

them were domesticated, though some human remains from neighbouring tumuli and interments are reported on by Dr. Pruner-Bey. The animals whose bones occurred are described as ox (possibly domesticated), pig, stag, sheep, goat, and horse, which is rare. The bones are not always broken, and the vertebræ occasionally occurred in juxtaposition, as if meat at times had been extremely abundant. There is no mention of any remains or traces of dogs, and this condition of the bones seems to afford an argument in favour of their absence, which, if established, would be a remarkable fact. Some teeth of reindeer are mentioned as having been found on the plateau, and it would be of great interest to ascertain their relation to the other remains. Let us trust that ere long there may again be a season in France when a though may fairly be bestowed on other camps and other earth-works than those on which attention is now so unfortunately concentrated.

J. E.

SPONTANEOUS GENERATION

I HAVE repeatedly subjected various solutions for Dr. Bastian to a temperature of 150° to 156° C. in sealed vacuous tubes, in order that he might afterwards submit them to a microscopical search for living organisms. The result of this search led him to conclude that living organisms had been generated from non-organised matter, whilst Professor Huxley, who examined the contents of one of the tubes, considered that no such conclusion could be drawn from his own observations. I therefore determined to repeat these experiments, operating in exactly the same manner as before in the preparation of the solutions, the sealing them up in vacuous tubes, and exposing them to a high temperature, but taking additional and much more stringent precautions against the subsequent admission of atmospheric germs into the tubes.

For this purpose four tubes of hard Bohemian glass were prepared, and about half-filled with a liquid consisting of

Carbonate of Ammonia . . .	15 grains.
Phosphate of Soda	5 grains.
Distilled Water	1 oz.

No care was taken to exclude living germs from these ingredients, reliance being placed, for the destruction of their vitality, upon the high temperature to which they were afterwards subjected.

These tubes were carefully exhausted by means of the Sprengel pump, and hermetically sealed; they were then, on July 18, 1870, exposed for four hours to a temperature varying from 155° to 160° C. in a Papin's digester. After being allowed to cool, the digester was opened, and the tubes immediately plunged, two of them into colourless concentrated oil of vitriol, and the remaining two into a nearly colourless saturated solution of carbolic acid in water. These precautions were taken in order to avoid the possible admission of atmospheric germs through invisible cracks in the glass; such cracks, entirely invisible to the eye, are known sometimes to exist, and to be in some cases so excessively minute as to require several days for the admission of enough air to perceptibly impair a torricellian vacuum within. By keeping the tubes entirely immersed in liquids which are immediately fatal to vitality, I hoped to meet any objections that might be raised, in the event of living organisms being subsequently found in the tubes, that the germs of such organisms had gained access to the enclosed liquids through invisible fissures in the glass. On examining them when they came out of the digester, it was evident that the interior walls of the glass tubes had been corroded by the enclosed liquid, and as the tubes had stood upright in the digester, it was easy to see, by the sharp limits of the erosion, the extent to which the

liquid had expanded under the influence of the high temperature to which it had been exposed.

The cylinders containing the immersed tubes were now maintained at a temperature from 60° to 75° F., and were exposed to bright diffused daylight, and sometimes to sunlight, for a period of more than five months.

The liquid in all the tubes became more or less turbid, and in some cases a small quantity of a light flocculent precipitate subsided to the bottom. On the 24th of December last two of the tubes, which exhibited the greatest turbidity, were selected for examination (one of them had been immersed in concentrated sulphuric acid, the other in the solution of carbolic acid). The vacuum was unimpaired, and the liquid in the interior formed a good water hammer. These tubes were opened in the presence of Prof. Huxley and Mr. Busk, and we submitted their contents to a searching microscopical examination with powers varying from $\frac{1}{3}$ th to $\frac{1}{7}$ th. Especially was the flocculent sediment in the tubes subjected to careful inspection. So far as the optical appearances presented by the sediment go, they may be appropriately described in the terms which Dr. Bastian applied to the matter found by him in a solution of like composition and similarly treated (see NATURE, July 7, 1870, p. 200). "A number of little figure-of-eight particles, each of which was $\frac{1}{2000}$ " in diameter, were seen in active movement, even in situations where they could not have been influenced by currents. The portions of the pellicle were made up of large, irregular, and highly refractive protein-looking particles imbedded in a transparent jelly-like material. The particles were most varied in size and shape, being often variously branched and knobbed. There were also seen several very delicate, perfectly hyaline vesicles, about $\frac{1}{2000}$ " in diameter, these being altogether free from solid contents." But the movement of the particles which we observed was obviously mere Brownian motion; and many of the particles were evidently minute splinters of glass. There was not the slightest evidence of life in any of the particles. The water on the slide containing these solid matters was evaporated off, and they were treated with hot concentrated sulphuric acid, the temperature of the slide being raised to about 100° C. There was no blackening, and the rounded and dendritic bodies remained as entirely unaltered as the glass splinters. Indeed, some of the larger spheroidal bodies were evidently rounded particles of glass which had become detached from the inner walls of the tube by the corrosive action of the enclosed liquid at the high temperature to which it had been exposed in the digester.

