SCIENCE.—SUPPLEMENT.

FRIDAY, MAY 7, 1886.

IS THE OCEAN SURFACE DEPRESSED?

I.

THE *Revue scientifique* published recently the following discussion on the communication made in January at the Sorbonne by H. Faye, upon the permanence of the earth's figure throughout geologic times. The eminent academician then affirmed that accord exists among geodesists as to the figure of our planet; that the measures of arcs of meridians already made have done away with all irregularities, which at the beginning of this century were supposed to exist; and that one can assign for the form of the surface of the sea an ellipsoid of revolution, having an eccentricity of 1: 292 (accurate to one unit in the denominator).

I do not feel able to say how the assertions of M. Faye can be reconciled with the diametrically opposite ideas which have been developed in recent German works, noticeably in the 'Lehrbuch der geophysik' of Günther, the works published in 1868 by P. H. Fischer, in 1873 and 1877 by Listing, and, above all, in the important memoir which Bruns published at Berlin in 1876; which last is not even mentioned by the learned French astronomer. I can only call attention to his estimation of their value, without being able to judge of the reasons which have determined it. I must leave this to the geodesists.

I would say the same thing of another assertion of M. Faye, — that relative to the constancy of the force of gravitation at the surface of the sea along the same parallel. "Navigators," says he, "have carried the pendulum at the surface of the sea over a large portion of the earth, and in both hemispheres, without the pendulum indicating the least diminution of the force of gravity ascribable to depression of the earth's crust." Now, Fischer, as well as Hann, states, that, upon the islands situated in the open ocean, the pendulum, when swung at the surface of the sea, executes at least nine and one-third more oscillations than upon the shores of the large continents. This, at the rate of one hundred and twenty metres for one oscillation, gives more than a thousand metres for the depression of the sea at the centre of the oceans; and this same conclusion is elaborated also by Listing as well as by Pinck. So startling is this disagreement, that we acknowledge that it is almost beyond credence; and, as attention has been called to it, proper experiments should be undertaken to clear away all doubts.

But, even if we admit the correctness of the data given by M. Faye, there is one point in his theory which we cannot pass over, because it touches the constitution of the earth's crust. The eminent academician reasons somewhat in this way: at any point over the sea, the density of the water being sensibly inferior to that of rocks, there should be a local diminution of the attracting mass, and consequently the pendulum ought to oscillate less rapidly. Since this is not the result, there must be some cause counteracting the diminution of the superficial mass. This cause, according to M. Faye, can only be an increase of the density of the crust. As the solid rocks have in general a density greater than that of the molten materials from which they are obtained, and if under the sea the solidification has progressed farther than under the continents, the increase of the solid mass under the seas could compensate the diminution of density resulting from the column of sea-water above. But to this conclusion I am not ready to assent.

If it be true that a majority of bodies are more dense in the solid condition than in the liquid, it is also true that we know very little of the physical condition of the interior of the earth. Even in our day many *savants* hold that the earth is entirely solid. But, admitting the existence of a liquid interior covered by a solid crust, how can we assert that this crust, traversed by numerous crevasses, does not contain sufficient open spaces to annul the slight increase of density due to solidification.

Let us accept Faye's hypothesis for the time being, and search with him for the cause which has produced this increase of solidification. We know, from the submarine investigations of the last few years, that everywhere on the bottom of the large oceans there reigns a temperature in the neighborhood of 0° C. The cause of this is to-day well known. The water of the polar regions, rendered denser by cooling, sinks, and, following the bottom of the sea, tends to replace the water evaporated in the tropical regions. M. Faye says this cause for the cooling of the bed of the oceans has existed ever since there have been ice-caps at the poles, and that it is impossible that such an action, prolonged through a sufficiently long period, should not have affected the temperature of the earth's crust beneath. This is the principle of his hypothesis, but it is not sufficient to announce it. It is also necessary to justify it in proving that the cause is adequate to the effect. This it is that M. Faye has neglected to do; and I would add, that, in my conviction, such a proof is impossible.

