LV.—*Laboratory Experiences on board the "Challenger."*

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[A Lecture delivered before the Chemical Society, February 21st, 1878.]

IN commencing my lecture upon laboratory experiences on board the "Challenger," it will be fitting that I should describe the laboratory in which they were made.

The laboratory was situated on the main deck, just abreast of the mainmast, and therefore almost exactly in the middle of the length of the ship, on the right-hand side looking forward. It was lighted and ventilated by a port, which under ordinary circumstances would accommodate a gun. Besides this window or port there were windows into the deck, capable of supplying a certain amount of borrowed light. Against the foremost partition was fitted a locker, the top of which formed a seat, and indeed occasionally a bed, and the interior formed a storehouse for instruments and spare apparatus. Above it, and fixed to the same partition, were two book-shelves, which accommodated the greater part of our chemical literature. In front of the seat was a writing-table, attached at one end by hooks to the one sash of the window, and supported on the other end by folding iron legs. It was capable of easy removal, and indeed was only "shipped" when the laboratory became a study. On the other side, in front of the seat, was a blow-pipe table, of common deal, with a double action circular bellows, of eight inches diameter, underneath.

A tallow lamp was supplied to me for use with the blowpipe, as being the kind of lamp used by glass-blowers by trade. No doubt, for such persons, it is suitable enough, but in a laboratory a blowpipe is wanted often for a few minutes at odd times, when it would be impossible to spend the time necessary for the preliminary melting of the tallow, and I finally adopted a spirit-lamp, made out of a wide-mouthed four-ounce bottle, with a piece of metal made to take a wick just as broad as the mouth would accommodate, and with it it was possible to do every sort of glass blowing that was necessary. The blowing of lead glass was especially easy with it. The wind from the bellows was directed across the burning wick by means of a glass jet in the ordinary way. For delicate glass blowing an arrangement of this sort is preferable to any of the forms of gas blowpipe, because there is not the glare of the smoky flame, and moreover lead glass can be worked with

the utmost facility without blackening, and those who have conquered their first repugnance to lead glass, and have learned to work it, know how much pleasanter it is to handle than even the softest German glass.

On the sternward side of the window was the working bench, which was constructed of teak. The height was 3 feet 10 inches, which was found extremely convenient for working at, as there was no necessity for bending. It was fitted in the usual way with drawers. The reagents also were accommodated in drawers. Each bottle was partitioned off, so that in the rolling of the ship they could not roll against each other. For smaller bottles these drawers were divided in the middle, so as to form two trays, one behind the other. This arrangement was convenient, because a drawer filled with reagent bottles could thus be taken out and put upon the bench for use. It was not liable to be disturbed by the rolling, because it was kept in its place by the battens which surrounded the table. In order to support flasks or apparatus of any kind, it was of course impossible to take the ordinary retort stand of the laboratory, because it would be difficult to fix it in such a way as would not be absolutely permanent and still secure. I therefore adopted this stand, taking the metal rod, half an inch in diameter, which is used for retort stands, having a longitudinal eye forged upon it like the eye of a needle, which was then fastened into the beam here by an eye-bolt, and had therefore a certain amount of play up and down. The rod was then shoved into a hole in the thick teak board on the top of the working bench, the retort stand rings and lamp, such as a spirit-lamp, being slipped up from below before having the rod put into the hole in the top of the bench. This was an exceedingly convenient apparatus, because when not in use it could be slipped up, and the rings taken off from below, and the rod put up against the roof with its end in a hook overhead, and therefore out of the way. This was used then for setting up apparatus such as that for determining the carbonic acid, to which I shall refer presently. For drying and evaporating, a cast-iron gimbal support was fixed to the ship's side. It was capable of carrying a sand-bath or water-bath, or drying chamber, always in a level position.

In the corner there was a sink, that is to say a pipe, which led away to a scupper, draining water off the main deck. Distilled water, supplied by the boiler of the ship, was kept in a bottle, and always ready for supply.

The glass apparatus was arranged in the ordinary way, on shelves with double perforation to keep each article in its place. Up above, the beams which support the deck were of considerable dimensions. They projected about 9 inches down from the deck above, so as to afford considerable room between them. These were all taken

advantage of for putting away apparatus and articles which were not in constant use.

The burettes were fixed to the shelves. They resembled graduated pipettes, the lower end being fitted with a pinchcock. To the upper one an india-rubber tube could be fixed with a glass tube for sucking. When any reagent was going to be used in the burettes, a glass tube of such length as to reach well down into the bottom of the bottle was attached to the point of the burette, and plunged into the liquid, which was sucked up into the burette.

One of the reagents in constant use was baryta-water for the determination of the carbonic acid in the sea-water. By using this method of filling the burette, so that none was spilt on the neck of the bottle, no more liquid was used than was absolutely necessary, and no more air got into the bottle than there was liquid taken out of it. A bottle holding rather more than a litre would last two or three months; and, taking a portion each day, its standard did not alter sensibly.

The remaining arrangements are not of any special interest, and need not therefore be further referred to.

The principal defect of the laboratory was the want of means of removing noxious vapours. From the position in the ship it was found impossible to provide any kind of draught chamber, the only means of ventilation being by the door or window; hence mineral analysis of any extent was rendered impossible.

The dimensions of the laboratory were ten feet long and five feet eight inches wide, the height being exactly six feet at the highest part.

The space therefore was somewhat confined, but for one worker it was quite sufficient, and after getting accustomed to the motion of the ship, there was no great difficulty in working in all ordinary weather. In fact, as long as the weather was not so boisterous as to make it necessary to close the outside port, the work could always be done. With the port closed, light and ventilation were shut out and work became impossible on that account alone, even supposing the ship had been steady enough.

Having thus described the nature of the laboratory, I shall go on now to notice the experiments made with these instruments. Before starting on the expedition, it appeared to me to be of great importance to have some means of controlling the depth as ascertained by the sounding line. The records of deep sea soundings contained depths of as much as 8,000 fathoms, which had been obtained by various observers without bottom having been reached. This was explained by saying that in the ocean if there is no current at the surface, there is such a multitude of under-currents going in ways which cannot be seen or

estimated, that all sounding in this way must be fallacious. Of course having no practical experience of it myself, I could form no opinion, but I thought it was desirable to endeavour to have some means of checking the depths as given by the sounding line, and the first and most ready means which occurred to me was to adopt the barometric method. After studying and experimenting in various directions, I decided to adopt as a measure of the pressure the apparent contraction produced by it in a mass of water enclosed in a glass envelope.

