meetings of the Scientific Bodies of London" for 1874-5.

"Glossary of Fossil Mammalia," by J. E. Gore, A.I.C.E.,; from the Author.

"On the Gault of Folkestone," by F. G. H. Price, F.G.S.; from the Author.

"Notice sur la Faune Marine du Terrain houiller du Bassin Septentrional de la France," par M. Charles Barrois; from the Author.

"Catalogue des Poissons Fossiles du Terrain Crétacé," par M. Charles Barrois; from the Author.

"Etude sur le Terrain Crétacé," par M. Charles Barrois; from the Author.

The following were elected Members of the Association:—

Stephen Austin, Esq.; Major R. Carey Barnard, F.L.S.; Edward Berry, Esq.; D. C. Davies, Esq., F.G.S.; Major Francis Duncan, M.A., D.C.L., LL.D., F.G.S., F.R.G.S.; Miss Emily Faust; Miss Jelley; Julius Maier, Esq., M.D.; R. H. Pearson, Esq.; Rev. M. F. W. Ralph; Rev. S. J. W. Sanders, M.A., L.L.M., F.G.S.; and J. J. Harris Teall, Esq., B.A., F.G.S.

The following Lecture was delivered:—

ON THE CONDITIONS WHICH DETERMINE THE PRESENCE OR ABSENCE OF ANIMAL LIFE ON THE DEEP SEA BOTTOM.

By Dr. W. B. CARPENTER, F.R.S., F.L.S., F.G.S., Corresponding Member of the Institute of France, &c., &c.

It is, of course, quite unnecessary for me to point out to the Members of the Geologists' Association, that the foundation of the whole of geological science—that is, the interpretation of the phenomena presented to us in the study of the earth's crust—must

be based upon the study of the changes at present going on upon the surface of the earth, of course including the depths of the sea. This may be considered as the distinct feature of modern geology, when it is compared with the geology of the earlier part of this century; and I think it will be generally admitted that there is no one who has done more to place the science upon this sound basis than Sir Charles Lyell. I do not say that he stood alone, but I think that it will be generally admitted that there is no one who has done more to lead or to direct the course of geological inquiry in this truly scientific and only trustworthy manner.

Now, until recently, the knowledge of the conditions which prevail over the deep sea bottom has been, I may say, almost nothing. The very depth of the ocean, not many years ago—quite within my own recollection—was so uncertainly known that it might be regarded, in fact, as not known at all. We heard of depths of six or eight miles, and of other parts of the ocean as unfathomable. Now, at the present time, we are confident that these statements have no satisfactory foundation. That amount of sounding line might be paid out, and no bottom struck; but it did not at all follow that the sea had anything like the depth corresponding to the length of that line. The American surveyors, some years ago, devised a new method of sounding, which I must not stop to detail to you, but the principle of which is now universally accepted. It is one which so far avoids the errors of the older method, that there is no part of the ocean yet examined, in which a bottom may not be found, with a considerable degree of certainty, or, at least approximation to certainty, as to the depth at which it lies. And there is no trustworthy sounding, so far as I am aware, that goes beyond the depth of about four miles and a quarter. You may have heard of a recent sounding taken in the Pacific, towards the coast of Japan, in which a far greater depth than this was recorded; but I utterly distrust that statement. The sounding was taken in the line of a strong surface current—the *Kuro Siwo*—the Gulf Stream of the Pacific, as it is sometimes called—the warm current that is the reflex of the Equatorial current of the Pacific, turned towards the north-east by the coast of the Philippine Islands, of China, and of Japan; and knowing the difficulty which Captain Nares, with all his experience, had in obtaining a trustworthy sounding through the Gulf Stream, I feel a strong convic-

tion that, unless we have an adequate assurance that the method employed was one which would overcome that difficulty, we may distrust the statement that a depth of eight miles was obtained.*

At present, then, it has been quite clearly ascertained that the general depth of the Atlantic does not exceed three miles. In some places, however, it sinks deeper. The most remarkable sounding that was taken in the "Challenger" survey of the Atlantic, was one which gave a depth of 3,800 fathoms; that is about $4\frac{1}{4}$ miles; and this was apparently in a deep hole, 100 miles to the north of St. Thomas's. The "Challenger" was on her way from St. Thomas's to Bermuda, and unexpectedly came upon this tremendous depression. Captain Nares was very confident, from the stillness of the water, and the circumstances under which the sounding was taken, that it was a 'true sounding; and there was one very curious fact that proved that the depth was very much greater than that which had been previously taken, namely, that the two thermometers sent down were both crushed. These thermometers had been tested with $3\frac{1}{4}$ tons on the square inch, which is equivalent to that of water of 2600 fathoms' depth. The pressure to which they were subjected would be about $4\frac{3}{4}$ tons at that depth of 3,800 fathoms; and the fact that they were crushed, you see, shows that they had gone down to a greater depth than that to which they were tested.†

Hence, you see, it has only been comparatively recently that we have known anything about the real *depth* of the sea. And it is only still more recently, that we have known, with accuracy and certainty, the *temperature* of the sea bottom. Various observations have been taken in different voyages upon this point. Everyone interested in physical or geological or biological inquiry knew perfectly well that this was a subject of very considerable importance; but unfortunately the greater number of observations taken upon this point have been entirely untrustworthy, in consequence of allowance not having been made for the effects of pressure upon the thermometers. This point, I

* The official report of the "Tuscarora" soundings, which has since come into my hands, shows that depths somewhat exceeding *five* miles were found in this position by the use of Sir William Thomson's sounding-wire.