But, before attempting to show this, it would be well, perhaps, to call attention to one singular consequence, which is entailed if it is necessary to admit the theory of the cooling of the earth's crust by contact. No one is ignorant of the fact, that, if the temperature of the bottom of the sea is in the neighborhood of 0°, there are on the surface of the continents wide stretches of country which are still less favored. Without speaking of mountainous regions covered with perpetual snow, we will only mention the plains of Siberia, and especially that of the district of Yakootsk, where there reigns a mean temperature of — 10°C. This temperature, as may be readily seen, was established at the same time as the ice-caps around the poles, and has tended to produce a change of temperature of the crust for a time at least as long as that during which the cold waters have flowed over the ocean-bottoms: consequently, as the earth's surface affected by this cooling is far from being negligible, it is there that the pendulum ought to oscillate the most rapidly.

But, aside from this argument from the facts in the case, there are other strong reasons deduced from what we know of the bad conductive power of rocks. Experiments at Paris have shown that a change in the mean monthly temperature propagates itself in thirty-eight days to the depth of one metre, and that at ten metres below the surface all variation in the temperature of the air becomes absolutely insensible. This being the case, one would think that a cooling coming from the surface could hardly exercise any effect on the inside of the crust of the earth.

To argue the possibility of such a cooling effect, it would be necessary first to have some idea of the probable thickness of the crust. Whatever hypothesis we accept as to the interior constitution of the earth, it is inadmissible that, at the time when the glaciers took possession of the poles, the thickness of the solid crust had not reached at least twenty kilometres. Fossil botany teaches us that in the middle of the tertiary period the regions immediately around the poles possessed a rich vegetation of a character essentially temperate, which certainly could not have existed in the neighborhood of ice. The first appearance of polar ice was therefore not in the carboniferous period, when we know, moreover, that the arctic seas were inhabited by corals like those which now live only in the tropics. This granted, if we take account of the generally given thickness of the gneiss and micaschists, by all estimated at many thousands of metres; if we add to this the Cambrian, Silurian, Devonian, and carboniferous deposits, even attributing to them only a small part of the depth which they have in Europe, — we find that a total thickness of twenty thousand metres for the crust constitutes certainly a moderate valuation.

Let us suppose, then, a crust of twenty thousand metres, of which the temperature, about 2000° C. on its lower side, decreases regularly up to the surface, where it is about 20° C., — the minimum of tropical regions, — or a diminution of one degree for ten metres. Can we imagine a difference of twenty degrees in the surface temperature could have produced an appreciable difference in the interior even after millions of years?

Let us consider more closely in what way the distribution of temperature exists in the interior of the earth. We know that this temperature increases constantly with the depth. But it has long been granted that the flow of heat does not contribute to the exterior temperature more than the thirtieth of a degree. Let us reverse the calculation, and ask how far a temperature of 0° C. could contribute to the diminution of heat which reigns at twenty kilometres depth. Cannot the answer be made without discussion?

But we have the reply expressed in figures in the results of some investigations in Siberia. In 1836 a merchant of Yakootsk, wishing to utilize the internal heat, dug a well in the hope of reaching water. In this well, dug to a depth of 115 metres, the temperature increased progressively from — 10° C. to 0° C.

The well was abandoned because such a great depth rendered it useless for the purpose proposed; but a little later, in the steppes of Katchongin, another well reached water at a depth of 126 metres. Therefore, below the constantly frozen surface of Siberia, the temperature rises in 126 metres at least ten degrees to 0° C. The increase is thus one degree for twelve metres and a half; that is to say, three times more rapidly than in the temperate regions, where it is one degree for from thirty-five to thirty-seven metres.

What, therefore, is to be concluded? Even that a great superficial cold only affects the layers immediately in the neighborhood of the surface, and that this influence at any considerable depth must become absolutely insignificant. If, then, the force of gravitation is not diminished above the oceans; if at the same time, on the authority of all others with the exception of M. Faye, there exists a sensible increase, — it is not to an increase in the density of the crust that this result can be attributed. The only admissible explanation is a diminished distance to the attracting centre, and consequently a deformation of the ellipsoidal surface of the sea.