During the spring of 1872, I made experiments on an instrument of special dimensions in the deepest water which was to be had near home, namely, in Loch Lomond, where the depth in some parts is as much as 105 fathoms. I thus found that the method was capable of practical application. In Fig. 1 the piezometer is depicted in

Fig. 1.

section. It consists essentially of a thermometer-shaped instrument open at the end. A cylindrical bulb, A, contained, in the one that was chiefly used, about 9 c.c. The stem, which was rather more than a foot long, had a diameter of almost exactly one millimeter. The end of the stem dips into the bulb, D, which was filled so far with mercury, and the instrument was set by heating it to such a temperature, that when it cooled down to the atmospheric temperature the mercury would rise to a convenient height so as to be visible and to be able to be read at any moment at a given temperature by plunging it in water. The arrangement for protecting the open end of the instrument is somewhat peculiar. It is necessary to allow the water on the outside to get access to the mercury in the bulb in order that the pressure may exert itself, in the interior of the instrument, in the same way as air must have access to the mercury in the reservoir of the barometer. At the same time it is of importance that the mercury should not be able to come out of the bulb. The means, therefore, that have been taken for this purpose was to have a bulb, D, blown, into the

neck of which the stem of the instrument fitted with some accuracy. This was connected with the stem by means of a piece of india-rubber, which was prevented from fastening hermetically on the stem by having a small piece of glass rod, H, shoved in between the india-rubber and the stem. In this way communication was constantly kept open between the outer water and the mercury in the bulb.

The stem of the instrument was divided into millimeters, and was carefully calibrated, the weight of the water filling the instrument, and also the coefficient of expansion of the glass were at the same time determined.

If the position of the water-mercury meniscus in the stem be noted under observed conditions of temperature and pressure, and the instrument be then observed under different conditions of temperature and pressure, the apparent volume occupied by the water, and therefore the position of the meniscus, will depend on the difference of the combined effects of temperature and pressure on the water and on the glass. This resultant effect is measured by the position of a magnetic index similar to or in fact exactly the same as that used in Six's thermometer. The deep-sea thermometer which was used was after Six's pattern, with a protected bulb. When the instrument is subjected to increased pressure or diminished temperature, or both together, the index is pushed up by the mercury, which enters owing to the decrease of temperature and the increase of pressure, and its position thus gives us the sum of the effects of change of pressure and of temperature on the apparent volume of the water.

If now, along with this instrument a sufficiently protected thermometer has been attached to the line, and its readings are taken at the same time, we have a measure of the temperature to which the instrument has been subjected. Knowing the dimensions of the instrument in every particular, and its behaviour under varying conditions of temperature, we can subtract from the whole reading of the instrument that which is due to temperature, and the remainder is that due to pressure. If we know the coefficient of apparent compressibility of the liquid, the depth is given at once.

Now before starting, I thought that the pressure would be more accurately measured by means of a gauge attached to a hydraulic pump, such as that used by Mr. Casella for testing the strength of the protected bulbs of the deep-sea thermometers. Mr. Casella was good enough to put his pump at my disposal for experiments on my original instruments, and their performances were satisfactory.

After we had been at sea for some time, and had made some experiments on the sounding line, it became evident from the readings that the depths so found were not to be relied upon—that in point of fact the gauge which had been used to determine the pressure was not a trustworthy standard, and on reflecting upon the matter, it became evident that in cases where one has a satisfactory sounding, there is no better gauge of pressure than the length of the sounding line. I consequently then directed my attention to determine the apparent compressibility of distilled water and some other liquids by means of the sounding line, that is to say, using the sounding line as the gauge of pressure, and taking particular care to observe that these experiments were made when the sounding was not vitiated by perturbing causes. When currents are present, they are always very evident from the behaviour of the sounding line. If the sounding line

remains vertical during the whole of the sounding, then we may be perfectly certain that there is no disturbance from currents either at the surface or below. If there is a current of any appreciable force, the sounding line begins to wander about, and has to be followed by the ship. This is an operation of considerable delicacy even in good weather, and in bad weather, when the winds and currents cross and complicate each other, it is one which calls for the highest skill on the part of the officer in charge. There is therefore no difficulty in determining if a sounding has been good, and only such soundings free from vitiation by any of the above-mentioned perturbing causes were used for our purpose.

It was only after we had been for a considerable time at sea—in fact, when we were really on the way home, that I was able to make a sufficient series of experiments with this instrument to determine satisfactorily the apparent compressibility of water. The instruments occasionally got out of order, and other work prevented my immediately putting them right. Then it was necessary to get a certain amount of skill in working, because repairing instruments of this kind especially at sea was not learnt in a day. The indexes worked very well in the deep-sea thermometer containing a solution of carbolic acid, which appears to preserve the elasticity of the hair (the spring of the index is a human hair, which clings to the side of the tube and keeps it in position), but not so well on being immersed in distilled water or saline solution, which seems to have the effect of impairing the elasticity in a certain length of time. The consequence was that the indexes began to get loose, and in the unavoidable jars of sounding they were shaken from the position which they had taken up at the deepest part, and became untrustworthy. And here is where the usefulness of the removable mercury bulb into which the stem dipped became apparent. Supposing the end had been permanently fitted with any of the glass blower devices, such as a double safety bulb, there would have been no means of getting at the inside of the instrument, which would thus have become useless as soon as the index had become loose. By the very simple operation of removing the instrument from the ebonite backing, slipping the bulb off and warming the instrument to expel the mercury from the stem, the index could be taken out with a magnet and a fresh hair put on and the instrument set again, when it was as good as new.

In this way, then, a number of experiments were made when the ship was on the way home between Tahiti and Valparaiso, in a long stretch of water of moderate depth, that is to say, between 2,000 and 2,500 fathoms, where the soundings were always very satisfactory; the weather was tolerably good, and there were no apparent currents at all. Fifteen very satisfactory soundings were thus obtained with

this instrument, and the result was to give the coefficient of mean apparent expansibility of distilled water in glass as 0·0009 per 100 fathoms of average sea-water. This corresponds to 0·0000491 per atmosphere, which is not very different from what Regnault obtained at very low pressures, and Aimé experimenting in the Mediterranean got 0·0000486 per atmosphere. I have used, however, as the unit always 100 fathoms, because for our purposes it is more convenient. From experiments made with another instrument at an earlier date, the mean apparent compressibility of water at pressures between 0 and 3,000 fathoms was found to be 0·000878 per 100 fathoms.