† A depth of about 4,500 fathoms, or more than *five* miles, has since been met with by the "Challenger" near the north coast of New Guinea. In this last sounding, taken with a hepen line, three out of four strongly "protected" thermometers were crushed by the tremendous pressure.

ought to say, was taken up some years ago by Admiral Fitzroy, who devised a protected thermometer which was used once or twice. The principle of that thermometer was a perfectly correct one, but it was not worked out very satisfactorily in practice; and when we took up the subject six years ago—in the year 1868—we were not at all aware that such a thermometer had been devised. We went out with the best Admiralty-pattern thermometers. The results we obtained were such as led us to wish very much to carry out this inquiry with as great a freedom from any sources of error as could be obtained. Taking scientific friends into our counsels, and having experiments made, we found that a pressure of one ton upon the square inch, which is equivalent to 800 fathoms, would make a difference in the height of the very best thermometer. It would send up the mercury in the very best Admiralty-pattern self-registering thermometer about three degrees; and a pressure of 3 tons, which would be equal to about 2,400 fathoms, would send up the mercury eight or nine or ten degrees in the best instruments, and in inferior instruments would send it up twenty, thirty, forty, or fifty degrees. Therefore, you see, the old observations made with unprotected thermometers could not be trusted. Now, it has only been within the last few months that I have come to be aware—through the careful researches which my friend Mr. (now, I am glad to say, Professor) Prestwich has been making for many years into all the antecedent observations upon this subject, and which he has embodied in a paper which he has communicated to the Royal Society—that nearly fifty years ago a very able Physicist of that time, Lenz, a Professor in the Academy of St. Petersburg, worked out with his colleague, Professor Parrot, this very subject. From experiments of the same kind with those made fifty years later by ourselves, Lenz came to the conclusion that the ordinary self-registering thermometers were of no use at all at great depths; and, not having devised any method of protecting them, he carried into effect, during the second voyage of Kotzebue, from 1824, I think, to 1826, a method which he had planned, and which we should say was so clumsy and so laborious that we could scarcely imagine how it could be effectively carried out. It was this. Considering it quite futile to attempt to obtain the real temperature of the sea-bottom by sending down self-registering thermometers, he sent down a large cask to bring up a sufficient body of water to retain

its temperature in a non-conducting wooden vessel, whilst coming up through the upper warmer strata. According to a formula, which Biot, the celebrated French physicist, devised, a certain reduction from the actual temperature of this mass of water was made for the increase of heat which it might undergo through being brought up through the warmer superstratum. The barrel, I should say, had two valves, both opening upwards. It was considered that, while it was sinking, the water would stream up from below through the valves. Then when the barrel was being hauled up again, the valves would be closed down by the superincumbent pressure, and the water taken in at the bottom would remain in the cask, and would be brought up to the surface; thus bringing up, as it were, the temperature of the deep stratum, with a certain amount of alteration produced by the water having to ascend through the warmer superstratum. This was a clumsy and an excessively laborious process, because the hauling up of a sufficiently large cask from such a depth in the ocean we should have thought could only have been done by a powerful donkey engine; and the Russian sailors must have toiled at the capstan a great many hours to be able to make only one observation.

Now it is a very remarkable circumstance that Lenz's observations gave him a series of temperatures which correspond very closely indeed with those obtained in the "Challenger;" and they are by far the best series of observations which were made antecedently to the "Challenger" expedition—I mean in the great oceans. The general conclusions drawn by Lenz were published only in a little incidental notice in the "Bulletin of the Academy of St. Petersburg," which I believe has escaped the notice of other scientific men almost entirely, until Professor Prestwich dug it out; but these conclusions accord so precisely with those to which I had myself been led, that if the two had been put in parallel columns, it might have been almost supposed that I derived my ideas from those previously put forward by Lenz. But no one whom I ever spoke to on the subject, gave me the least idea that Lenz had previously arrived at them. No physicist, I believe, either in France, Germany, or Great Britain, was at all acquainted with them; and the conclusions which I myself have drawn, I drew entirely from the observations in which I had myself taken part.

I must come at once to these conclusions, without detaining

you upon the mode in which they have been arrived at. The first of them is that in the great oceanic area communicating with the polar basins, there is a flow of polar water from each polar area towards the Equator. This has been most clearly worked out in the case of the Atlantic basin, which has a communication, as you well know, with both the north polar and the south polar areas. But the communication of the South Atlantic with the Antarctic area is perfectly free and unrestricted; there is nothing whatever to narrow it or diminish it. On the other hand, the communication of the North Atlantic with the Arctic area is a comparatively limited one. You must not draw your ideas on this point from a Mercator's chart. You know perfectly that in a Mercator's chart all the distances of the polar portion of the chart—both the north and the south polar portions—are immensely exaggerated. You should draw your conclusions from looking either at a globe, or at a chart of the polar portion in which the Pole is made the centre. You will there see that the communication between the North Polar area and the North Atlantic is really a very narrow one; the only communication of any breadth being that between Greenland and Iceland, and that is much narrower than you would judge it to be in looking at an ordinary map. Between Iceland and the Faroe Islands there is another communication; but, though the map does not show it, the hydrographer knows that this is a very shallow one. There is a bank running from the Faroe Islands, and almost touching Iceland, that is not more than from 200 to 300 fathoms in depth; and that is really the coast-line of the deeper portion of the polar area. Between the Faroe Islands and the north of Scotland, there is a narrow channel, which was the first scene of our explorations, and which I call the "Lightning Channel." It lies between the Faroe Islands and the 100 fathom line which you will see marked in all charts of the British Islands, and which is the boundary of that remarkable platform upon which all the British Islands are situated. If the whole of our corner of Europe were elevated not more than half as much again as the height of Saint Paul's—600 feet—not only would all the British Islands be brought together, but the land would be extended considerably to the north and west; the North Sea would be almost entirely extinguished, that is to say, it would become dry land, with the exception of a deeper channel near the coast of Norway; and Great Britain would be united with Denmark, with

Holland, with Belgium, and with France, the British Channel being no where 100 fathoms deep.

Now, between the northward extension of this 100 fathom platform, and the similar elevation which forms the Faroe Banks, there is a channel of from 500 to 600 fathoms' depth, to which our course was directed, as the nearest deep water that was to be obtained within easy reach of our own coast. I shall presently state to you the remarkable phenomena which that channel presents. This is one of the feeders, as it were, of glacial water to the Atlantic; but it is a little rill in comparison with the great, vast area of the North Atlantic. I have no doubt whatever that it will be found, if the Admiralty will take means to ascertain it in the intended Arctic Expedition,* that the whole of the lower portion of the channel between Greenland and Iceland, which extends to 1,200 fathoms' depth, and which was sounded from the "Bulldog" some years ago, when Dr. Wallich went out with Sir Leopold McClintock, gives passage to a flow of glacial water—of water at or below the temperature of the freezing point of fresh water (32° Fahr.),—and is the principal feeder of the cold stratum which extends over the bottom of the North Atlantic. But if you consider how narrow is that channel in comparison with the great broad area of the North Atlantic, you will perceive at once that the communication between the North Atlantic and the North Polar area, or Arctic basin, is a very limited one. On the other hand, the communication between the South Polar area or Antarctic basin and the South Atlantic is perfectly free.