Hence I express my desire for further measures of great circles, following the suggestions of Bruns, the astronomical and geodetic observations to be combined with the most precise levellings and with measures of the force of gravity. Then only could the question be decided in a definite manner. Up to that time it is premature to wish to attack it, either by hypothesis in discord with the laws of science, or in passing over in silence the work, which, true or false, merits at least a respectful examination. A. DE LAPPARENT.

II.

M. DE LAPPARENT'S high authority as a geologist renders it my duty to give certain explanations in support of the partly geological theory which I have recently presented.

First, as regards the figure of the earth, it is not a question of authority taken second or third hand. The measurements of arcs of meridians are well known ; and the calculation which permits us to conclude from these measurements the figure of the earth is very simple, and may be verified by any one.

The surface of the earth conforms so well in all parts with an ellipsoid of revolution, that the deviations are absolutely unappreciable, save by the most delicate measurements.

As regards the pendulum, with which the most recent measurements have been made by Mr. Clark of England, and in the United States by Mr. Peirce, the results are no less striking. These two reach by the same method of observation, wholly independent of the measurements of arc, and by calculations easily verified, the same flattening, 1: 292.

It is very true, as M. de Lapparent has remarked, that, among the numerous observations made in all parts of the earth, those which have been made on the small isolated islands in the middle of the ocean have indicated a force of gravity a little too strong ; but these slight anomalies do not vitiate the general result, that is to say, the value of flattening above given.

This fact has been known for seventy years, but it has been wrongly interpreted. Some have laid the blame upon the observers. Others have said, that, as the islands were of volcanic origin, the materials composing them have a greater density, which would account for the excess of local attraction. Others, fifty years ago, have said, what M. de Lapparent repeats to-day, that, if the force of gravity is a little greater on the islands, it is because the surface of the sea is nearer the centre of the earth.

The true interpretation is less pretentious, and does not contradict assured scientific facts. It is simply that it has been forgotten to take into account the excess of attraction of the submerged mountains, at the summit of which observations were made, over the attraction of an equal volume of water, which it replaces in the middle of the sea. Unfortunately the navigators have not thought to determine by suitable soundings the form of the submarine pedestal on which their instrument was placed, so that it is impossible to-day to apply the necessary corrections to their results.

Finally, the chief argument of my opponent is the poor conductibility of the rocks which compose the earth's crust. I will say first, that, despite this feeble conductibility, the earth has become sufficiently cool in the course of the geologic ages to have acquired a solid crust of from thirty thousand to forty thousand metres in thickness. It follows, then, that the central heat traverses this thick crust, notwithstanding its slight conducting-power, to finally lose itself by radiation in space. I am unable to see that this undoubted cooling operates everywhere under the same conditions. Leave aside the argument of Siberia, and consider a spherical surface a league or a league and a half below the surface of the earth. At this depth it is necessary to distinguish two regions, — one situated beneath the continents, and the other found in the depths of the ocean. The central heat which arrives at this surface in the first region must still traverse an enormous bed of rock before it can radiate into space. Precisely on account of the slight conductibility of this highly protecting thickness of rock, very little heat passes ; and there beneath our feet, at this depth, the central heat makes itself strongly felt, the temperature rising to more than 200° C. In the other region — the submarine region — the case is different. There the superincumbent bed, of a league and a half in thickness, is water ; but water is an excellent transporter of heat when received from the bottom, the water carrying the heat upward, not by conduction so much as by the ascending currents, to which the least accession of heat gives rise. Thus the central heat passes easily in such a region. Moreover, the continual flowing-in of polar water at a temperature of — 1° or — 2° aids the refrigeration.