London, January 16

E. FRANKLAND

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

The Continuity of the Chalk

SIR CHARLES LYELL devotes a paragraph of his valuable "Students' Elements of Geology," just published, to the consideration of what he regards as a "popular error as to the geological continuity of the Cretaceous period." I feel the utmost diffidence in venturing to controvert any opinion of an authority so unrivalled in such questions, but as I believe the first definite suggestion of this view occurs in the report of the *Lightning Expedition* of 1868 by my friend and colleague, Dr. Carpenter, where it is specially associated with my name, I feel bound to defend so far as I can, or at all events to explain, an opinion which I then held on perhaps somewhat slender grounds, but which further investigation and reflection have since ripened into a firm belief. Sir Charles Lyell says (p. 263) that "certain points of resemblance which the deep-sea investigations have placed in a strong light, have been supposed by some naturalists to warrant a conclusion expressed in these words: 'We are still living in the Cretaceous epoch;'" a doctrine which has led to much

popular delusion as to the bearing of the new facts on geological reasoning and classification." I do not say that the phrase "we are still living in the Cretaceous epoch" is defensible in a strictly scientific sense, chiefly because the terms "geological epoch" and "geological period" are thoroughly indefinite. We speak indifferently of the "Silurian period" and of the "Glacial period," without consideration of their totally unequal value, and of the "Tertiary period," and of the "Miocene period," although the one includes the other. It is intended rather, I believe, in a popular sense, to meet what seems to be the general popular impression, that a geological period has, in the region where it has been studied and defined, something in the shape of a beginning and an end, that it is bounded by periods of change,—elevation, denudation, or some other evidences of the lapse of unrecorded time; and that it would be inadmissible to speak of two portions of the same continuous deposit, however distant the times of their deposition might be, and however distinct their imbedded fauna, as belonging to two geological periods.

It was certainly under this idea that in an address to a popular audience in April 1869, I stated my belief that it is not only chalk which is being formed at present in the bed of the Atlantic, "but the chalk, the chalk of the Cretaceous period." Sir Charles Lyell says, in summing up his objections at the end of the paragraph, "the reader will at once perceive that the present Atlantic, Pacific, and Indian Oceans are geographical terms, which must be wholly without meaning when applied to the Eocene, and still more to the Cretaceous period; so that to talk of the chalk having been uninterruptedly forming in the Atlantic from the Cretaceous period to our own, is as inadmissible in a geographical as in a geological sense." I confess I do not understand the geographical difficulty; the "Atlantic Ocean" is doubtless a geographical term, but the depression under discussion occupies the area at present expressed by that term, and to use it seems to be the simplest way of indicating its position. That it is highly probable that the chalk has been so uninterruptedly forming over some parts of that area, is, however, exactly what I wish to show. I will therefore set aside the question of expression, and address myself simply to the consideration of the fact. And first with reference to the physical aspects of the case.

All the principal axes of elevation in the north of Europe and in North America have a date long anterior to the deposition of the Tertiary and even of the newer Secondary beds; and these strata were, consequently, all deposited with a certain relation in position to certain main features of contour, which are maintained to the present day. Many oscillations have, undoubtedly, taken place since, and every spot on the European plateau has probably many times alternated between sea and land, but it is difficult to show that these oscillations have occurred in the latitude of Britain to a greater extent than 1,500 feet up and down; a subsidence to that extent would, however, be sufficient to produce over most of the northern land a sea 100 fathoms deep, the average depth of the German Ocean.