Now, it was the study of this channel which first led me to the doctrine which I have since advocated, of the existence of two flows in the great oceanic area, which are quite unrelated to any sensible current. I put out of view altogether the Gulf Stream, or any other of the great surface currents. The Gulf Stream—I mean the true Gulf Stream which issues from the Gulf of Mexico through the Florida Channel—is entirely a product of the action of the Trade Wind on the equatorial Atlantic, driving it into the Carribean Sea and Gulf of Mexico, and then forcing on the current through the Florida Channel to come out as what we call the Gulf Stream. That has a certain superficial spread, but it is a mere surface cur-

* The "Valorous," which has accompanied the "Alert" and the "Discovery" to Disco Island as a store-ship, is charged with the duty of carrying out on her return voyage a series of observations on the Physical and Biological conditions of the North Atlantic.—W. B. C.

rent. The great movement which I am about to describe to you has a much greater depth and a much greater volume. This surface current is, in fact, when you look at it in sectional area, a mere little rill in comparison with the immense breadth and depth of the North Atlantic; and I believe we may say with confidence that the true Gulf Stream dies out in the mid-Atlantic. I must not go into this question farther; but it is one which is of very considerable interest. I shall have the pleasure of presenting to the Association a copy of a paper in which I fully discuss this subject, and, therefore, my views upon it will be accessible to the Members.

We found in the "Lightning" Channel, that there was a superficial warm stream, and a deep cold stream; and that we pass from one to the other within about 50 fathoms, finding a most marked difference of temperature. We found, for instance, that at two places within twenty miles of each other, the bottom had 13 or 14 degrees' difference in temperature; for whilst at one the temperature went down pretty regularly from about 52° or 54° at the surface to about 43° , at the other, it would go down at the same slow rate for about 100 or 150 fathoms, and then would descend rapidly, until it would come down to 32° , or even 30° , at the very same depth at which we found it 43° elsewhere. This is expressed in Fig. I., in which the depths are indicated by the vertical lines, and the degrees of Fahrenheit's thermometer by the horizontal lines.

You will see here that in No. 87, starting from a surface temperature of 53° , the thermometer went down at 100 fathoms to about 47° , and then remained nearly the same at different successive depths down to about 500 fathoms, where it fell to 45° ; between this and 777 fathoms it went down to 41° ; and a further descent would have been shown if the thermometer had gone deeper. On the other hand, in No. 52, taken in the cold area, falling at precisely the same rate in the upper stratum, and reaching 45° at 200 fathoms, it sunk down rapidly to $30^{\circ}5$ at 300 fathoms, and showed the same at 384 fathoms. In No. 64, the upper warm stratum was cooler and thinner, and the descent of the thermometer more gradual; it showed 32° at 300 fathoms, 30° at 500, and $29^{\circ}5$ at 640. Now, what is the meaning of this? It can have no other meaning, in my mind, than that there are two distinct movements of water. Whenever we have sudden and striking

FIG. 1.—Curves constructed from Serial Soundings in the Warm and Cold Areas, the depths being represented by the vertical lines, and the Degrees of Fahrenheit's Thermometer by the horizontal lines.

(Reprinted by permission from *Proceedings of the Royal Society*, No. 121, page 459.)

changes of temperature, we know that there has been a change of wind. We all know, for instance, that a northerly and north-easterly wind brings a low temperature; and if, on the other hand, in the course of a few hours we have a rise of 10 or 15 degrees of temperature, we know that the wind must have changed, and has come from a southerly source. Well, here it is still more certain with regard to the movement of water. It is perfectly clear that glacial water of this kind, much colder than the lowest mean winter temperature of the latitude, could only have come from a northern source. I shall presently show you how much hangs upon that. On the other hand, where we find the temperature higher than the normal temperature of the latitude, the water must have come from a southern source. There can be no doubt, I think, in the mind of anyone, that there was a warm flow—that this surface flow was coming up from the south-west. It would be called in popular language a branch of the Gulf Stream; I shall show you that there is another account to be given of it. At any rate this polar water must have come down from the north-east; and, in perfect accordance with that derivation, we found that the animals of this cold area were all animals of Spitzbergen, Greenland, and Iceland—animals known to belong to the boreal fauna. There was one very remarkable form, the beautiful large *Comatula Eschrichtii*, well known as a Greenland and Iceland form, which we could get in any quantity on this cold area. The shells of the Echinodermata generally, Mollusca and Crustacea, were the animals of the cold area. There was one curious thing about the echinoderms, and that was that we found our familiar British forms dwarfed in a peculiar manner. The common *Solaster papposa* of our own seas, with which anyone who has dredged our own shores is familiar—the twelve-rayed star-fish, of which a fair-sized specimen would be of about the size of a pudding plate—was dwarfed down to about the size of a crown piece. So the ordinary five-rayed star-fish was dwarfed down to about a star of the same proportion. It was not that these were young specimens, because we got any quantity of them, all small—none larger than I have described.

That shows you, then, the influence of Temperature in determining the character of the Fauna. Now, the fauna was an abundant one, showing that even this low temperature of under 30° Fahr. did not prevent an abundance of animal life. Here, then, is

the first condition I have to speak of—that temperature will determine the kind of animal life; but that no temperature we know, as yet, to be found on the sea bottom, is so far inimical to animal life as to prevent its existence. Cold may dwarf the fauna, may limit the variety and number of its forms; but I do not think that, as long as sea water is liquid, there will be any temperature that can be reached, which shall not be consistent with the existence of animal life.