In an instrument precisely similar the compressibility of sea-water was determined. The results obtained were very concordant. Up to 800 fathoms the apparent compressibility was 0·000770, up to 1,000, 0·000787, and 1,500, 0·000793, the temperature being between 2° and 3° C. The mean value is 0·000783, and the ratio to the apparent compressibility of distilled water is 0·87. The dimensions of the instrument did not allow of its being used at greater depths.

These experiments were all made on the sounding line, and at temperatures which were comparatively low. At all depths above 800 or 1,000 fathoms the temperature of the ocean varies but little. It is always somewhat under 4°, and generally under 3° C., and usually above 1° C., so that it may be said that these compressibilities are good for an interval of temperature between 1° and 3° C. In order to find out the compressibility at higher temperatures, there was on board a hydraulic instrument, similar to what Mr. Casella used for testing his thermometers. It was somewhat larger than Mr. Casella's, made for the purpose of being taken away. In it a number of experiments were made at different temperatures. But here we had no gauge, except an artificial one, which we found already was not to be relied on. Consequently the only determination that could be made was the relative apparent compressibility of various liquids, referred to that of distilled water as unity. Adopting one of the instruments filled with distilled water which had been used frequently on the sounding line as a gauge, we knew approximately what the pressure was, but we could not of course say that a compression of a certain amount at 20° C. represented the same pressure as the same compression at 2° C., and consequently in the result of these experiments I have only put down the *relative apparent compressibility* of these substances. The apparent compressibility of distilled water at the temperature of the water of the pump being unity, the relative apparent compressibility of sea-water and of a 4 per cent. chloride of sodium solution at different temperatures are given in the following table:—

Temperature C.	Relative apparent compressibility of			
	Sea-water.	4 p.c. NaCl solution.	Mercury.	Distilled water.
26·5°	0·9360	0·9051	—	1
25·8	0·9191	0·8813	—	1
22·5	0·9214	—	0·0301	1
22·1	0·9283	0·8661	—	1
13·7	0·9264	0·8684	0·0260	1
13·3	0·9203	—	0·0247	1
2·5	0·8950	—	0·0325	—

The relative apparent compressibilities appear to diminish with the temperature. This probably is owing to the fact that the compressibility of distilled water was found by Regnault, at low pressures at least, to diminish with the temperature. Now the compressibility of sea-water probably does not diminish so much, and that of chloride of sodium probably still less—possibly it even increases with the temperature. These results at any rate would tend to show that the compressibility differs in that direction. In the fourth column of the table the values of the relative apparent compressibility of mercury are given. To this I shall now refer. The compressibility of mercury was determined in an instrument (Fig. 2) the same as the water-piezometer; the stem,

FIG. 3.

FIG. 2.

however, was bent round, so that the mercury should not have a tendency to flow down the stem. The bulb, A, contained about 250 grams of mercury, about 19 c.c. The stem was filled with water through a

considerable portion of its length, BC, in which a magnetic index worked. This instrument was finished off in exactly the same way as the water-piezometer, that is, by dipping the open end into a cup of mercury (see Fig. 2). Here again the instrument registered the sum of the effects of temperature and pressure. On the sounding line experiments were made with this instrument. A number of observations were obtained at 800 fathoms and at 1,000 fathoms, with very concordant results. There were a number at other and greater depths, and those were plotted in the ordinary way on paper, and through them an average line was drawn, so as to give the compressibility of the mercury. The result was, that the apparent compressibility of mercury for 100 fathoms was 0.0000271, being per atmosphere 0.0000015.

Similar observations were then made in the pump. The results are given in the annexed table.

Temp.	Comp.
22.3	0.0301
13.3	0.0261
11.5	0.0247

There is a remarkable difference in the result at 11.5° and 13.3° from those obtained at lower temperatures than at 22.3° in the pump. The results at 11.5° and 13.3° C. were obtained by two different instruments, and are each the mean of about 15 different observations, so that they are probably correct. On the sounding line, at temperatures between 1° and 3° C., the relative apparent compressibility of mercury was found to be 0.0322, 0.0327, and 0.0301 in three different series of observations. It is impossible to say to what the low relative apparent compressibility at these temperatures is due, and the subject is worthy of further investigation.

Having then determined in this way the apparent compressibilities of these liquids, it was easy to apply it to the practical purposes of sounding. One of these instruments was attached to the line, and the apparent contraction cleared for temperature gave the depth.

It was, however, found that in many places the temperature remained apparently constant for a considerable depth; that is to say, after a certain depth, we will say, for instance, 1,000 fathoms, was reached, then if the depth of the sea was perhaps 2,500, the readings of the Casella thermometer showed constantly the same temperature all the way from 1,000 fathoms down to the bottom. This afforded evidence that either the temperature was the same all the way from 1,000 fathoms down to the bottom, or it was higher than at 1,000 fathoms; but there were no means of ascertaining this unless the temperature really rose to be higher than that at the surface, where of course the

maximum index of the Millar-Casella thermometer showed it. For this purpose the mercury-piezometer came into use, and it was for that purpose principally it was made. As in the case of the water-piezometer, its readings show the combined effect of change of temperature and of pressure, either of which being given the other can be deduced.

But it will be evident that if, after reaching a certain depth, the temperature remains constant, the apparent volume of the liquid will diminish in simple proportion to the increase of pressure, and if the temperature increases from a certain depth downwards, the apparent volume of the liquid will diminish in a less proportion than the increase of pressure. If this increase of temperature is such as to expand the liquid so as just to compensate the compressing influence of increase of pressure, the index will remain stationary. The method of using the two instruments combined will be evident from a reference to the figure.

The change of volume of water with change of temperature at the low temperatures found in the deep seas is very slight. The change of volume of mercury, however, for all ordinary temperatures is very considerable. On the other hand, the compressibility of water, or its sensibility to change of volume with change of pressure is very great, whereas that of mercury is very small. Consequently, by sending a pair of these instruments down upon the sounding-line, and reading them when they come up, we have two independent values of the sum of the effects of change of temperature and of pressure. Taking as the first approximation to the depth the length of the sounding-line, applying it to the reading of the mercury instrument, and so correcting it for pressure, we have a first approximation to the temperature; applying this temperature to the reading of the water-piezometer, we obtain a second approximation to the depth, indeed the true depth. The reading of the mercury-piezometer now being corrected for pressure by this value of the true depth, we have a second approximation to the temperature. In fact we have now practically the true depth and the true temperature. In the figures the thick lines represent the apparent changes of volume for change of pressure, and the dotted lines the apparent changes of volume for changes of temperature.