From a glance at a geological map of Europe and North America, it would seem that the sum of these elevations and subsidences has produced a gradual elevation of the edges, and a general contraction, of a basin the long axis of which coincides roughly with the axis of the Atlantic. The Jurassic beds crop out along the outer edge of this basin, the Cretaceous beds form a middle band, while the Tertiaries occupy the troughs and valleys. All of these, however, maintain a certain parallelism, determined by the contour of the older mountain ridges, to one another and to the shores of the present sea.

From the parallel of 55° N. lat., at all events to the equator, we have on either side of the Atlantic a depression 600 to 700 miles in width, averaging 15,000 feet in depth.

These two valleys are separated by the modern volcanic plateau of the Azores. I cannot think it at all probable that any general oscillations have taken place in the northern hemisphere since the commencement of the Tertiary period sufficient to form that immense abyss, or, if formed, to convert it into dry land; but on this point I am able to quote the highest authority:—"If at any former period the climate of the globe was much warmer or colder than it is now, it would have a tendency to retain that higher or lower temperature for a succession of geological epochs. . . . The slowness of climatal change here alluded to, would arise from the great depth of the sea as compared to the height of the land, and the consequent lapse of time required to alter the position of continents and great oceanic basins. . . . The mean height of the land is only 1,000 feet, the depth of the sea

15,000 feet. The effect, therefore, of vertical movements equally 15,000 feet in both directions, upward and downward, is to cause a vast transposition of land and sea in those areas which are now continental, and adjoining to which there is much sea not exceeding 1,000 feet in depth. But movements of equal amount would have no tendency to produce a sensible alteration in the Atlantic or Pacific Oceans, or to cause the oceanic and continental areas to change places. Depressions of 1,000 feet would submerge large areas of the existing land, but fifteen times as much movement would be required to convert such land into an ocean of average depth, or to cause an ocean three miles deep to replace any one of the existing continents." (Lyell, "Principles of Geology," 1867, pp. 265-6.) The wide extent of Tertiaries in Europe and the north of Africa sufficiently proves that much land has been gained in Tertiary and post-Tertiary times, and the great mountain masses of Southern Europe give evidence of great local disturbance. But although the Alps and the Pyrenæes are of sufficient magnitude to make a deep impression upon the senses of men, taking them together, their materials would, it spread out, only cover the surface of the North Atlantic to the depth of about six feet, and it would take at least 2,500 times as much to fill up its bed. It would seem by no means improbable that while the edges of what we may call the great Atlantic depression have been gradually raised, the central portions may have acquired an equivalent slight increase in depth; but it is most unlikely that while the main features of the contour of the northern hemisphere remained the same, an area of so vast extent should have been depressed by more than the height of Mont Blanc. On these physical grounds alone I should be inclined to believe that a considerable portion of this area has been continuously under water, and that consequently a deposit has been forming uninterruptedly from the period of the chalk to our own.

I would now refer to the palæontological bearings of the question. Sir Charles Lyell says (p. 263), "The reader should be reminded that in geology we have been in the habit of founding our great chronological divisions, not on foraminifera and sponges, nor even on echinoderms and corals, but on the remains of the most highly organised beings available to us, such as mollusca. . . . In dealing with the mollusca, it is those of the highest or most specialised organisation which afford us the best characters in proportion as their vertical range is the most limited. Thus the cephalopoda are the most valuable, as having a more restricted range in time than the gasteropoda, and these again are more characteristic of the particular stratigraphical sub-divisions than are the lamellibranchiate bivalves, while these last again are more serviceable in classification than the brachiopoda, a still lower class of shell-fish, which are the most enduring of all." With great deference to Sir Charles Lyell, I cannot regard the most highly specialised animal groups as those most fitted to gauge the limits of great chronological divisions, though I admit their infinite value in determining the minor sub-divisions.

The culmination of such animal groups, such as we find in the marvellous abundance and variety of both orders of cephalopods at the end of the Jurassic and the commencement of the Cretaceous period, undoubtedly brings into high relief, and admirably illustrates to the student, the broad distinctive characters of the Mesozoic fauna; but speaking very generally, the more highly a mollusc is specialised, the shallower is the water which it inhabits. The cephalopods are chiefly pelagic and surface things, and their remains are consequently found in deposits from all depths. The gasteropods, with comparatively few exceptions, range from the shore to 1 to 200 fathoms, and lamellibranchs become scarce at a slightly greater depth; while some orders of crustacea, brachiopods, echinoderms, sponges and foraminifera, descend in scarcely diminished numbers to a depth of 10,000 feet. In fact, the bathymetrical range of the various groups in modern seas corresponds remarkably with their vertical range in ancient strata.