Now, then, with regard to Pressure. I have already spoken of the tremendous pressure under which animals must live, because, of course, the pressure upon animals is the same as the pressure upon a thermometer. We know that the pressure is about a ton per square inch for every 800 fathoms of sea water; a column of sea water one inch in section, and 800 fathoms high—4,800 feet—weighing just one ton. Every animal, then, that is living on the sea bottom must be living under that pressure; but I need not explain to those whom I may presume to have some knowledge of physics, that this pressure, being equal in every direction, will not in the least degree affect any vital functions. You must bear in mind that water is almost incompressible. The difference between the specific gravity of bottom water under three miles' pressure, and that of surface water, will not be equal to the difference between the specific gravity of sea water and that of fresh water, the amount of compression being extremely small. Now, think of how this pressure will act. We will first take the simplest idea of an animal. Let us suppose a globular film—a globular bag of the most delicate possible membrane; and that globular bag has a hole in it for a mouth, with tentacles round the hole. That is just the simplest form of an animal possessing a stomach. Now, if that was globular at the surface of the water, it would be globular under any pressure, because, whatever the pressure outside may be, the pressure inside will be exactly equal to it. The animal can contract and expand just as well under a pressure of three tons to the square inch, as it can upon the surface. The pressure will have no effect in modifying its conditions; and you may judge of that by the fact that we ourselves are living under a tremendous pressure, which very few people realise. A man of my height, for instance, will have an atmospheric pressure of somewhere about 20 tons upon his surface, and a difference of the barometer such as we had last Saturday will exert upon him a difference

in pressure of somewhere about two tons ; and yet in what a perfect ignorance we are left of this change, by any feeling that we experience. You must bear in mind that the reason why a human being cannot be subjected to a pressure of this kind under water, is simply that his cavities contain air. These creatures of the sea are entirely made up of water and of what the water contains. There is no air in them, except the air in solution in the water. There is nothing that can be compressed. If we go down and subject ourselves to the pressure of four or five atmospheres, it is the pressure upon the air in our chest that disturbs the circulation ; the whole of its conditions being altered by the inclusion within our bodies of this elastic fluid—the air. Pressure, therefore, I believe, we may say, has no direct influence on animal life. I will not say that an animal suddenly changed from one pressure to another—suddenly carried down, for instance, from the surface to a depth at which it would have to sustain a pressure of three tons to the square inch—might not be in some degree deranged or disturbed ; or that an animal quickly brought up from that depth to the surface might not be somewhat deranged or disturbed ; still the animals lived, and moved, and walked about when they were brought up from these great depths. And if the change were gradual, I believe that there is no depth at which, as a mere matter of pressure, animals might not exist. But you will presently see that there are some other considerations which enter into this question.

If, then, depth has no influence as regards pressure, if temperature has no influence, you will ask, “What has an influence?” I believe, more than anything else, the supply of Oxygen ; and I will now explain to you the conditions under which, as it appears to me, the deep Ocean is placed, in contrast with the conditions of such an Inland Sea as the Mediterranean.

This under-flow of polar water over the deep sea bottom, of which we have now the most distinct proof from temperature-observations, has been sometimes spoken of as the flow of polar currents. Lenz, I believe, first put forward the doctrine that it is a general flow extending over the ocean-bottom, not limited to any particular passage or area. This was the view to which I was myself led—that it is a general flow ; and that there will conversely be a general flow of the upper stratum from the Equatorial towards the Polar areas ; and it seemed to me that this would depend

upon, or be maintained constantly by, the difference of specific gravity produced by the difference of temperature.

You must, in the first place, bear in mind that sea water is quite differently circumstanced from fresh water, in regard to the effect of temperature upon its density. Fresh water, as you know very well, contracts down to 40 degrees, or about $39\frac{1}{2}$ degrees, and then expands again. So that the water just going to freeze at 32° is really as bulky as the water at about 47° . I have sometimes illustrated it in this way, which perhaps some present will find to convey a more distinct idea to their minds than the mere statement of the fact. Suppose you have what we may call a water-thermometer and a mercurial thermometer side by side—a tube and bulb forming each; you fill the bulbs and tubes up to a certain point, and you note that point. When you immerse both bulbs in a freezing-mixture, the mercury will go on sinking and sinking, until it comes down to the point which you may have previously determined to correspond with the freezing point of water, viz., 32° . The water in the water-thermometer will sink and sink down to about $39\frac{1}{2}^{\circ}$, but then it will rise again. While the water is actually getting colder in the tube, it will rise again, because the water, in cooling below 39° , is also expanding—becoming more bulky; and when just about to freeze, it will stand where it stood when it had been cooled down to only about 47° . But *sea-water* will go on, like mercury, diminishing in bulk down to its own freezing point, not only to the freezing point of fresh water, but down to the freezing point of sea water, which is about 27° if the water is agitated, or 25° if it is kept perfectly still; and it is just before it freezes that sea water has the greatest density—that is to say, it is then the smallest in bulk, or of the highest specific gravity. Now, then, you at once see that if it has increased density with cold, colder water will, bulk for bulk, be heavier than warmer. A column of water at, we will say, 27° will be considerably heavier than an equal column of water at, we will say, 50° ; and, therefore, when there is free communication between two such columns, through the oceanic area, since the polar column will outweigh the equatorial column, and since the lateral as well as the bottom pressure will be greater, there will be a lateral outflow at the bottom of the polar area, which will extend itself continuously towards the equatorial area. Then, on the other hand, this will cause a lowering of the

water in the polar area, which will produce an indraught towards it; and there must, therefore, be a continual flow of the upper stratum from the equatorial towards the polar area. Further, the cold water streaming from each polar area towards the equatorial, and there meeting the polar flow from the other end, will rise up in the equatorial area, to compensate for the draughting away of the upper stratum, and will thus bring cold water nearer to the surface under the Equator than it is anywhere else, except where the surface is itself subjected to cold.

In some lectures which I gave three or four years ago, I illustrated this by a piece of apparatus which I was able to obtain at the Royal Institution, namely, a long glass trough with glass sides. I had a piece of ice wedged in at the surface of one end of the trough, and a heated plate on the surface of the water at the other end. Now, what is the effect? As soon as we put in some coloured liquid at the cold end of the trough, it fell down to the bottom; for by coming into contact with the ice, its specific gravity was so much increased that it at once sank; it then crept along the bottom towards the warm or equatorial end, and it came up under the heated plate. Being then heated by contact with the plate, it went on, as warm surface-water, from the equatorial to the polar end of the trough, to be again made to descend by cold. You see at once that this circulation would be maintained as long as the opposition of temperatures continues.