Fig. 4, *a* refers to the water-piezometer, and Fig. 4, *b* to the mercury-piezometer. Distances measured along the horizontal line of abscissæ represent depths in the scale of 0·01 inch to a fathom, and temperatures on the scale of 0·1 inch to a degree Centigrade. Distances measured along the line of ordinates represent scale divisions (millimeters) on the scale of 0·1 inch to a division. For 100 fathoms of depth the apparent contraction of the mercury instrument was 0·7 millimeter on the stem; in the water instrument the apparent contraction for 100 fathoms was somewhat over 7·8 millimeters. Considering that the effect of a change of temperature of 1° C. causes an

apparent change of volume represented by about 2.5 millimeters, while in the water-piezometer at the low temperature always found in deep

FIG. 4, a.

FIG. 4, b.

seas the temperature may be anything between 0° and 10° C. without altering the apparent volume of the water by more than 2 millimeters on the stem, we see that an error in determination of the depth of 100 fathoms would only make a difference in the reading of the mercury instrument of about 0.6 millimeter, equivalent to a difference of temperature of about 0.25° C. Therefore, applying the possibly erroneous depth given by the sounding-line to "clear" the reading of the mercury-piezometer for effect of pressure, we obtain a first approximation to the temperature which would almost always be within half a degree of the truth, but which might occasionally differ more than a degree from it. Using the temperature thus found to clear the reading of the water-piezometer for the effect of temperature, we obtain a second approximation to the depth which cannot differ appreciably from the true depth. Applying the depth so found to clear the reading of the mercury instrument for effect of pressure, we obtain a second approximation to the temperature which cannot differ appreciably from the truth. This process of gradual approximation may of course be carried as much further as we please, but the results obtained in the second approximation may under all circumstances be taken as representing the truth.

In Fig. 1 (page 448) the stem of the water-piezometer is represented as being swelled into a small bulb at F. The purpose of this bulb is to enable the instrument to be used at depths so great that with a uniform stem the contraction produced would be equal to the whole volume of the stem. The capacity of F is equal to the contractions

produced by the fall of temperature and the first 1,000 or 1,500 fathoms of depth, so that the instrument would only register depths greater than 1,000 or 1,500 fathoms, but it would do so with as much precision as can be obtained for less depths.

I also made an attempt to determine the compressibility of glass, in order to get the true compressibility of these liquids. I shall only allude to it in order to indicate the method—the result not being sufficiently accurate to be applied yet.

A tube such as that used for the stems of the piezometers, of about 1 millimeter bore, and graduated into millimeters, was sealed at one end. A glass rod was then drawn out to such a size as to pass easily into the tube, and about 2 inches shorter than the tube, which was made of the extreme length of the receiver of the pump. The rod having been introduced into the tube, and a magnetic index on the top of it, the end of the tube was sealed up. When now the tube was subjected to high pressure, the material of the tube was compressed and diminished in volume in every direction, and of course therefore in its length. The rod, however, in the inside not being exposed to extra pressure, maintained its original length. The index having been brought down home on the top of the rod before exposing the system to pressure, was kept in its place by the rod, and the tube on the outside was therefore pressed down past it to a certain extent. When the pressure was relieved, the tube recovered its length, and slightly lifted the index. The extent to which the index was free above the rod when that pressure was relieved measured the linear compression of the glass; and multiplying that by three, we had the cubical compression. The results obtained by these means were too rough to be quoted as of any particular value, because measurement of this distance was effected simply by the eye on the scale of millimeters, which was etched on the tube, but with an apparatus I have since had constructed I expect to obtain results of great accuracy, not only for glass, but for other solids.

I shall now direct your attention to a subject of a somewhat more chemical nature, namely, the atmospheric contents of the sea-water. The carrying out of these investigations was one of the principal objects of having a chemical laboratory on board the ship, and that we were able to do so successfully I owe in great measure to the fact of having met Professor Jacobsen, the chemist who was in the Prussian expedition to the North Sea in the year 1872, who very kindly showed me all the apparatus which he was using, and especially his adaptation of Bunsen's method of boiling nitrogen and oxygen out of the water, which was of the greatest value; in fact it made the operation possible on board ship, which otherwise it would not have been.

In order to boil atmospheric gases, that is to say oxygen and nitrogen

(because in sea-water carbonic acid may almost be said to belong to the fixed constituents, as it is quite impossible to eliminate it in the ordinary way *in vacuo*), the general arrangements of Bunsen's apparatus was used. The apparatus has been fully described by Professor Jacobsen, in the *Annalen der Chemie und Pharmacie* for May, 1873. Its speciality consists in the means of making and breaking connection between the flask and the glass tube destined to receive the air eliminated from the water. The water from the bottom was usually collected in the so-called "slip" water-bottle, which has been described by Professor Jacobsen. Water from intermediate depths is obtained in an instrument represented in section in Fig. 5. It is made entirely of brass, which, however, might advantageously be nickel-plated. It consists of a cylinder A, terminated at both ends by similar stopcocks, B, B, which are connected by the rod C. This rod carries, near its upper extremity, a piece of stout sheet brass (D), 10 centimeters long by 15 broad, soldered to the casting, E, which is movable about the axis *e*. The function of this part of the apparatus will be more easily explained by describing the manipulations necessary when collecting water.

FIG. 5.