A change in the distribution of sea and land involving a mere change in the course of an ocean current, might modify the conditions of an area for all cephalopods, pteropods, heteropods, and other surface living animals of high type, even to their extinction. By oscillations of 500 feet up and down, the great mass of gasteropods and all reef-building corals, would be forced to emigrate, become modified, or destroyed, and another hundred fathoms would exterminate the greater number of bivalves; while elevations and depressions to ten times that amount might only slightly affect the region of brachiopods, echinoderms, and sponges.

In the late deep-sea dredgings by M. de Pourtales, off the American coast, and by H.M.S. *Lightning* and *Porcupine*, and

Dr. Marshall Hall's yacht *Norna* off the west coast of Europe, no animal forms have been discovered, so far as I am aware, identical with chalk fossils. Additional evidence has, however, been procured that over a large part of the bottom of the Atlantic a deposit is being formed mainly of disintegrated globigerinæ and other foraminifera and coccoliths, which appears to be undistinguishable from the ancient chalk.

Not fewer than twenty genera of vitreous sponges have been dredged belonging to two groups, the Hexactinellidæ and the Lithistidæ of Oscar Schmidt, both of which groups are highly characteristic of the chalk and greensand. These sponges are in vast numbers, like the ventriculites and their allies in older Cretaceous beds; and they, with other silicious organisms equally abundant in the modern chalk area, seem to be capable of supplying that amount of silica in a fine state of division which might explain the production of chalk flints. A large series of echinoderms were found, recalling to a remarkable degree, from the profusion of Cidarids and of star-fishes of such genera as *Archaster*, *Astrogonium*, and *Stellaster*, the general facies of the chalk echinoderm fauna; and besides this general resemblance, members of several families have been recovered which were supposed to be extinct. *Salenocidaridaris varispina* A. Ag., dredged by De Pourtales in the Strait of Florida, is a living Salenia; *Echinolampas caratomoides* A. Ag. perpetuates some of the most marked characters of the Galeritidæ. *Pourtalesia*, a genus first found by Count Pourtales and afterwards in the *Porcupine* expedition, is a true Dysaster. *Porocidaridaris purpuratus*, a fine species dredged off the Butt of the Lewis, represents a genus hitherto known only by some isolated plates and radioles. Two very remarkable generic forms, dredged from the *Porcupine* off the coasts of Scotland and Portugal, only known from some fragments in the English white chalk, found a new family near the Diademidæ. Some new ophiurids approach their fossil ancestors; and off the coast of Portugal the dredge brought up at one rich haul twenty or thirty examples of a fine *Pentacrinus*; while over the whole area *Rhizocrinus loffotensis*, a degenerate little Bourgueticrinus, seemingly one of the last of the pear-encrinites, is abundant.

I am not in a position to say much about those groups which I have not personally examined, except that Mr. Gwyn Jeffreys and Prof. Martin Duncan report that among the mollusca and corals many species occur which have been hitherto known only as fossils, principally, as might have been expected, in comparatively shallow water forms in the Tertiaries.

I do not see that there is any object in attempting to explain this singular resemblance between these deep-sea deposits in the Atlantic and the old chalk in composition and structure and in embedded fauna on any other assumption than that of a continuity of conditions over some part at all events of the area. During the lapse of time, while the fauna of shallower water has again and again undergone almost total change by changes in the distribution of temperature and in the distribution of sea and land, the fauna of the deep water has been also affected. To a depth of 5,000 feet it is at present heated over a large portion of the North Atlantic many degrees above its normal temperature. Accepting, as I believe we are now bound to do, some form of the doctrine of the gradual alteration of species through natural causes, one is quite prepared to expect a total absence of the identical forms found in the old chalk. The utmost which might be anticipated is such a resemblance between the two faunæ as might justify the opinion that the later fauna bears to the earlier the relation of descent with extreme modification. Sir Charles Lyell asks if we have dredged belemnites, ammonites, baculites, hamites, turrilites, &c.; that question is, I think, best answered by the record of the old Cretaceous beds themselves, which are scarcely more remarkable for the presence of these singular and beautiful forms than for their rapid extinction. According to the view which I have felt myself compelled to adopt, the various groups of fossils characterising the Tertiary beds of Europe and North America represent the constantly altering fauna of the shallower portions of an ocean whose depths are still occupied by a deposit which has been accumulating continuously from the period of the pre-Tertiary chalk, and which perpetuates with much modification the pre-Tertiary chalk fauna. I do not see how this view militates in the least against the "reasoning and classification" of that geology which we have learned from Sir Charles Lyell. Our dredgings only show that these abysses of the ocean which Sir Charles Lyell admits in the passage quoted above to have outlasted on account of their depth a succession of geological epochs, are inhabited by a special deep-sea fauna