Now, I further reasoned in this way. If this be true, the bottom temperature of the South Atlantic will be lower than that of the North Atlantic, because the bottom temperature will depend upon the amount of glacial water which will flow along the floor of the great ocean. In the North Atlantic, that amount is limited by the narrowness and comparative restriction of the channels which I have described to you; while in the South Atlantic there is a free flow from the Antarctic area into the South Atlantic. You may therefore imagine the interest with which I looked at the returns of the "Challenger" when they first were sent home to our Admiralty. I found precisely what I expected. The accompanying section (Fig. 2) is not one given in the "Challenger" report, but it is one which I have constructed from their report. Their sections were taken in a sort of zigzag, for they passed four times across the Atlantic.

In constructing this diagram, I selected from the various tem-

FIG. 2.—Section of Mid-Atlantic. Taken nearly North and South.

perature-soundings of the "Challenger," those which would best combine into a line passing nearly north and south, from 38° N. lat., through 22° N. lat., to the Equator, and thence through 7° S. lat., and 20° S. lat., to 38° S. lat.

The general temperature of the North Atlantic bottom was shown by numerous temperature-soundings to be about $35\frac{1}{2}^{\circ}$ or 36° . It was nowhere found lower than that, except near Halifax. As they went north, towards the Polar channel, they got down to 35° ; but they also got down to 35° and even 34° as they approached the Equator, near St. Thomas's. That was the first section which we received, and I said at once "There is the influence of the Antarctic flow." I quite expected that the Antarctic flow would extend to the north of the Equator, for it was so much more voluminous than the Arctic flow. Then when we got the South Atlantic sections, I saw at once that my surmise had been perfectly correct, for we then found that under the Equator we got the lowest temperature that we anywhere met with, viz., 32.4 . The sections of the South Atlantic were taken at wide intervals, for the sea was very stormy at the time they crossed it. There was only one section made across the South Atlantic, and they did not hit upon the channel which brought in that water at 32.4° . There was no other section which was much below 34° or $33\frac{1}{2}^{\circ}$, but clearly, of course, that water came from the Antarctic. And what is most remarkable is the line of 35° , which can be clearly traced in the South Atlantic across the Equator, and then gradually slopes down in the North Atlantic till you lose it altogether.

Now that, I think, is as clear an indication as anything can be of this great under-flow of Polar water in the Atlantic,—the greater mass of polar water coming in from the Antarctic area, and the smaller mass of polar water coming from the Arctic area, which becomes diluted, as it were, by the water of the North Atlantic itself, so that the bottom temperature of the North Atlantic is higher, by about 3 degrees, than the temperature of the South Atlantic.

But the special point that I would have you dwell upon is this—that the isotherm of 40 degrees of temperature comes nearer to the surface under the Equator than it does anywhere else. At 22° N. lat., the line of 40 degrees lies at about 700 fathoms, and in 38° N. lat., the line of 40 degrees still lies at 650 fathoms. But from 22° N. lat. to the Equator there is a gradual ascent of this line of 40° , till under the Equator it comes to within 300

fathoms. Going down 300 fathoms, from a surface-temperature of 75° , you actually come down to a temperature of 40° . In the South Atlantic the line of 40° , though lying deeper than under the Equator, does not lie so deep as it does in the North Atlantic; and that it does not lie so deep there is, I believe, owing to the far larger mass of polar water which comes in from the Antarctic area, and the thinning out of the upper warm stratum as it moves from the Equator towards the South Pole, by the recession of its land-borders, whilst in the North Atlantic, by the approximation of its land-borders, it is pressed together laterally, and is therefore increased in thickness.

Now, you at once see why—on the doctrine I advocate—this cold water comes nearer to the surface under the Equator. It is because the warm surface water is continually being draughted off towards each Pole, while the cold water is rising up from below.

A very remarkable confirmation of that view is obtained from comparison of specific gravities. The density of surface water increases from the Polar area towards the Tropics; but the density of the bottom water remains very much the same at the tropics as at the polar areas; and that is just what you would expect if all this bottom water comes from the polar area. I am speaking now of salinity. You would expect that the bottom water should have the low salinity of the surface water of the poles. And so it has, we may say, speaking roughly, when the specific gravity is corrected for temperature. Of course, all such comparisons are made with reference to a uniform temperature. You may take your temperature to be what you please. Reduce it to 32° , or have it at 60° , or any other standard you like, so that you keep the same standard. Let us say that the specific gravity of Polar water brought to the standard of temperature is 1026. Well, the specific gravity of the surface water, in consequence of the evaporation to which it is subjected,—increasing, of course, with its approximation to the Equator,—is continually augmenting, we will say, to 1027. Putting aside fractional parts, there is just about that increase—from 1026 to 1027. But the salinity of the bottom water remains the same; and under the Tropics you bring up from the bottom water of 1026. You will ask, How is it that this underlies? Why, because it is heavier in consequence of its low temperature. The specific gravity that I am now speaking of is the comparative specific gravity of water all brought to the same temperature; but the actual specific gravity of the bottom water is greater, although its salinity is less,

the reduction of its temperature more than compensating for its inferior salinity, and therefore it underlies the other.

But I wish now to impress upon you the curious fact that as you pass from either Tropic towards the Equator you find the specific gravity of the surface-water diminishing. This is a fact which was observed as long ago as Humboldt's voyages; and the "Challenger" has found that the salinity of the surface-water under the Equator corresponds closely with that of the bottom-water. Thus while the mean of eight observations between Bermuda and St. Thomas's gave 1027·2 as the sp. gr. of surface-water, and 1026·3 as the sp. gr. of bottom-water, a mean of seventeen observations on surface-water in the Equatorial zone gave a sp. gr. of 1026·3, whilst eight observations on bottom-water gave a mean sp. gr. of 1026·1,—showing that the salinity of surface-water had fallen under the Equator to that of bottom (polar) water.

Now, taking that in connection with the influence of temperature I think you will see very clearly that the facts which I have stated with regard to temperature at once explain the constant rise of bottom water under the Equator towards the surface. Lenz was, to a certain degree, acquainted with these facts; and adduced the low salinity of equatorial surface water as evidence of the rise of polar water from the bottom. And Lenz pointed out from a great series of temperature observations, taken at from 200 to 300 or 400 fathoms' depth, that there is a cold band of water under the Equator, colder than any water to the north or the south of it; and upon that he framed a theory of ocean circulation, precisely corresponding to that to which my own observations have led me, and which I think you will see has been entirely confirmed by the result of the "Challenger" survey of the Atlantic.