When intermediate water is to be obtained, the water-bottle is firmly attached to the sounding line, which carries at its end usually a 56lb. or 1 cwt. lead; the stop-cocks are then opened, giving them, with the rod C, the position represented in the figure. The line is then lowered carefully by hand, until the water-bottle is close to the surface, when it is let go, and the line allowed to run out without a check. During its passage downwards, the water courses freely through it, being considerably assisted by the conical end-pieces, K, K. When the requisite depth has been reached, the line is checked, hauled in a few fathoms, then let go, checked again at the same mark, and finally hauled in altogether by the donkey-engine. When the line is hauled in at first, the flap D falls down into a horizontal position, when it is caught by the movable piece of brass F, which moves round an axis *f*, and is supported on the side opposite to E, by the rod G, which rests on the spiral spring H. The water rushing past D when thus in a horizontal position, exercises a sufficient pressure upon the rod to close the stopcocks B, B. When the speed with which the bottle is hauled through the water is increased, the pressure on D becomes so great, that it overcomes the tension of the spring H, and E passes the catch

F, when the rest of the journey upwards is performed with the flap D hanging down, and therefore offering the least possible resistance to the water. The object of at first hauling in only a couple of fathoms or so, and letting the line go again, is to ensure the cocks being closed. For, supposing after the first hauling in they were not quite closed, by letting the instrument descend through the water, the flap D sets itself again, and, on heaving in, it shuts down the stopcocks, which were before but partially closed; or, if they were closed before, it only shuts them the tighter. When the water-bottle has been brought up, it is only necessary to substitute for the lowermost brass funnel, a small nozzle, when the water may be tapped into any vessel destined to receive it. This done, the bottle may be at once lowered to any other required depth, much time being spared by not having to detach it each time. At the upper end of the bottle a small spring safety valve L is introduced, in order that the considerably denser water from below may be able to make room for itself as the surface is approached. In order that the instrument may properly do its work, it is evident that first the stopcocks should be so stiff, that the weight attached to their levers be not sufficient to close them, and secondly, the spring H should be so strong as to ensure the shutting of the cocks before it itself gives way. These conditions are secured by the following means of adjustment. The stopcocks can be made stiffer in the usual way, by tightening the screws which secure the "keys" in the barrels; the tension of the spring H can be increased or diminished by means of a screw at the lower end of the tube containing it; and the mobility of the stopcocks can be further regulated by means of the screws M, M. Although from this description, the operation of adjustment may appear complicated, it is, in fact, practically, very simple. After the first time of use, it is rare that any further adjustment is required than a turn of the screws M, M.

The diameter of the apertures at either end is necessarily smaller than that of the cylinder, it is therefore impossible for the water in it to be entirely changed while descending through a distance equal to its own length. It became a question, therefore, for experiment to decide what actually was the rate of change of water. To this end, a few experiments were made in a fresh-water lake. The bottle being filled with water containing some yellow prussiate of potash, was sunk in the lake, until the surface of the water was on a level with the upper stopcock, when the stopcocks were opened, and the line let go. On being brought up again, the contents were tested with solution of perchloride of iron. It was found that when the bottle had been sunk to a depth of a fathom and a half, the water had been entirely changed, the iron solution being wholly without action on it. We may be certain, then, that the water which we obtain by this means is an

average of the last two fathoms through which the bottle has passed.

The weight used as a sinker should be chosen so as to impart sufficient velocity not to lose time unnecessarily over the operation, and at the same time not to give an excessive velocity at the depth where the water is to be collected, because the rate of change of water depends on the friction of the water inside the bottle, and so on the velocity of descent. In practice, for depths over 100 fathoms, a weight of 112 lbs. was used, and for depths from 25 up to 100 fathoms, a weight of 56 lbs. was used. For less depths, the weight of the bottle itself was sufficient. The velocity of descent at the depth where the water is to be collected, should not exceed twelve feet per second. The mean velocity of descent for the interval between 75 and 100 fathoms from the surface was, with 56 lbs. nine feet, and with 112 lbs. eleven and a half feet per second.

When once let go, it is essential that the line should run out to the required depth without a check; it is then, however, immaterial, as far as the water-bottle is concerned, what interruptions occur in heaving in. The fulfilment of the condition of running out without a check never presented any difficulty on board the "Challenger," depending as it does on the care of those who tend the line. When, however, by accident a check does occur, the line is stopped, and the water-bottle brought up again, reset and sent down again. In order to utilise any such accidents, it is usual to take the water from the greatest depth first; then if a check does occur, it may occur at one of the desired intermediate depths, and so no time would be lost. In designing the water-bottle, it had been my intention to use it not only for collecting water, but also as a flask, so that the atmospheric gases could be boiled out of it without transvasing the water. In practice, however, I have not been able to get air-tight stopcocks, besides which, it would make an inconveniently large apparatus in a very small laboratory. I have spoken of this water-bottle as being only used for intermediate waters, but there is no reason why it should not be used for bottom water; indeed, where the sounding lead does not weigh over 1 cwt., it is frequently used for this purpose. In the case of deep soundings, however, where a weight of three, and sometimes four hundred-weight is used, the "slip" water-bottle is always preferred.

When the water-bottle has been brought up full of water, the lower funnel is unscrewed and a nozzle put in its place, with a long glass tube attached to it. This tube reached to the bottom of the flask, and by it the water was run into the flask as carefully as possible without allowing any bubbling or unnecessary commotion in the water. The necessity for transvasing the water before boiling, is a defect in the method. When, however, it is carried out expeditiously and carefully,

it does not appear to introduce any serious error into the results. As a proof that this is so, I may mention that one sample of water brought up by this instrument from a depth of 2,800 fathoms, contained only 3 per cent. of oxygen; and a sample of rain-water which I collected on one occasion had less than 1 per cent. of oxygen. In transvasing, the flask was filled up to the lip and usually allowed to run over for a little, so that the upper layer of water which had been most in contact with the air might be eliminated. The connections with the receiving tubes were then made. The water in the small bulb was kept boiling vigorously for from 10 to 12 minutes before the upper end of the gas tube was closed with a glass rod and sealed up. When this was done, connection was established between the flask and collecting apparatus, the gas in the water immediately beginning to be evolved. The lamp was now lighted below the water-bath which contained the flask, and the temperature of the water thus raised until the water in the bath boiled; this was continued always for an hour, usually for an hour and a-half, and often longer, the exact duration of the boiling depending on the nature of the other work going on.

Although disadvantageous as regarded comfort and ventilation, the lowness of the roof in the laboratory presented some advantages especially for fixing apparatus. It will be evident that the apparatus with which we are at present dealing possesses a good deal of spring. In order to give it more rigidity, the upper extremity of the brass rod supporting the gas tube was supported by strings from the roof.

As the amount of oxygen and nitrogen contained in a litre of sea-water may amount to 22 or 23 c.c., it is advisable that the gas-tubes used for collecting it should not have less than 50 c.c. capacity, in order that the water may continue to boil vigorously when the flask is immersed in the water-bath. As long as this is the case, the water in the small bulb has no opportunity of cooling, and so choking the tube. The gas-tube itself, however, should not be unnecessarily heated.

The rolling of the ship (otherwise an annoyance) was here not altogether disadvantageous. It produced a species of pumping in the apparatus, so that when the boiling was getting sluggish, a heavy roll was enough to start it again.