possibly as persistent in its general features as are the abysses themselves.

WYVILLE THOMSON

Ocean Currents

ATTENTION has been much drawn of late to the subject of Ocean Currents and their causes, and it has occurred to me that there is a directing if not an originating cause of these streams, which has, so far as I am aware, been overlooked by physicists. It is known* that at some parts of the earth's surface there exists an atmospheric pressure capable of sustaining a column of mercury in the barometer of upwards of 30 inches in height; at the same time there are certain areas over which this pressure is only such as to raise the barometric column to a little over 29 inches. Now if we compare the difference of absolute weight sustained by two such areas, we shall see that in the space over which the higher atmospheric pressure exists, there is an excess of weight of air, amounting in round numbers to 1,000,000 of tons on each square mile. Applying this fact to the region of the ocean in which the surface currents are best known, the North Atlantic, we find from the isobaric chart that there is throughout the year over a large portion of the eastern side of this sea, next the coast of North Africa, a pressure (to use the convenient mode of expressing it) of upwards of 30.2 inches. To westward of this space, towards the Gulf of Mexico and the coast of the United States, the average pressure decreases; between Newfoundland and the British Isles the pressure is still diminished, till in the wide channel between Iceland, Norway, and Spitzbergen, we arrive at a yearly pressure of less than 29.6 inches. It is reasonable to believe that the waters which lie under the high pressure area have a tendency to escape from under the excessive weight, towards the space over which the pressure is less. But the high pressure area next the African coast is precisely that upon which the north-east trade winds descend, and the waters, aided in their choice of an exit, will naturally flow off to south-westward before the wind. Their continuance in this direction is barred, however; for across the whole of the southward passage between Africa and South America, there exists another belt of high pressure, out of which the south-easterly trade winds blow. The only course left for the escaping waters (allowing for the moment that the excess of pressure is a cause of their movement) is to westward, where the pressure is lessened, towards the Gulf of Mexico, and the east coast of America, and thence towards the low pressure space between Iceland and Norway. But this is exactly the course that the Gulf Stream, or rather the North Atlantic warm stream of which the Gulf Stream is the most prominent feature, is seen to take. Are we not then warranted in concluding that the difference of atmospheric pressure has some power both in originating and in directing the course of this ocean current?

In suggesting the unequal distribution of atmospheric pressure as a supplementary cause to difference of temperature and of density, to evaporation, rain, and winds, and to whatever further agents there may be in the production of ocean currents, I would venture to express a hope that some one in authority, by carefully comparing and valuing the power of each one of these motive forces, and their application to the known streams, will give to the world a system of the causes of ocean currents which will be vastly more relevant to the phenomena these streams are known to present, than any one of the theories which have as yet been put forth.

When we know that Sir John Herschel gives to the winds the entire right of setting the ocean streams in motion; that Captain Maury holds the universal circulation of the sea to be caused by nothing else than the differences in its specific gravity, and that Dr. Carpenter (or rather Professor Buff) would bring about a general interchange of polar and equatorial water by the aid of sunshine and frost alone; is it not time to ask which of these three causes we should accept as the true one, or if all three are partially concerned, what part is to be taken from each to let the others have their fair share in the work?

KEITH JOHNSTON, Jun.

The Measurement of Mass

I AM happy to meet with an opponent who comes so directly to the point as my Reviewer, W. M. W.

* I would refer those who desire to look more particularly into this matter to the monthly isobaric charts prepared by Mr. Buchan, to illustrate his admirable paper on the mean pressure of the atmosphere. *Trans. Royal Soc. Edin.*, vol. xxv.