Now, you will say, "What has this to do with animal life?" I believe it has everything to do with it; and I will show you why. This ocean circulation brings every drop of water in turn to the surface. There is no other way in which the carbonic acid imparted by animal respiration and animal decomposition can be removed, and oxygen substituted, than the aëration of the water at the surface. You know perfectly well that even in our shallow aquariums it is necessary to have a constant agitation of the water to bring the water to the surface and expose it to the aërating influence of the atmosphere; and the best provisions are those by which water is spirited up, a continual subdivision of it, as it were, being obtained by this movement, so as to get rid of the carbonic acid,

and to allow it to take in a fresh supply of oxygen. We made gas analyses of the bottom water. I do not say these gas analyses were made in a manner altogether satisfactory. They were made according to the mode laid down for us by some of our best chemists. The water was boiled—subjected to prolonged ebullition; the gases were very carefully collected; and then they were examined chemically, and the percentages pretty accurately determined. The source of error lies in the extreme tenacity which sea water has for carbonic acid, but still the quantity retained would be pretty constant; and therefore we may say that, if the proportions that were got did not absolutely represent the percentages, yet the differences that I am about to state were just as good, and just as important and significant, as if we had obtained the actual percentages; just in the same manner as our first series of thermometric observations made with unprotected thermometers, which showed a difference of 13 degrees between the warm and the cold areas at corresponding depths, were just as good as later observations, although in the first case our actual temperatures were not accurate. We found in the subsequent years with our protected thermometers exactly the same *differences* of temperature, but our protected thermometers carried down the temperature about 3 degrees lower. So here, if we did not get all the carbonic acid, we still got all that could be obtained by boiling the water; and what remained was a certain fixed or constant quantity.

Now it is very remarkable that marine animals can live and move and have their being in an atmosphere containing 40 or 45 per cent. of carbonic acid. I have my doubts whether this carbonic acid does not exist in a liquid state diffused through the water, rather than in the gaseous state in which, as we know, it exerts its most prejudicial influence upon animal life. But still we had from 16 to 20 per cent. of oxygen; and as long as we had that amount of oxygen, the animals, I believe, got what they most wanted. That was our experience in all our own observations upon the gases of the deep sea area,—a series of observations taken off the coast of Ireland and the coast of Scotland, going down to depths of over 2,000 fathoms in some parts.

Now, when I went to the Mediterranean, I found an entirely different set of conditions. Knowing that the bottom temperature would probably be considerably higher, I fully expected to find a Tertiary fauna. We had previously, as you know, obtained a number of forms of animals corresponding to those of the Chalk.

The *Globigerinæ* we got up by the ton,—*Globigerina* mud which, when dried, could not be distinguished from chalk; and this contained siliceous sponges, which remind every geologist of the ventriculites of the Chalk, and a great many other Cretaceous forms. Going through the Mediterranean, which we knew to be a sea of the Tertiary period, I expected to find a Tertiary fauna over the whole of its bottom; and what was my disappointment—I say “my” because my friend Dr. Wyville Thompson could not accompany me on this cruise—what was my disappointment at bringing up a viscid mud not containing animals at all. We devoted a great deal of time and labour, and did not get even any minute Foraminifera. Taking this mud between our fingers, and working it, I found scarcely anything but fine impalpable particles that gave no indication of organic forms. A few little fragments we found now and then, very few indeed, and very small; but excepting near the Strait of Gibraltar, we got no living animals—I am speaking of the depths of the Mediterranean—save in shallow water. Now, the Mediterranean goes down in its western basin to 1600 fathoms, and in its eastern basin to 2000 fathoms, between Malta and Crete, for instance, and this depth extends a good way towards the Levant. The bottom temperature of the Mediterranean is uniform, or very nearly uniform, from 54° to 56° . And here, you see, is a marked contrast between the thermal condition of the Mediterranean, a sea almost as deep as the Atlantic, and that of the Atlantic itself.

Now, what is the reason of this? It is because this extraordinarily deep basin of the Mediterranean is cut off from the Atlantic by a shallow channel. If the Mediterranean area were elevated only 200 fathoms—1200 feet—it would be converted into two extremely deep lakes, inland seas, like the Caspian. It would be cut off entirely from the Atlantic at the western embouchure of the Strait of Gibraltar, the ridge line between Cape Spartel and Cape Trafalgar; and the two basins would be separated by the bank which connects Sicily and the coast of Tunis. Still these basins would be of immense depth. Deduct 200 fathoms from 1600 fathoms, and it leaves 1400. Deduct 200 from 2000, and it leaves 1800. Neither area would be much diminished by this elevation, because there is water of 200 fathoms’ depth at a very small distance from land, on almost every part of this basin. What is the reason of this curious difference of temperature? Why, simply because the Mediterranean is cut off entirely from the influence of the

polar underflow. Off Lisbon this underflow, taking the temperature of 40° as its limit, lies at a depth of more than 700 fathoms.

Comparing the thermal condition of the Mediterranean with that of the Atlantic, we find that whilst the surface-temperature of the Atlantic under the same parallels ranges in summer between 65° and 70° , that of the Mediterranean ranges between 70° and 80° . At the first hundred fathoms there is a drop down to 55° ; but from this to the bottom, at whatever depth, there is no change at all. Below a stratum which is variable in temperature, the whole mass of the water is uniform in temperature; and this ranges down to 54° , 55° , or 56° , according to the latitude, being 56° in the eastern basin, 55° about Malta, and 54° towards Gibraltar.

We find this to agree with the mean winter temperature, or the isoeimal of the latitude; which, as shown by observations made at different points, gives 54° , 55° , and 56° , along that area of the Mediterranean. In the winter, the temperature from the surface to the bottom is equal: I do not mean to say within a degree, but close upon that. As soon as the atmosphere becomes warmer, the sun has more power. It shines upon the surface, and raises the temperature of the superficial layer, thereby producing increased evaporation. This evaporation, by rendering the superficial film more saline, increases its specific gravity; and it sinks down until its increased salinity is lost by diffusing itself through the water below. Practically we find that the surface heat is almost entirely limited to the first 50 fathoms; a little goes down to 100; and in the eastern basin, where the powerful action of the sun continues longer, I could trace it down to 200 fathoms; but the most powerful action is on the first 50 fathoms of the surface; and we may say practically that it scarcely reaches 100; and that from 100 fathoms downwards there is no difference in temperature whatever.