The apparatus for the determination of the carbonic acid is described by Professor Jacobsen in the memoir above cited. The alterations which I introduced into it were not of an essential character, and were suggested by exigencies of locality. In principle, the method consisted in distilling to dryness a measured volume of the water in a current of air free from carbonic acid. In the receiver and in the tubes interposed between it and the aspirator, a measured quantity of baryta-water of known strength was placed. The baryta removed

from this solution in the course of the experiment was a measure of the carbonic acid eliminated from the water. It is well known that sea-water retains its carbonic acid with great tenacity, mere boiling having but little effect on it. Amongst the experiments which occupied me during nine months previous to my receiving the appointment of chemist in the expedition, with a view to settle on a trustworthy basis the programme for the chemical work during the expedition, was a thorough investigation of the behaviour of sea-water to carbonic acid. These investigations showed that the dissolved sulphates were amongst the most energetic agents in the retention of the carbonic acid. From this I concluded that if the sulphates were removed, the carbonic acid would come away more easily. Consequently, before beginning to distil the water for determination of the carbonic acid, about 15 c.c. of a saturated solution of chloride of barium were added to it. The flask having been closed was shaken so as to mix the solution.

This no doubt had to a certain extent the desired effect of assisting the disengagement of the carbonic acid from the water boiling at ordinary pressure, although when half of the water was distilled there was still a notable amount (about one-tenth of the whole) in the residue; but it had no effect in rendering it more amenable to the action of reduced pressure. It had a very useful effect in eliminating "bumping" altogether, even when the distillation was carried to complete dryness. A liquid residue of about 10 c.c. was usually left in the flask. The baryta remaining in the solution was determined by means of hydrochloric acid, rosolic acid being used to determine the point of neutralisation.

The results obtained are put shortly in the table (p. 462). Of the gas samples which were boiled out in the Bunsen apparatus, I was enabled to analyse about a third before my connection with the expedition ceased; the remainder, as far as I know, have not yet been analysed. The results are therefore subject to correction from future experiments. The mean value of the oxygen per cent., that is oxygen and nitrogen being put as 100, is 33·67. Jacobsen experimenting in the North Sea, found a mean of 33·9, and observed a variation between the limits of about half per cent.; that is to say, he observed from 33·7 to 34·2 per cent. of oxygen in the mixture of oxygen and nitrogen, and he came to the conclusion that this was a real difference, and not due to any errors of observation. My observations, which extend over a much greater area, give a lower average, 33·67, and a very much greater variation in the individual results, in round numbers from 33 to 35 per cent.

The amount of oxygen obtained in samples in the Antarctic regions is usually very much higher than in the moderately warm regions

Depth. Fathoms.	Carbonic acid. Grams in 1 litre.	No. of determinations of CO ₂	Oxygen per cent. (N + O = 100).	Nitrogen. c.c. in 1 litre.	Average temperature of water.	Nitrogen according to Bunsen.	Deficiency of nitrogen.	Oxygen. c.c. in 1 litre.
D.	C.		$100 \cdot \frac{O}{N + O}$	N.	T.	N'.	N' - N.	O.
					°C.			
Surface.	0·0426	73	33·67	—	—	—	—	—
25	0·0337	2	33·36	—	—	—	—	—
50	0·0488	10	32·33	—	—	—	—	—
100	0·0436	5	30·21	11·26	14·6	11·75	0·49	4·24
200	0·0446	8	23·40	11·71	13·0	11·92	0·21	3·59
300	0·0440	4	11·37	13·00	6·9	13·45	0·45	1·67
400	0·0411	10	15·46	13·10	5·1	14·00	0·90	2·41
800	0·0422	7	22·62	13·86	2·5	15·00	1·14	4·06
over 800	0·0446	7	23·45	14·37	1·5	15·40	1·03	4·40
Bottom	0·0474	54	—	—	—	—	—	—

about the trade winds; and also in all the warm regions. In the Antarctic regions I found as much as 35 per cent., and as low as 32·9 per cent. in the trade wind regions. The number of observations is, however, as yet too small to admit of general conclusions being drawn from them.

The results obtained at other depths are given in the table. I have not taken a mean of the bottom ones, because the depth and other conditions were too various to make it of value.

Looking at the oxygen percentages dissolved in water at different depths, we see that they diminish with considerable rapidity as the depth increases, until a minimum is reached at about 300 fathoms, after which it slowly increases.

The percentage in very deep water appears to depend a great deal on the geographical position, being the greater the nearer it is to a position where the surface-water at some period of the year has the same temperature. This appears certainly to hold in the southern hemisphere. In the northern my observations on bottom water are from the Pacific, where there is without doubt a considerable admixture of southern cold water, which is therefore very remote from its source, and which therefore, mixed with the cold northern water, gives a result below what we find in similar southern latitudes. I believe the prevalent opinion attributing the low temperature in the deep waters of the North Pacific *entirely* to a southern source to be quite erroneous. In the northern parts of the Pacific the winter tempera-

ture is quite low enough, and the differences of temperature at the same place at different seasons, and in different places at the same season, are quite sufficient to cool down a large area of the northern Pacific, and when the remainder (the larger portion) of the air samples which I boiled out have been analysed, much light will no doubt be thrown on the question.

Whatever view we may adopt as to the cause of ocean circulation, there can be no doubt that the actual effect is to cause on the whole a rise of cold water towards the surface near the equator. This water has been impoverished since it was last exposed to the atmosphere, and would probably not contain more than 15 or 20 per cent. of oxygen in its absorbed atmospheric air. This percentage being so remarkably reduced at 300 fathoms shows the existence of a powerful reducing agent at that depth, and as we know from Mr. Murray's observations that life generally is remarkably abundant at these depths, it is a fair inference at least provisionally to assume that the disappearance of oxygen is due to the abundance of life. It must be remembered, however, that our observations were made in tropical equatorial waters, where surface aëration is hindered by the constant high temperature of the atmosphere. In colder regions we have every reason to believe that the aëration is much more energetic. Unfortunately in dealing with sea-water the amount of carbonic acid present affords no means of controlling the amount of oxygen which is deficient.

In distilled water carbonic acid is absorbed according to well-known laws, but this is not the case with sea-water, for which the laws have yet to be investigated.