It therefore seems to me evident that the cooling of the central mass is facilitated by the sea, and obstructed by the continents. Is it necessary to add that the waters of the ocean, under a pressure of from four hundred to six hundred atmospheres, penetrate deeply into the solid beds upon which the ocean rests, and render these beds more permeable to the heat? It is reasonable, and in no wise contrary to the laws of physics, to conclude that the cooling of our globe, elsewhere excessively slow, has progressed more rapidly and more deeply under the seas than under the continents. This difference has existed for many million years, and ought to have caused in that extent of time a notable variation of thickness in the solid crust. H. FAYE.

BACTERIA AND DISEASE.

DR. GEORGE M. STERNBERG, U.S.A., so well known as a writer and investigator in bacteriology, delivered a lecture before the Alumni association of the Long Island college hospital, Brooklyn, on the evening of April 20. The subject upon which he was requested to address the association was, "A general review of the relation of bacteria to disease, including an account of a personal observation of Pasteur's methods in the prevention of hydrophobia, and their results."

The lecturer called attention to the frequent references of late to the labors of Pasteur in his inoculations for hydrophobia. While some of these willingly accorded to Pasteur all the honor he deserved, there were others which criticised adversely not only his methods, but even his professional reputation, charging him with acting the charlatan in keeping his methods secret. It is true that Pasteur has not proclaimed his experiments abroad in all their details; but this is not because he desired to keep them secret, but because he wished to satisfy himself that his methods were right before he encouraged others to undertake them. In this respect he has done what every scientific man would do. He has, however, always been ready to explain to those whom he regarded as competent his method, and even to demonstrate it to them.

The basis of Pasteur's method depending on increase in the virulence of the virus by transmission through a number of rabbits, and its use in gradually increasing potency in inoculation, has already been described in *Science;* and his system of protecting inoculation is too well known to call for further mention at this place.

Before Pasteur inoculated any human beings, he had tested his method upon fifty dogs, and had in every case rendered them immune, that is, insusceptible to hydrophobia. The history of the first person inoculated, Joseph Meister, is too well known to need repetition here. Since this time (July, 1885), Pasteur has inoculated three hundred and fifty persons. Of course, Pasteur knows as well as any of his adverse critics that all these persons were not bitten by rabid dogs, but he could not refuse to inoculate them. With the exception of the Russians who have recently died, Pasteur has had but one unsuccessful result. In these cases the explanation is probably to be found in the fact that the inoculation was practised too late. It is just so in vaccination, which is recognized as a preventive of small-pox. If we can vaccinate in time, we may abort an attack of small-pox which would otherwise occur; while, if our vaccination is done at the close of the incubatory stage of the small-pox, it will be of no avail.

Dr. Sternberg read a translation of Pasteur's last communication to the French academy, published in the *Comptes rendus* of March 1. In this paper Pasteur gives the results of his inoculations, showing indubitably that the individuals operated upon had in most instances been bitten by rabid animals. These persons had come to him with certificates from medical men and veterinarians, showing this fact beyond a doubt. In speaking of his one apparent failure, Pasteur says that the child was not brought to him until thirty-seven days after the bite was received, and that the wounds in the axilla and the head were in themselves most serious, and that but for the sake of humanity he would have refused to treat the child for the hydrophobia.

Pasteur gives it as his opinion that one death from hydrophobia occurs in every six persons bitten, and that the disease is most apt to occur within forty or sixty days. Of the persons treated by him, one hundred were bitten more than seventy-five days before the publication of his communication, and were still well; another hundred had passed for six weeks to two months; and the others were still well, and time only could tell what would be the result in their cases.

In concluding his remarks upon hydrophobia and the methods of Pasteur, Dr. Sternberg said that the only criticism which suggests itself with reference to this interesting statement of facts is that Pasteur does not attach as much importance to the prophylactic value of early and thorough cauterization as this measure seems entitled to. The considerable number of cases in which cauterization was practised may have had a greater influence upon the favorable result in the extended series of cases reported than Pasteur has been willing to admit. At all events, it will be well to withhold our final judgment as to the value of the method as applied to man until the three hundred and fifty cases reported are all beyond