Now, then, what effect will this produce? Why, that the whole of the lower portion of the Mediterranean, below the influence of the Gibraltar current, which is limited to, we will say, 100 fathoms, is a stagnant pool. This, I take it, is the explanation of the absence of animal life upon the Mediterranean bottom. In my first year I was rather disposed to think it was the deposit of fine mud; and I do not say that this has not an influence. I found, as I mentioned to you, over the whole of the deep bottom of the Mediterranean, a deposit of fine mud gradually settling down, the particles being so fine that they would pass through the

ordinary filtering paper. When I wanted to clear the specimens of bottom water for the determination of the salinity, I found it difficult to do so. I had to pass the water through the filter three or four times till the pores of the paper became clogged, so that it should stop these very fine particles, and allow the water to go through clear. That gives you an idea of the extreme fineness of this sediment, slowly, slowly settling down upon the bottom of the Mediterranean. Now, where does this come from? A portion is, no doubt, the result of the attrition of the waves upon the soft Tertiary strata which form the greater part of the shores of the Mediterranean; but the greater part, I believe, is the clay brought down by the two great rivers, the Rhone on the western basin, and the Nile on the east. The coarse particles brought down by these rivers are deposited near their mouths; but the fine particles, being suspended for a much longer period, are dispersed by surface-currents over the entire area, and gradually settle down to the bottom. This, no doubt, would be inimical to the presence of animal life. Those of you who have read Mr. Dana's book on Corals, will remember that he mentions that where clay is brought down by a river, and deposited on a coral bed, it kills the coral. And I believe that, generally speaking, this is found to be the case with regard to most forms of animal life, especially bivalve Mollusca. The fine mud, settling down upon their gills, clogs their filaments; so that they will not thrive in places where there is much of this silt deposited upon them.

But on thinking over the general application of this doctrine of thermal circulation to the case of the Mediterranean, I saw that there could be no thermal circulation in a sea in which the temperature is uniform from the top to the bottom, with only this exception, that the surface layer is heated during the summer and cooled during the winter. There will be nothing to send the cold water to the bottom, and there will be nothing to send the warm water to the top. In the next year, when I visited the Mediterranean again, I took out with me apparatus for gas analysis; and, though not very proficient in such work, I was able still to make comparative experiments that were sufficiently satisfactory, for I found that the proportion of carbonic acid, instead of being from 40 to 45 per cent. was from 60 to 65 per cent.; and the proportion of oxygen, instead of being from 16 to 20 per cent., was only about 5 per cent. The nitrogen was about the same as in the ocean.

You will see at once why this should be. The rivers bring down with the clay a large quantity of organic matter, and that organic matter uses up the oxygen and converts it into carbonic acid ; and in this manner the deep water of the Mediterranean becomes loaded with carbonic acid, and is correspondingly deprived of its oxygen. You will, therefore, I think, see at once why there must be a deficiency of animal life upon the bottom of this deep sea, and how it is that this general oceanic circulation of which I have spoken has an important influence—the most important, in my mind—in maintaining organic life upon the bottom. I was going to say to any depth ; but I have been reading only this afternoon a paper by my friend Professor Wyville Thomson—the last communication he has made to the Royal Society—which gives a most curious qualification to that. It will appear that below a certain depth—about 2,200 fathoms, or it may be a little lower than that—there is a solvent power. I do not say that this is yet proved, but this is his belief from the character of the bottom at that great depth. Mr. Sorby has shown us how mechanical pressure will influence the solvent power of water. It is thought that at that great depth the water has a solvent power for the shells of *Globigerina*, no mass of *Globigerina* shells being found below a certain depth ; but, on the other hand, there is first a *grey ooze*, and then there is a *red ooze* corresponding to greater depths. The grey ooze is at a greater depth than the *Globigerina* mud, and the red ooze is at a greater depth than the grey ooze ; and it is Professor Thomson's belief that this solvent power of sea water at great depths causes it to act upon all organisms having calcareous matter as their skeletons, and that it may act also upon siliceous skeletons ; but he believes, at any rate, that pressure does produce an influence upon animal life, not on account of any direct influence, but through its solvent power for calcareous matter.

There is another interesting observation contained in the same paper, which shows how extremely important all this careful examination of the sea bottom is to geologists. You know very well, no doubt, that Professor Ehrenberg first brought into view some years ago the fact that the green sands which occur in various geological formations mainly consist of the internal casts of Foraminifera ; the body having been replaced by a deposit of silicates, and the shell having been subsequently dissolved away. It was first ascertained by the late Professor Bailey, of the United

States, that that very process is going on at the present time. He found in Foraminifera dredged up in the Gulf of Mexico exactly the same thing—that the cavities were occupied by glauconite, or some other green or ochreous silicate. In consequence of these observations, my friends, Professor Rupert Jones, and Mr. Parker, who had in their possession the dredgings of the late Professor Jukes upon the Australian coast, put these into acid, dissolved away the shell, and got out a series of the most wonderfully beautiful casts, which furnished the foundation of my own interpretation of the structure of *Eozoon*; because I look upon the peculiar structure of *Eozoon* as being due to a change of precisely the same character, namely, the replacement of its sarcodic body by a green silicate, usually serpentine—a silicate of magnesia. Now, I have obtained similar casts from elsewhere. The dredgings of Captain Spratt from the *Ægean* gave me a series of most beautiful casts, of a smaller size than those of Professor Jukes, but equally perfect. The observations recently made in the “Challenger” show that all along the line of the Agulhas current there is a green sand covering the bottom. Now, as Professor W. Thomson says, we should simply have noted this as green sand, if we had not had in our mind the observations of Professor Ehrenberg. Looking at this green sand with a microscope, we are able at once to recognise the grains as almost entirely the internal casts of Foraminifera. The question arises, however, how the shells were got rid of; you will see at once how it is, if they have lain on a bottom deep enough, supposing this doctrine is true. It will have to be tested; and I think the proper test will be to subject calcareous organisms to the pressure of the same hydraulic press which we have used for testing our thermometers, and to see whether sea water under this pressure will dissolve all calcareous structures. If it should do so, you at once see how these green sands have been formed. A process of chemical substitution—a process which I do not pretend to explain, but which all my friends amongst the most able chemists of this country agree with me in regarding in that light,—during the decay of the body of the animal, has effected a replacement, particle by particle, of the organic sarcodite by these silicates. When that change has filled the chambers of these Foraminifera with these silicates, and the shells have been dissolved away by the solvent power of sea water under this tremendous pressure, there you have the green sand. If this view should be borne out

by experiment—and I think it is experiment that is to determine it, and the experiment is tolerably easily made—I think you will say that this is one of the most beautiful applications of this kind of research to the explanation of geological phenomena that has ever been brought forward.