The amount of carbonic acid per litre in water at the surface, according to an average of a great number of observations, was 42·6 milligrams. The highest average amount, 48·8, was at 50 fathoms, falling to 41·1 at 400 fathoms, and rising to 47·4 at the bottom.

It will be seen from the table that the absolute amount of carbonic acid dissolved is very great, and that the variations with depth are insignificant. Observations at the surface showed that, on the whole, less carbonic acid was present in warm than in cold water, but no law was apparent. We know from other investigations that the saline contents exercise a very strong influence on the amount absorbed, and there are other causes which introduce seeming irregularity into observations of this kind.

There is a remarkable difference in the results which I have obtained in the open ocean and those obtained by Dr. Jacobsen in the North Sea. I have found approximately 45 milligrams per litre as a mean amount. Jacobsen found 88·6 milligrams, or about exactly double the quantity. It is difficult to imagine to what cause so great a discrepancy is to be attributed if it does not depend on the nature of the

water. The water in which nearly all my observations were made was the deep clear ultramarine-blue water of the ocean. The North Sea water in which Jacobsen experimented is comparatively opaque and green. In the Antarctic Ocean, where such water was met with occasionally, though very sparingly, the carbonic acid was always present in marked excess. The green colour of shoal water is generally attributed to the influence of solid matter, which may also tend to retain carbonic acid, as we know is the case with dissolved saline matter. The subject is worthy of investigation; and as sea-water collected well out at sea contains, as far as I have been able to observe, no appreciable amount of putrescible matter, and as the carbonic acid dissolved has no tendency to escape at ordinary temperatures, samples, if carefully collected and preserved in clean stoppered bottles, would be in a perfectly fit state for examination after a long voyage.

In the Table at page 462, we have seen the distribution of gaseous contents at different depths. In the Table below we have the mean results from different zones of bottom water. It will be seen that the highest percentage of oxygen, as well as of nitrogen and carbonic acid, is met with in Antarctic regions, and that the amount of all these bodies present in the water diminish as we approach equatorial regions.

Bottom Water.

Latitude.	Oxygen per cent. O + N = 100.	Carbonic acid per litre. Grams.	Nitrogen. c.c. in 1 litre.
66° S. to 35° S.	27·63	0·0518	13·73
35° S. to 10° S.		0·0466	
10° S. to 10° N.	21·85	0·0409	11·71
10° N. to 35° N.		0·0459	
35° N. to 38° N.	20·76	0·0426	12·91

The absolute maximum of carbonic acid in bottom water was observed at the highest southern latitude where observations were made, namely, 0·0829 gram per litre in lat. 65° 42' S., long. 79° 49' E., and 1,675 fathoms. The minimum amount in this zone was 0·0385 gram per litre in lat. 48° 37' S., long. 55° 17' W., and 1,035 fathoms. Between 35° and 10° S. latitude I have analysed only one air-sample from bottom water. It was collected lat. 12° 45' S., and long. 147° 1' E., in a depth of 1,700 fathoms, and contained 17·4 c.c. of oxygen and nitrogen in one litre, 20·97 per cent. of which was oxygen, and 0·0609 gram CO₂ in a litre.

In this part of the ocean, which has been called the Melanesian Sea, I observed some anomalous results. Amongst them may be reckoned the above very high percentage of carbonic acid. The amount varied

greatly at different stations. In lat. $14^{\circ} 7' S.$, long. $153^{\circ} 43' E.$, and 2,450 fathoms, it was 0.0434 gram, and in 1,400 fathoms, close to Raine Island, it was 0.0361. Both the maximum and the minimum amounts were found over "globigerina ooze," while the medium (0.0434) was found over "red clay." At the surface, too, in lat. $17^{\circ} 25' S.$, long. $169^{\circ} 5' E.$, there was found in one litre 0.096 gram carbonic acid, 14 c.c. of oxygen and nitrogen, of which 32.35 per cent. was oxygen. The temperature of the water was $25.8^{\circ} C.$, and, notwithstanding, it contained the greatest amount of carbonic acid observed in any water during the cruise, and also the least oxygen percentage of surface water.

At other stations in the same sea 0.0317 and 0.0357 gram carbonic acid were observed. Only one air-sample, however, has as yet been analysed. Again, in the same zone, but to the north of New Guinea, a very large amount of carbonic acid was met with in the bottom water, though the surface water was not abnormal in this respect. In lat. $4^{\circ} 19' N.$, long. $130^{\circ} 15' E.$, and 2,500 fathoms, 0.0524 gram carbonic acid was found, and in lat. $5^{\circ} 31' N.$, long. $145^{\circ} 13' E.$, and 2,325 fathoms, 0.0619 gram carbonic acid was found in one litre. In the Celebes Sea 0.0524 gram carbonic acid and 21.78 per cent. oxygen were found. In the Sulu Sea the carbonic acid is 0.0243 gram, and the oxygen percentage 29.63. The deep water in this sea has a temperature not below $10.2^{\circ} C.$, so that it is in many respects peculiar; under these circumstances, however, it is remarkable that there should be so little carbonic acid and so much oxygen.

We see, then, that in this zone there is very considerable fluctuation in the gaseous contents of the sea-water, especially at the bottom. In the zone from the parallel of 10° to that of $35^{\circ} N.$ latitude, there is an analysis of only one air-sample from the bottom. It is from the deepest sounding from which bottom has been obtained, namely, 4,475 fathoms in lat. $11^{\circ} 24' N.$, and long. $143^{\circ} 16' E.$ Here again we have a very small quantity of carbonic acid (0.0312 gram), and of nitrogen and oxygen (13.9 c.c.), with a high oxygen percentage (29.11). As the depth reached on this occasion was quite unexpected, the sounding was repeated with substantially the same result as regards depth, but the water-bottle came up the second time unclosed, so that the attempt to verify or control the somewhat abnormal results obtained from water from such exceptional depth was frustrated in the most annoying way possible. The water-bottle was that commonly used for bottom water, which is constructed to close on touching bottom. The "tumbler," which by falling forward disengages the cylinder which encloses the water between suitable valves, had in this case fallen backwards, thus, as it were, choking itself. In this zone the maximum carbonic acid was 0.057 grams in lat. $19^{\circ} 4' N.$, long. $65^{\circ} 7' W.$, and

3,875 fathoms, being the deepest water found in the Atlantic. The minimum carbonic acid was that above referred to as having been found in the Pacific in the deepest water attained during the cruise, with the exception of this last. All the observations in this zone are from the North Atlantic, where the conditions are in many respects exceptional. The saltness is much above the average, and it is therefore not astonishing that the carbonic acid should also be comparatively high.