I have to apologise for having trespassed so long upon the time of this Association, but I do not think that I have said anything that can be regarded as superfluous; and it is difficult to convey a general doctrine of this kind, without laying one's foundation sufficiently over the whole area, as it were, that has to be covered. I might have gone into the history; I might have spent a good deal of time in telling you of my old friend Edward Forbes; but I must notice the one curious fact—the limitation to 300 fathoms which Forbes's work on the *Ægean* brought out, and which was supposed for a long time to dominate the life of the Ocean, is perfectly true of the *Ægean*. It is perfectly true of the whole of the Mediterranean in shallow water down to 250 fathoms. In our own researches we obtained exactly what Edward Forbes obtained—an abundant life in the littoral zone, a zone within 50 fathoms; a diminishing life down to 100 fathoms; a rapidly diminishing life from that down to 250 fathoms; and then a zero. Excepting for the vegetable *Nullipore*, which used to be called coralline, I should fix the zero, or the limit of life, in the Mediterranean at 250 fathoms; but then you will bear in mind that that limit, if my view is correct, is not a limit of pressure, or a limit of heat, or a limit even of food (though circulation will of course have the same effect in carrying down food, as in carrying down oxygen; it will carry down the surface food, that is, the organic matter which is produced by surface vegetation): the limit is mainly the limit of oxygenation. The thermal circulation is essential to the maintenance of oxygenation; and therefore the condition of an inland sea cut off from the great oceanic area is altogether different from that of the oceanic area; and any deposit that is being formed in an inland sea we may expect to be without any fossils, excepting those which represent the animal life of its shallower portions.

I may mention a confirmation of this view, which I have myself met with. You know that Malta is chiefly a calcareous formation—Middle Tertiary; a good deal of it—I believe most of it—Miocene and later; but it is almost entirely calcareous. Some of the calcareous beds contain quantities of fossils—Echinodermata,

Mollusca, Corals—and these in a beautiful state of preservation. These beds are generally rather coarse. On the other hand, there are beds of very fine texture—texture so fine that it corresponds very closely indeed with the fine calcareous mud which I dredged up where it is least clayey. I could see at once that this fine mud varied a great deal in its composition; and on testing it with acids I could prove it at once. In some parts it was chiefly calcareous; in other parts it was aluminous—clayey. The very fine sandstone of Malta corresponds closely with the finest calcareous deposit of the Mediterranean. Now, this stone is used a good deal for building; but the finest beds of it, which occur in the island of Gozo, are used by the carvers who make the beautiful carvings that I dare say many of you have seen in this extremely fine “sandstone,” as they call it. I asked the man who makes the largest carvings in Valetta, “Do you find any fossils in this stone?” “No, sir,” he said, “nothing but sharks’ teeth.” I repeated the inquiry when I went the second time, the significance of this having occurred to me more strikingly. I got all the information I could from all those who had paid any attention to the geology of the island, and I received precisely the same answer—that the fossils are found in the comparatively coarse beds which are, I believe, the shallow water beds, and that this bed which is composed of the excessively fine “sandstone,” as it is still called, though chiefly a calcareous deposit, contains no fossils but sharks’ teeth. Now, of that you at once see the *rationale*. There is no life on the deep sea bottom, on which the finest particles slowly subside, and there are no fossils but those which would drop into it from above.

Now, of course, this view, if it is taken up by geologists, will have a most important influence upon geological inquiry, especially in its bearing upon a doctrine which my friend Professor Ramsay is well known to advocate. Certainly the extraordinary paucity of fossils in a very considerable number of strata, especially the Red Sandstones of inland seas—at least in many parts of those beds—does seem to correspond very closely with the facts which I have stated to you as to the absence of animal life upon the Mediterranean bottom. Of course everything will turn upon the depth of the communication between the inland sea and the ocean—whether the inland sea has any communication at all, and whether such communication is one of any depth. I might tell you of a number of cases which I have collected, where, by the comparison of

temperature, I am able to say with considerable certainty what is the depth of the communication between the inland sea and the sea outside. I find the temperature going down to a certain point, the same as in the sea outside, and then continuing uniform to the bottom. Where that is the case, I believe I can say that the communication between the inland sea and the ocean outside stops just at that point, and the depth at which we come upon the permanent temperature will correspond with the depth of the communication. It is so with the Mediterranean, and it is so in many other cases, so that of course the depth of the communication will have a great influence upon the degree in which the oceanic circulation will penetrate, as it were, into the inland sea; but where the bottom to any considerable depth is cut off from the oceanic area, there, I will venture to say, we shall have a paucity of animal life. I do not say an absolute deficiency, because I have reason to believe that in the Red Sea there may be a rich fauna even on the deep bottom. You may ask why this should be, since the Red Sea is cut off from the Persian Gulf by the Straits of Babel-Mandel, which are shallow. The reason of the difference between the condition of the Red Sea and the condition of the Mediterranean is that into the Red Sea no rivers discharge themselves. The Red Sea has a rocky border, and there are no rivers to bring down the *débris* and organic matter of the land. The Red Sea, therefore, will be a very pure sea. There will be less stagnation at the bottom, as there is an agitation of the surface from the large amount of water that streams in owing to the enormous evaporation of the Red Sea, which has been estimated to be nearly 22 feet per annum, so that if the Straits of Babel-Mandel were closed up, the Red Sea would be dry in two or three hundred years. I say, then, that with this large quantity of water going in, and no organic matter being brought down by rivers to use up the oxygen, I can very well believe that there is quite enough movement of the water to carry down to the bottom a sufficient supply of oxygen for the maintenance of animal life. I do not think, therefore, that this would apply to the Red Sea; but to a sea, such as the Caspian, into which great rivers discharge themselves, I believe that this would apply most completely; and I look forward with great interest to the time when these observations may be carried out, and these questions put to the test—the only sure test, the verification of actual conformity to fact.