To the north of 35° N. latitude the observations are principally in the Pacific, the mean amount of carbonic acid observed being 0·0426 gram per litre. The two highest results (0·064 and 0·0536 gram) were observed in the North Atlantic. If these are eliminated, the mean amount in this zone of the Pacific is 0·0361 gram. The absolute amount of oxygen and nitrogen is higher in this zone than in the equatorial one. The amount of nitrogen is 12·91 c.c. per litre. In the same latitude in the South Atlantic two observations give 14·01 and 14·76 c.c. per litre. If the bottom water in this zone of north latitude were made up of equal parts of water prepared in the southern and in the northern hemispheres, the mixture would probably contain nitrogen in the quantity above quoted.

Amongst the air samples collected from intermediate depths, those from 800 fathoms present a remarkable uniformity in the oxygen percentage. The results are given in the following table:—

Latitude.....	32° 36' S.	37° 47' S.	35° 45' S.	3° 10' N.
Longitude.....	137° 43' W.	30° 20' W.	18° 31' W.	14° 51' W.
Oxygen percentage	22·05	22·95	23·25	22·22
Nitrogen c.c. per litre ...	13·72	14·10	15·81	11·82
Oxygen c.c. per litre	3·88	4·20	4·79	3·38

The first three samples are from southern latitudes, between the parallels of 32° and 38°, and the fourth is from a position close to the equator in the Atlantic. It will be seen that the oxygen percentages are much more uniform than the absolute amounts of that gas per litre of water. Indeed, if we compare the first and the last column, we see that the equatorial Atlantic water possesses absolutely much less oxygen than the North Pacific water, though its percentage is higher. The amount of nitrogen present is also very variable. In the South Atlantic we have as much as 15·81 c.c. per litre, while at the equator in the same ocean there is only 11·82 c.c. The temperature of the water in all the cases was practically the same, being from 2·5° to 3° C., and the specific gravity was similar, in the two extreme cases identical. This disappearance of nitrogen is very remarkable. Humboldt and Provençal (*Mém. d'Arc.*, ii, 377), in their investigations on the respi-

ration of fishes, noticed a similar phenomenon, nitrogen always disappearing side by side with the oxygen, though in no constant ratio to it. They experimented on tenches, letting them remain in the water until they were uneasy, then boiling out the water. They found that a tench will exhaust the oxygen in a cubic meter of river-water in 21 months. They observed the oxygen percentage ($N + O = 100$) reduced as low as 1.3, and observed that want of oxygen affected the fish more than the presence of excess of carbonic acid.

The lowest oxygen percentage observed was 3.84, in water fetched 75 fathoms above the bottom, in lat. $30^{\circ} 22' N.$, long. $154^{\circ} 56' W.$, the depth of the sea being 2,950 fathoms. The absolute amount of oxygen was 0.6 c.c., and of nitrogen 15 c.c., so that there was no deficiency of nitrogen.

While becalmed off the north coast of New Guinea, I boiled out the air from rain-water collected as it ran off the awning. The temperature of the rain so collected was usually from 23° to $24^{\circ} C.$, and in order not to interfere with the work of the day, it was kept in perfectly full bottles over night and boiled the next morning. Three rain-waters were so treated, and the oxygen percentages found were 18.56, 25.42, and 0.52, and in the last two were found 11.27 and 11.66 c.c. nitrogen per litre. There was therefore rather more nitrogen than would have been expected from the temperature. The amount of oxygen present, however, varied in the most extraordinary manner, the last sample being almost completely deprived of oxygen. If the carbonic acid be reckoned as oxygen, we obtain the following percentages in the same order, 28.45, 30.83, and 25.68. The rain-water when freshly collected remained perfectly clear on addition of baryta-water; the carbonic acid had therefore been entirely formed during the night at the expense of the oxygen dissolved in the water. There must therefore have been also a considerable amount of easily oxidisable carbon in the water, and in fact I collected a considerable quantity to be used for the preparation of standard solutions, but could make no use of it, as it speedily produced a fungus-like deposit. Some also which I tested for ammonia continued to give it off on distillation almost to dryness; and the water remaining in the retort reduced permanganate energetically. At the time I did not know of the deficiency of oxygen, and I attributed the organic matter to contamination from the awning, although the explanation never fully satisfied me, because the amount of rain which fell was sufficient to thoroughly wash the awning, and as it did not answer the purpose for which I had particularly collected it, I reverted to the ship's distilled water, which was always of excellent quality. It will be seen that the carbonic acid does not completely cover the loss of oxygen; no doubt, however, some water was formed, and it is not

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impossible that sulphurous acid from the furnace gases escaping by the funnel may have had some reducing effect.

The determinations which were made of the absolute amount of these permanent gases, as we are accustomed to call them, in the litre, have been plotted in Fig. 6. The curve represents Bunsen's curve of the absorption of nitrogen in distilled water. The ordinates represent cubic centimeters of nitrogen absorbed by distilled water of different temperatures when exposed to the atmospheric mixture of nitrogen and oxygen. The results obtained by boiling the gases out of the surface water have been plotted on the same figure, and also those obtained by Jacobsen in his experiments in the North Sea, in order to see how they would fall in with the curve already found for distilled water. I have found it was impossible either to get my own results or Jacobsen's to agree with this curve. With hardly an exception all the observations fall below the curve. About 10° to 12° C. they nearly coincide with it, but both at lower and higher temperatures, and especially at higher temperatures—and my own experiments were made to a great extent in water of higher temperatures—fall very considerably below it. The shading covers the whole of the experiments, so that it shows at a glance how the experiments agree with the curve. We see at once that above 20° C. they depart very markedly from the law indicated by the line A B. The strong convexity of Bunsen's curve does not appear to be probable, knowing, as we do, how rapidly all but the very last traces of air are eliminated from water by heat. The curve as indicated by our experiments on sea-water would very much more nearly approach a straight line; it would, however, have a slight convexity in the same sense as A B. On this point the analysis of the remaining air-samples which were collected will throw much light, especially as a large number of them are from surface water in the warmest regions of the earth.

FIG. 6.

Cub. cents. nitrogen dissolved in 1 litre sea water at different temperatures. x "Challenger." o "Pomerania."
AB is the curve according to Bunsen for distilled water at a pressure of $\frac{4}{5}$ ths atmosphere.